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THE SIGNIFICANCE OF THE AUDITORY ELECTRICAL EFFECTS FOR AUDITORY THEORY

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In a recent paper,[1] a survey was made of the various investigations concerned with the electrical effects produced in the neighborhood of the auditory apparatus by sound stimuli. Its purpose was to determine whether any of the investigations provided conclusive evidence that the electrical effects reported were due to auditory action currents, and to determine the extent to which the conditions and problems of the various investigations were comparable. The aim of the present paper is to discuss the significance which these electrical effects may have for auditory theory, particularly in the light of new evidence that has appeared in recent papers.

The theories upon which the auditory electrical effects might be expected to have the most direct bearing are the frequency theories. The term "frequency theory" is used here to refer to any of the theories which assume that the entire range of audible frequencies may be retained in the response of the nerve. It was supposed at first that this type of hypothesis required the assumption for the fibers of the auditory nerve of a refractory period considerably less than that known to occur in other mammalian nerves. Thus, a given fiber, to be capable of responding at frequencies as high as 20,000 a second, must have an absolute refractory period not greater than 1/20,000 of a sec. But from measurements in other nerves, it did not seem likely that any fiber could respond steadily at frequencies much greater than 1000 a sec.[2] Troland,[3] however, was able to avoid this diffi-

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[1] G. Kreezer, A critical examination of the investigations of auditory action currents, this JOURNAL, 44, 1932, 638-676.

[2] Cf. H. Rosenberg and K. Kitayama, Untersuchungen über Nervenaktionsströme, *Arch. f. d. ges. Physiol.*, 223, 1930, 723.

[3] T. Troland, The psychophysiology of auditory qualities and attributes, *J. Gen. Psychol.*, 2, 1929, 43 f.

culty by suggesting a type of theory which has been designated by some writers as the volley theory. Troland pointed out that even though no fiber of the auditory nerve responded at frequencies greater than 1000 per sec., the nerve as a whole might show frequencies much greater if it were assumed that *different* fibers discharged in response to immediately successive periods of the stimulating wave. A basis for such behavior would be provided, according to him, if differences in excitability occurred among the fibers terminating in the region of the basilar membrane under vibration. Thus, granted that the basilar membrane vibrates at the frequency of the stimulus, the following behavior might be expected for the frequencies above 1000. Corresponding to the first cycle of the wave, only the hair cells of maximum excitability would be set off; but they would not be able to respond at the next cycle on account of still being in the absolute refractory state. The cells of next higher threshold, which had not been set off at the first cycle, might, however, respond at the second cycle, because of the summation in them of the excitation process due to both the first and second cycles of the stimulating wave. Thus the process might continue until the fibers that have responded first have recovered from their refractory condition and are able to respond again. Troland's hypothesis thus supplied a way to avoid the difficulties raised by the question of the absolute refractory period. This type of theory, although it made the frequency doctrine more plausible, did not, of course, show that it was true.

The term "volley theory" may be somewhat misleading as a name for this type of hypothesis. In investigations of motor nerves, the term "volley" has been used to designate an aggregation of impulses fired from a group of fibers in response to a single stimulus.[4] The term could be applied to each of a series of such aggregates or groups of impulses whether the successive aggregates were fired by the same set of fibers or different sets. The distinctive feature of Troland's and other volley theories, on the other hand, is not so much that volleys occur in the nerve response, but that the immediately successive volleys fired by the nerve at the higher frequencies are provided by *different* sets of fibers. The volleys from different sets of fibers are thus staggered in time so that a new set discharges for each immediately successive period of the stimulating wave, up to a certain point.[5] Such staggering is necessary if, as the volley theories assume, the nerve as a whole is to transmit the entire range of audible frequencies without exceeding the frequency limits considered probable in the individual fibers.

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[4] R. S. Creed, D. Denny-Brown, J. C. Eccles, E. G. T. Liddell and C. S. Sherrington, *Reflex Activity of the Spinal Cord*, 1932, 3.

[5] Troland, as we have seen, assumes differences in the excitability of different fibers to account for the staggering feature of his volley theory. Wever and Bray suggest, instead, differences in refractory period of the fibers to account for the staggering. (E. G. Wever and C. W. Bray, Present possibilities for auditory theory, *Psychol. Rev.*, 37, 1930, 376 f.)

It would seem that the question of whether the higher audible frequencies can be transmitted by the auditory nerve should be capable of direct answer by experiments designed to detect the action potentials which occur in this nerve in response to sound waves of various frequencies. The earlier attempts for detecting the action potentials of the eighth nerve were made, however, with instruments incapable of responding at the higher frequencies.[6] It occurred to Wever and Bray to repeat this type of experiment, but with an amplifier and telephone as the indicator of the characteristics of any electrical effects that might be produced instead of the more slowly responding string galvanometer.[7] When they did so, they found that the electrical currents picked up by an electrode on the eighth nerve provided telephone tones of the pitch of the stimuli up to frequencies as high as 5200 cycles. The checks they performed to rule out the possibility of artifacts satisfied them that the effect was due to action currents in the auditory nerve. Their results were therefore interpreted as lending support to a frequency type of auditory theory.[8] Experiments of a similar sort were subsequently performed by a number of other investigators. The results of these investigations have been reviewed in the paper of which mention has already been made, and the conclusion drawn that none of the investigations considered provided conclusive evidence that the electrical effects reported were due to auditory action currents.[9]

Before proceeding now to a consideration of the present status of the question, it is necessary to consider the properties of the different electrical effects so far reported and to specify the terms to be used in designating them. Davis and Saul,[10] in recent papers, have emphasized the fact that two different physiological effects may be produced upon stimulation of the ear by sound, and have summarized the properties of these two effects as determined in their investigations. The one effect they refer to as "true action currents," the other as electrical or "cochlear spread." The properties with respect to which the two effects are described are (1) the range of frequencies of the effect as a function of the frequency of the stimulus, (2) the form characteristics of the effect, (3) the velocity of propagation, or the latency between the arrival of sound waves in the cochlea and the appearance of the electrical effect in some particular tissue, (4) the place of occurrence of the effect, (5) the influence of general and local narcotics, and (6) the influence of death.

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[6] F. J. Buytendijk, On the negative variation of the nervus acusticus caused by a sound, *K. Akad-Wetensch., Amsterdam, Proc. Sci. Sect.*, Eng. trans., 13, 1911, 649-652; A. Forbes, R. H. Miller, and J. O'Connor, Electrical responses to acoustic stimuli in the decerebrate animal, *Amer. J. Physiol.*, 80, 1927, 363-380.

[7] E. G. Wever and C. W. Bray, The nature of acoustic response, *J. Exper. Psychol.*, 13, 1930, 373-387.

[8] Wever and Bray, Present possibilities for auditory theory, *Psychol. Rev.*, 37, 1930, 374-380.

[9] G. Kreezer, *op. cit.*, 674-676.

[10] L. J. Saul and H. Davis, Electrical phenomena of the auditory mechanism, *Trans. Amer. Otol. Soc.*, 22, 1932, 137-142; Action currents in the central nervous system, I. Action currents of the auditory tracts, *Arch. Neurol. and Psychiat.*, 28, 1932, 1104-1116; Auditory action currents, this JOURNAL, 45, 1933, 358-359.

In terms of these properties, "cochlear spread" is designated by Davis and Saul:[11] (1) as reproducing the frequency of the stimulating sound up to at least 5000 cycles; (2) as reproducing the wave form of the stimulus above 800 per sec., although at lower pitches the response may be more complex (observations made by means of the cathode ray oscillograph); (3) as not restricted to the auditory tracts but broadcasting throughout all the tissues of the head; (4) as showing a latency in its appearance anywhere in the head, after the arrival of sound waves in the cochlea, of less than 0.1 σ; (5) as being very resistant to narcosis; and (6) as persisting (although with reduced intensity) many minutes after death.

The descriptions which the other investigators have given of the effects reported by them show different degrees of completeness with respect to the various properties so systematically examined by Davis and Saul. Nevertheless, when we examine the properties which *are* reported by the other investigators, we find that they are like those specified by Davis and Saul for cochlear spread. Thus, with respect to frequency, Wever and Bray[12] report a correspondence in frequency of stimulus and electrical effect up to frequencies of about 5000 (5200) cycles, and state that speech can be readily transmitted. Adrian,[13] Hughson and Crowe,[14] and Rademaker and Bergansius[15] report that they confirm this finding. Wever and Bray, and Adrian report that the effect is resistant to narcotics injected into the nerve tissues,[16] and that it persists for some time after the death of the animal.[17] Wever and Bray, Adrian, and Rademaker and Bergansius all indicate that the effect detected reproduces the wave form of the stimulus.[18]

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[11] Saul and Davis, *op. cit.*, this JOURNAL, 45, 1933, 358 f.

[12] Wever and Bray, *op. cit.*, *J. Exper. Psychol.*, 13, 1930, 377.

[13] E. D. Adrian, The microphonic action of the cochlea: an interpretation of Wever and Bray's experiments, *J. Physiol.*, 71, 1931, xxviii-xxix.

[14] W. Hughson and C. J. Crowe, Function of the round window, *J. Amer. Med. Assoc.*, 96, 1931, 2027-2028.

[15] G. G. I. Rademaker and F. L. Bergansius, Expériences sur la physiologie de l'ouie, *Arch. neerl. de physiol.*, 16, 1931, 346-349.

[16] Adrian, *op. cit.*, xxix; Wever and Bray, Action currents in the auditory nerve in response to acoustic stimulation, *Proc. Nat. Acad. Sciences*, 16, 1930, 248.

[17] E. D. Adrian, D. W. Bronk, and G. Phillips, The nervous origin of the Wever and Bray effect, *J. Physiol.*, 73, 1931, 2P-3P; Wever and Bray, *op. cit.*, *J. Exper. Psychol.*, 13, 1930, 382.

[18] Cf. Kreezer, *op. cit.*, 673.

Wever and Bray, and Adrian report that it can be detected in tissues remote from the auditory tracts.[19]

Thus the properties listed by Davis and Saul as characteristic of cochlear spread are substantially like those which have been given as characteristic of the electrical effects reported in the other investigations.[20] Since this effect has come to be known as the "Wever-Bray effect," we shall refer to it by that name instead of as "cochlear spread." This does not imply, however, that it is valid to assume that the effect is due to identical phenomena in the different experiments.[21]

The other effect reported by Davis and Saul is designated by them as "true action currents." The specifications given concerning its properties are in direct contrast with those given for "spread." The true action currents of Davis and Saul are described:[22] (1) as following the frequency of the stimulating waves up to but not over 900 or 1000 per sec.; (2) as appearing on the cathode ray oscillograph as waves of the shapes and time relations characteristic of action currents in peripheral nerves; (3) as being localized in the auditory tracts, and as being more intense in the larger tracts and centers than in the eighth nerve; (4) as traveling with the usual speeds of nerve impulses; (5) as showing the usual sensitivity to local or general anaesthetics; and (6) as disappearing just before or instantly at death. An examination of the reports of the other investigators fails to reveal in any of them an effect with properties like that reported by Davis and Saul for true action currents. Davis and Saul are apparently the only investigators who have isolated this particular effect. We shall therefore refer to it as the "Davis-Saul effect."

THEORETICAL SIGNIFICANCE OF THE ELECTRICAL EFFECTS

We may now consider the significance which these effects may have for certain questions of importance for auditory theory.

Davis-Saul effect: (a) Cause. Certain of the lines of evidence given by Davis and Saul to prove that the Davis-Saul effect is due to action currents were considered in our previous review.[23] The two new lines of evidence presented in their more recent papers relate to the form characteristics of the effect and to its velocity of propagation.

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[19] Adrian, *op. cit.*, xxviii; Wever and Bray, *op. cit.*, *J. Exper. Psychol.*, 13, 1930, 382.

[20] T. H. Bast, et. al. (The electrical currents associated with sound reception by the ear, *Proc. Soc. Exper. Biol. & Med.*, 30, 1933, 638-640) have recently reported observations on the Wever-Bray effect in the dog. The effect here too has properties like those summarized above with respect to frequency, place of occurrence, resistance to narcotics, and persistence after death.

[21] Cf. Kreezer, *op. cit.*, 648 f.

[22] Saul and Davis, *op. cit.*, this JOURNAL, 45, 1933, 359.

[23] Kreezer, *op. cit.*, 664-668.

The discoverers of the Davis-Saul effect report (1) that the effect appears "on the cathode ray oscillograph as waves of shapes and time relations characteristic of action currents in peripheral nerves," and (2) that it travels "with the usual speed of nerve impulses."[24] These findings undoubtedly lend strong support to their conclusion that this effect is due to action currents in the auditory nerve and tracts. Assuming that this conclusion is correct, let us now examine its consequences for the different types of auditory theory, and consider some of the questions which may be asked concerning the detailed nature of the events in the nerve.[25]

(b) Bearing on frequency theories. Frequency theories, it will be recalled, are of two types: (1) the 'telephone theories,' which assume that the entire range of auditory frequencies are transmitted by the individual fibers; and (2) the 'volley theories,' which assume that the higher frequencies appear only in the response of the nerve as a whole, and that the individual fibers are transmitting impulses at lower frequencies.

The report of Davis and Saul that the frequency correspondence of stimulus and total nerve response does not go beyond 1000 cycles per sec., has bearing strictly only on those frequency theories which are concerned with the response in the nerve as a whole. The results of Davis and Saul demonstrate the incorrectness of the hypothesis that the frequency of response of the nerve as a whole can extend throughout the whole range of audible frequencies. The volley theories, which were designed to account for the transmission of the higher frequencies by the nerve as a whole, are thus found to disagree with the facts.

The determination of the frequency limitation for the whole nerve does not, however, indicate decisively whether or not a correspondence obtains between the frequency of the stimulus and the response of the *individual fibers* of the nerve, and through what range this correspondence holds, if it occurs at all. The correlation suggested in the telephone type of theory may be improbable on other grounds, but so far as the existent data on the frequencies and wave-forms exhibited by the Davis-Saul effect goes, it still remains a possibility.

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[24] Saul and Davis, *op. cit.*, this JOURNAL, 359.

[25] In the rest of this paper, the successive waves of the Davis-Saul effect which correspond to the successive cycles of the stimulating waves will be referred to as volleys. For these waves were detected in the nerve as a whole, and so, presumably, are due to the combined effect of action potentials from many coöperating fibers. In our reference to these waves as volleys, no implication is being made concerning the presence or absence of the stagger arrangement specified in the *volley theories.* Cf. page 2, above.

(c) The activity of the individual fibers. An essential step in the determination of the correlations that exist between the attributes of auditory experience and events in the auditory nerve is, naturally, the preliminary determination of the precise nature of the events in the nerve. As we have seen, the data so far reported on the Davis-Saul effect provide a direct answer to the question of the way the frequency of the volleys of the nerve as a whole depends on the frequency of the stimulus. These data do not, however, permit as yet an answer to certain questions concerning the activity and characteristics of the individual fibers of the nerve. This limitation rests on the fact that the record of a composite curve produced by a number of coöperating fibers does not necessarily permit conclusions to be made concerning the part played by the individual fibers.

Some of the factors, for example, on which the characteristics of the curve representing a single volley may depend are: (1) the type of fibers discharging (the form and duration[38] of the action potential may show differences, as does the velocity of propagation, in different kinds of fibers); (2) the number of fibers discharging (variations in this factor might cause variations in the height and form of the composite wave); (3) the temporal order in which the different participating fibers discharge (variations in this factor might cause variations in both the duration and form of the composite wave). It is evident that without special analysis, based on supplementary data, the wave form representing a particular volley might be interpreted in a number of different ways with respect to the part played by the individual fibers.

Let us now examine the character of some of the problems that still remain concerning the properties of the individual fibers of the auditory nerve.

(1) What is the maximum frequency possible in the response of the individual fibers? Is it 1000 impulses a second, less than 1000 or greater than 1000? Any one of these possibilities still exists.

It is not difficult to see that the frequency of response of individual fibers might be either less than or equal to the frequency of the volleys discharged from the nerve as a whole. It may not be quite so apparent that the response-frequency of individual fibers might be higher than the frequency of the volleys. If it were known that the coöperating fibers all fired synchronously, and that therefore the duration of the volleys was not greater than that of the individual impulses, then, it is true, the fibers could not be firing at rates greater than that shown by the volleys of the nerve as a whole. But none of the data so far available on the Davis-Saul effect indicate whether the volleys from the nerve are synchronous or asynchronous.

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[38] Cf. E. A. Blair and J. Erlanger, Comparison of individual axons in the frog, *Proc. Soc. Exper. Biol. & Med.,* 30, 1933, 728-729; also R. S. Lillie, *Protoplasmic and Nervous Action,* 2nd ed., 1932, 325 ff.

Suppose, for example, that the impulses contributing to a volley occurred asynchronously and so as to build up a wave form of the type exhibited by action potentials, and that the duration of the volleys was not less than twice the duration of a single impulse plus the duration of the intervening refractory period of the fibers. In such a case, a given fiber might discharge twice during the interval of time occupied by a given volley, without the betrayal of this fact in the record of the wave-form of the successive volleys. The frequency of discharge of individual fibers might thus be greater than that of the successive volleys from the whole nerve. Some reasons for expecting asynchronous volleys to occur will be discussed below. That frequencies much greater than 1000 can occur in the individual fibers may seem improbable on the basis of the magnitude of the absolute refractory periods known in other nerves. Such behavior is not, however, as yet excluded by Davis and Saul's results. It remains for further research to decide the question.

(2) What is the relation between the frequencies in the individual coöperating fibers and the frequency of the volleys from the nerve as a whole? This question is closely related to the one above, but is not identical with it. Thus, a given fiber might be capable of responding with a *maximum* frequency of 1000 impulses per sec., but might not do so except at maximum intensities of stimulus, if frequency of discharge is a function of intensity of stimulus. In cases where the frequencies in all of the coöperating fibers *are* less than the frequency of the volleys from the whole nerve, then a *stagger* arrangement such as suggested in the volley theories would seem to be necessary.

(3) Upon what characteristic of the stimulating sound wave does the frequency of response in the individual fiber depend? Does it depend on the frequency of the stimulus, as suggested in the telephone theories, or does it depend on the intensity of the stimulus, as found by Adrian in other nerves? Either of these relations seems to be possible on the basis of the existent data.

(4) What type or types of fiber are present in the auditory nerve? Gasser, Erlanger, and their co-workers, it will be recalled, have found marked differences in the velocity of propagation of action potentials in different fibers, and they have shown that different fibers may be classified as of different types on the basis of such differences. Thus, they distinguish between the A, B, and C types, and within the A group, the α, β and γ types.[27]

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[27] J. Gasser and H. S. Erlanger, The compound nature of the action current of nerve as disclosed by the cathode ray oscillograph, *Amer. J. Physiol.*, 70, 1924, 624-666; The rôle played by the sizes of the constituent fibers of a nerve trunk in determining the form of its action potential wave, *Amer. J. Physiol.*, 80, 1927, 522-547;

It would be of interest to determine what type or types of fiber occur in the auditory nerve, in terms of this classification. This question should prove answerable by means of procedures like those used by Gasser, Erlanger and Bishop in other nerves. Davis and Saul's specification concerning the velocity of conduction is not yet specific enough to answer this question. These authors state only that the velocity of the action currents is like that found in other nerves;[28] the quantitative data given are in terms of the differences in time of arrival of the Davis-Saul effect at different points in the auditory tracts, without specification of the distances involved.[29]

(5) Asynchronism of the volleys. What temporal relations obtain among the constituent impulses in a volley? Do coöperating fibers fire synchronously (simultaneously), or is there asynchronism of any particular sort? Existent data fail also to throw light on this question. The type of data which might lead to the solution of this problem would consist of oscillographic records of the form and duration of the neural volleys as a function of various properties of the stimuli (both for natural sound stimuli sent into the ear, and for electrical stimuli applied directly to the nerve). But the data available concerning the form and duration of the volleys in the Davis-Saul effect are limited to the report that the volleys appear on the cathode ray oscillograph as waves of the shapes and time relations characteristic of action currents in peripheral nerves.[30] It is possible to conceive of waves of such characteristics being built up either by synchronous or asynchronous impulses in a volley; the specifications given do not help, therefore, in the solution of the problem mentioned above.

In the study of motor nerves, the question of the asynchronism of the impulses in a volley has been found to be of great importance, both in the interpretation of the mechanical records obtained from the muscles excited by the nerve-volleys and in the explanation of certain features of reflex and voluntary movement.[31] It is not unlikely that the temporal order in which the constituent impulses in a volley discharge may be of equal importance in the explanation of certain phenomena of audition.

There is one particular type of asynchronism among the impulses which, it seems to us, is to be expected on the basis of certain views commonly held concerning the mechanism of stimulation in the cochlea. This hypothe-

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The action potential in fibers of slow conduction in spinal roots and somatic nerves, *Amer. J. Physiol.*, 92, 1930, 43-82.

[28] Saul and Davis, *op. cit.*, this JOURNAL, 45, 1933, 359.

[29] Saul and Davis, *op. cit.*, *Trans. Amer. Otol. Soc.*, 22, 1932, 141.

[30] Saul and Davis, *op. cit.*, this JOURNAL, 45, 1933, 359.

[31] See, for example, J. F. Fulton, *Muscular Contraction and the Reflex Control of Movement*, 1926, 265-490.

sis was discussed in a recent paper as a phase of a frequency theory of hearing.[32] But the connection is not a necessary one. The essential features of the argument involved may therefore be outlined in terms that do not limit it to a frequency theory.

Let it be assumed (1) that corresponding to the cycle of displacements represented in a single period of a stimulating sound wave (or in a fraction of the period, such as one-half a period), there occurs an equivalent cycle of displacements of all or part of the basilar membrane from its resting or neutral position; and (2) that stimulation of the hair cells of the basilar membrane is due to this displacement of the membrane, and that, other things equal, the greater the displacement, the greater is the excitation effect produced in any hair cell in a displaced region of the membrane. These assumptions are implicit in many of the current discussions of the action of the cochlea.

The bearing of these assumptions on the question of asynchronism in the neural volleys may be illustrated in terms of two of the hypotheses used to account for an increase in the number of cells responding with increase in the intensity of the stimulating sound wave. One hypothesis (the "spread hypothesis") rests on the presumption that the extent of displacement along the *length* of the basilar membrane is greater for stimuli of greater amplitude. It is contended that more intense stimuli produce basilar displacements of greater linear extent, and thus lead to the stimulation of more cells. The second hypothesis (the "multiple limen hypothesis") suggests that not all the hair cells in a given region have the same limen. Sound waves of greater amplitude should stimulate more cells since the greater amplitude of displacement produced in the basilar membrane should lead cells of relatively less sensitivity to respond as well as those of maximal sensitivity.

Now if, in the case of either of these arrangements, the basilar membrane is followed through for an individual cycle, it becomes apparent that the constituent impulses in a volley will not be discharged synchronously but will, instead, be spread out in time. Suppose, for example, that cells of like sensitivity occur along the extent of the basilar membrane. Those which occur at the part of the membrane first undergoing displacement will necessarily be stimulated before those occurring in immediately adjacent regions. Consider, secondly, the consequences of the "multiple limen hypothesis." Suppose fibers of different limens occur at a region of the membrane undergoing displacement, and that these are all stimulated by the displacement. The more sensitive fibers must necessarily be stimulated before the less sensitive ones. For in the transition of the membrane from zero displacement to that necessary for stimulating the least sensitive of the fibers, all intermediate displacements must occur. So the fibers of different thresholds will discharge successively, in the order of their sensitivity, the most sensitive one discharging first.

An additional point of interest here relates to the phase position of the stimulating wave at which any given cell will discharge. It is apparent that the phase at which the cell will discharge must vary in a definite way with the amplitude of the wave for a stimulus of given frequency. For, the displacement of the basilar membrane necessary to excite the cell must be reached earlier in a wave of large amplitude than in one of small amplitude. This point becomes obvious immediately if two sine waves of the same frequency but different amplitudes are compared. An ordinate of any given height occurs sooner in the cycle of the wave of large amplitude than it does in that of small amplitude.

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[32] G. Kreezer, A neglected possibility in frequency theories of hearing, this JOURNAL, 43, 1931, 659-664.

One consequence of the hypothesis outlined above might be tested without any great difficulty. Records of the Davis-Saul effect obtained by means of a high frequency oscillograph, such as the cathode ray oscillograph would show whether, as the hypothesis suggests, the duration of the successive volleys increases as the amplitude of a stimulating sound wave of given frequency increases from liminal to maximally effective values.

Wever-Bray effect: (a) Cause. Before any significance which the Wever-Bray effect may have as an index of neural events can be determined, it is obvious that the phenomenon or phenomena on which it depends must first be discovered. As the problem stands at present, it is possible to conceive of any one or more of a considerable variety of bio-microphonic phenomena as involved in the effect.[33] But it would be valid to use the effect as an index of neural events only if it were due to a particular type of bio-microphonic phenomenon.

One of the possibilities that seemed at first to exist was that the Wever-Bray effect might be due to action currents of the auditory nerve. But this possibility has apparently been eliminated by the evidence that the action currents of the nerve give rise to the Davis-Saul effect. The afferent impulses in the nerve cannot be characterized by two contrary sets of properties. If the volleys of afferent impulses have properties like those described for the Davis-Saul effect, they cannot have properties like those characterizing the Wever-Bray effect.

One possibility remains, however, that would be of undoubted significance for auditory theory: the Wever-Bray effect may be due to action currents produced in the very earliest stage of neural conduction, that is to say, in the hair cells, regarded collectively. The available data on the Davis-Saul effect do not exclude this possibility; for these data link the Davis-Saul effect only with the afferent impulses in the auditory nerve and tracts. The nature of the action currents in the auditory receptors themselves, their frequency and form as a function of the stimulus, is still a problem for further research. These action currents may have the properties of the Davis-Saul effect; they may have the properties of the Wever-Bray effect, or they may have properties different from either.

There is one line of evidence, however, which makes it seem unlikely that the Wever-Bray effect can be due to action potentials in the hair cells.

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[33] Cf. Kreezer, *op. cit.*, this JOURNAL, 44, 1932, 641-649.

A number of investigators, it will be recalled, reported that the Wever-Bray effect reproduces not only the frequency but also the *form* of the stimulating sound wave. Thus, Wever and Bray, Adrian, and Rademaker and Bergansius all indicate that when the stimulus is sinusoidal in form, the electrical effect detected is likewise sinusoidal.[34] Davis and Saul too report that, except for frequencies beneath 800 cycles, the "spread" reproduces the wave form of the stimulus.[35] For frequencies beneath 800 cycles, they state that the form may be somewhat more complex.[36]

Now none of the hypotheses proposed so far to account for the retention of the stimulus frequencies in the response of the nerve as a whole explains how the wave form of the stimulus might also be retained. The volley theories account only for the reproduction of the stimulus-frequency, but not for that of the form.[37] Surely, any neural explanation of the *form* of the Wever-Bray effect, to be adequate, must not only suggest a temporal order of occurrence of action potentials in different fibers capable of building up the form of the stimulus wave, but must also suggest a plausible way in which this temporal order might be produced in the cochlea by the action of the stimulus. The mechanism of the cochlea must be taken into account as well as the properties of the nerve cells. The present lack of such a hypothesis makes it difficult to believe that the Wever-Bray effect can be due to action potentials in the hair cells.

The methodological character of the problem involved in the determination of the properties of the action potentials in the hair cells deserves emphasis. So long as the possibility exists that sound waves transmitted to the labyrinth fluids may give rise to a number of *different* electrical phenomena, the problem of determining the properties of the action potentials in the hair cells becomes one of developing a technique by means of which these potentials can be *isolated*. A similar problem was involved in the determination of the properties of the action potentials of the nerve. This problem Davis and Saul were apparently able to solve by an appropriate control of stimulus-intensity, the degree of amplification, and the position and character of the electrodes. In the case of the auditory receptor cells, analogous means must be developed for isolating the action potentials in order that their properties may be determined, unobscured by the properties of other phenomena which might occur simultaneously.

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[34] See footnote 18.

[35] Saul and Davis, *op. cit.*, this JOURNAL, 45, 1933, 359.

[36] This additional complexity in form may possibly be due to harmonics introduced into the stimulating sound waves as a result of the non-linear transmission characteristic of the ossicles. The question cannot, however, be settled without quantitative data. Cf. G. Kreezer, *op. cit.*, this JOURNAL, 44, 1932, 674.

[37] Cf. Troland, *op. cit.*, *J. Gen. Psychol.*, 2, 1929, 43 f., 50 f.; Wever and Bray, *op. cit.*, *Psychol. Rev.*, 37, 1930, 375-379.

(b) Its use as an index of sound transmission in the ear. Our previous discussion has been concerned with the possible significance of the auditory electrical effects as indicators of neural processes. Another possibility of importance is that the effects may also be capable of use as indicators of events in the physical stage of the hearing process; that is to say, they may be able to throw light on the intensity of sound transmitted as stimuli to the inner ear, and on the function of various parts of the ear in regulating this transmission.

An attempt to use the Wever-Bray effect as an index of the intensity of the sound waves transmitted to the cochlea has been made by Hughson and Crowe.[38] As a result of their experiments, these authors have proposed a new hypothesis concerning the function of the round-window membrane. Finding that pressure on the membrane led to an increase in the intensity of the Wever-Bray effect, they concluded: (1) that the increased pressure must have led to an increase in the intensity of the sound stimuli reaching the cochlea; (2) that the round-window membrane, owing to its mobility, absorbs a large percentage of all sound entering the ear; and (3) that this membrane acts as a safety valve to protect the structures of the inner ear. In their recent experiments with the Wever-Bray effect, Hughson and Crowe have placed emphasis upon the development of more precise methods for measuring the intensity of the effect as detected in the telephone or loud speaker.[39] The weakness in their whole position lies, however, not in the lack of sufficient precision in their observations, but in the failure to prove that the Wever-Bray effect may be accepted as a reliable indicator of the intensity of the sound waves effective as stimuli in the cochlea. In the absence of such proof, the observations of Hughson and Crowe on the changes produced in the Wever-Bray effect by experimental modifications of the round-window membrane cannot be regarded as lending support to their hypothesis concerning the functions of this membrane. Their hypothesis, moreover, receives no support on the grounds that it represents the only possible explanation of their results. As we shall see below, other explanations may be given.

Justification for the use of a given effect a as an indicator of some variable might be claimed, conceivably on one of two grounds: (1) on the grounds merely of an empirical correlation between the properties of the effect and those of the variable; or (2) on the basis of knowledge of the *phenomena* connecting the effect and the variable in question as well as knowledge of the relations existing between their respective properties. Thus, if the effect were known to depend on the variable x in a given way, or if the effect and the variable x both depended on the same phenomenon, the properties of the effect and the variable x would vary coördinately, other things equal. In the use of the one as an index of the other, it would be necessary, of course, that other things be kept equal, that is to say, it would be necessary that additional factors which might influence the properties of the effect or of the variable x be kept constant, or their influence allowed for by means of appropriate calculations.

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[38] Hughson and Crowe, *op. cit., J. Amer. Med. Assoc.,* 96, 1931, 2027-2028.

[39] W. Hughson and S. J. Crowe, Experimental investigation of the physiology of the ear, *Acta Otolar.,* 18, 1933, 291-339.

The first method mentioned above for justifying the use of an effect as an indicator is that of establishing an empirical correlation between the properties of the effect and the variable in question. The usefulness of an indicator resting on such a basis is subject to certain limitations. For so long as the nature of the phenomena connecting the variable and the effect is unknown, there is never any assurance that the correlation will hold under conditions different from those under which it was established. In order, therefore, to permit the *general* use of a given effect as an index of some variable, it would seem necessary that the nature of the causal relation between the two be known.

This point may be illustrated by reference to a hypothesis[40] which we have suggested to account for the observations of Hughson and Crowe without accepting their conclusions concerning the function of the round-window membrane. Suppose it is granted that sound energy transmitted to the labyrinth liquids by the vibration of the base of the stapes will be propagated in all directions through the liquids in accordance with the usual factors determining sound propagation. One channel for the sound waves will be through the cochlea by way of the basilar membrane and out through the round window membrane. Another channel will be through the cochlea by way of the helicotrema and out through the round window. A third channel will include all the paths by which sound waves may be transmitted through the liquids to the bone walls of the labyrinth and out through the walls to the tissues beyond. These three channels must be regarded as in parallel with each other, in the sense in which this expression is used in describing electrical systems. Further, we may assume that only the sound travelling in the first channel, via the basilar membrane, can be effective in stimulating the receptor cells in the organ of Corti, which rests on the basilar membrane. Suppose, secondly, that the sound energy transmitted through the walls of the labyrinth leads to a biomicrophonic effect which we may call the α effect, having properties like those of the Wever-Bray effect, and that the intensity of this effect varies with the intensity of the sound waves thus transmitted through the walls (in Channel 3). This bio-microphonic effect might be thought of as produced in any or all of three possible ways: (1) as an electrical effect produced in the tissues upon the passage of the mechanical sound waves through them;[41] (2) as a resistance microphonic effect at the electrodes;[42] or (3) as a condenser-microphonic effect at the electrodes.[43] Let it be assumed, further, that no part of the α effect is due to action currents in the hair cells.

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[40] Cf. Kreezer, *op. cit.*, this JOURNAL, 44, 1932, 671 f.

Let us now examine the conditions under which a correspondence would be found between the α effect and the intensity of the sound waves transmitted via the basilar membrane. Consider, first, what the effect would be of varying the intensity of the sound waves sent into the ear. Corresponding to such a variation, there would occur a variation in the same direction, both of the intensity of the sound waves transmitted via the basilar membrane, and in the intensity of the sound transmitted through the walls, and in consequence, a perfect correlation between the intensity of the sound transmitted via basilar membrane and the intensity of the α effect. Secondly, suppose various operations were performed upon the ossicles such as were performed by Hughson and Crowe in their experiments.[44] There would be a consequent decrease in the efficiency of the bones in transmitting sound vibrations, and therefore, for a given intensity of physical stimulus, a decrease both in the energy transmitted via the basilar membrane in unit-time and in that transmitted through the walls. Thus again a correlation in the intensity of the sound transmitted via basilar membrane and in the intensity of the α effect would be found. Thirdly, suppose the round window membrane (or some part of the walls) is punctured so as to lead to a loss of labyrinth fluid (as in certain of Hughson and Crowe's experiments).[45] The loss of the liquid would reduce the efficiency with which sound waves were transmitted by means of the liquid to the walls or through the cochlea. There would consequently be a decrease in the intensity of the α effect as well as in the intensity of the sound waves transmitted via the basilar membrane. Again a correlation between the α effect and the intensity of the sound waves transmitted via basilar membrane.

Suppose now, in an animal with intact labyrinth, pressure were exerted on the round window membrane, so as to increase the acoustic impedance of the cochlear channels to the range of stimulus frequencies being used. The intensity of the sound waves transmitted through the cochlear channels would accordingly decrease. *On the other hand,* since the alternating pressures set up by the stapes at the oval window could not now be reduced by the displacement of the cochlear fluids made possible by the yielding of the round window membrane, *more* acoustic energy would flow *through the walls* in unit-time.[46] There would thus be an *increase* in the intensity of the α effect, although at the same time a *decrease* in the intensity of the sound waves transmitted via basilar membrane. In this case, then, the α effect would not be reliable as an index of the intensity of the sound waves transmitted via the basilar membrane.

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[41] *Ibid.,* 646-648. [42] *Ibid.,* 644-646. [43] *Ibid.*

[44] Hughson and Crowe, *op. cit., J. Amer. Med., Assoc.,* 96, 1931, 2027.

[45] *Ibid.,* 2028.

[46] This type of behavior might be illustrated by considering how the amount of swelling of the walls of a rubber tube into which air is forced at a given pressure depends on the elasticity of the membrane closing the opposite end of the tube. The swelling of the walls would obviously be much greater when a rigid cap is used than when an easily stretched membrane is used to close the end of the tube.

It is apparent that although a perfect correlation might exist between the effect and the variable x for a number of conditions, this fact would not assure the existence of the correlation for still other conditions. To determine whether an effect might be used as an indicator under conditions different from those under which a correlation had been established, it would be necessary for the phenomena connecting the effect and the variable to be known.

An examination of the present status of the Wever-Bray effect, in the light of these considerations, indicates its inappropriateness as a general index of the intensity of the sound waves effective as a stimulus in the inner ear. The work of Davis and Saul shows that the Wever-Bray effect cannot be due to action currents of the auditory nerve. Whether the effect can be due to action potentials of the auditory receptor cells remains a question for further research. For the effect to be reliable as an indicator, moreover, it would not be sufficient merely for some component of the effect to be due to action potentials of the hair cells. It would be necessary also that other possible factors in the magnitude of the effect, such as bio-microphonic phenomena of the sort previously mentioned, should be under control so that they could either be eliminated or kept from varying independently of the component due to the hair cells.

The difficulties standing in the way of the use of the Wever-Bray effect as an index of cochlear events do not exist in the case of the Davis-Saul effect. The discoverers of the Davis-Saul effect have, as we have seen, presented strong evidence that the effect is due to action potentials of the auditory nerve. They have developed a technique for isolating the effect which permits its properties to be determined without masking by the Wever-Bray effect. In a particular experimental-animal, there should be no difficulty in maintaining sufficient precautions against the occurrence of variations in the excitability of the auditory receptors or nerve fibers, variations which would vitiate the reliability of the effect as an index of the intensity of the stimulus in the inner ear. Some additional work must be done, it is true, to determine the precise attribute of the Davis-Saul effect which is correlated with the intensity of the physical stimulus. As has been suggested, it is conceivable, on the basis of the data so far available, that the impulses in each of the successive volleys are asynchronous, and that the duration of the nerve volleys may vary with the intensity of the stimulating wave. In such a case, the total area under the curve representing any volley would have to be taken as the index of the intensity of the stimulus rather than the height of the curve. On the other hand, if the impulses contributing to a given volley should be found to be synchronous, then for a given frequency, the height of the curve could be considered the appropriate index. There is every reason to expect that, after such preliminary correlations, the Davis-Saul effect might be used as a reliable index of the intensity of the sound waves effective as a stimulus in the inner ear.

SUMMARY

The aim of the present paper was to discuss the significance of the auditory electrical effects for auditory theory. Two distinct effects have so far been reported in the literature: the Wever-Bray effect and the Davis-Saul effect. There are two circumstances under which such effects might be considered of significance for auditory theory: (1) if the effect could be considered an index of *neural* events occurring at some stage of the auditory pathways; and (2) if the effect could be considered an index of events in the *physical* stage of the hearing process, that is to say, an index of the sound waves serving as stimulus in the inner ear.

The possibility of using an effect as an index of neural events depends on the strength of the evidence that the effect is due to action potentials occurring at some level in the auditory pathways. New data recently brought forward by Davis and Saul seem to show conclusively that the effect discovered by them is due to action potentials of the auditory nerve and tracts. Consequently their report that the frequency-correlation between the stimulus and the volleys produced by the nerve as a whole does not extend beyond frequencies of 1000 per sec. indicates the incorrectness of the so-called *volley theories* of hearing. For these theories assume that the higher audible frequencies (*e.g.* those beyond 1000 cycles) can be transmitted as volleys of the auditory nerve as a whole. The data concerning the frequency limitation of the volleys from the whole nerve do not, however, throw any additional light on the correctness or incorrectness of the telephone type of auditory theory, which assumes that the entire range of audible frequencies are transmitted by the individual fibers of the nerve. This indecisiveness rests on the difficulty of obtaining information about the properties of the individual fibers from records of the volleys produced by the nerve as a whole. A number of additional problems relating to the properties of the individual fibers of the auditory nerve are discussed.

With respect to the Wever-Bray effect, the evidence that the Davis-Saul effect is due to action potentials of the auditory nerve and tracts eliminates the possibility that the Wever-Bray effect can be caused by action potentials of these tissues. The possibility still remains that the Wever-Bray effect may be due to action potentials of the auditory receptor cells. There are facts which make this seem unlikely, but the question remains for further research to decide.

The present lack of knowledge as to the specific phenomenon or phenomena responsible for the Wever-Bray effect also makes it impossible to consider it reliable as a general indicator of the intensity of the sound-waves effective as stimuli in the cochlea. Consequently, the observations of Hughson and Crowe on the changes produced in the Wever-Bray effect by experimental operations upon the round window membrane cannot be regarded as lending support to their hypothesis concerning the functions of this membrane. Nor does their hypothesis receive support on the grounds that it furnishes the only possible explanation of their results, for other explanations can be given. The difficulties which face the use of the Wever-Bray effect as an indicator do not exist in the case of the Davis-Saul effect. It is accordingly suggested that it might be used as a reliable index of events at the physical stage of the hearing process.

RETROACTIVE INHIBITION IN THE LEARNING OF POETRY

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The phenomenon of retroactive inhibition has become, long since, a securely established fact; and as knowledge of the characteristics and conditions of such inhibition accumulates, it is becoming increasingly probable that forgetting is to a large extent a function of the events interpolated between learning and the measurement of retention.[1] The major experimental investigations of the problem have, however, almost always employed, as learning materials, series of disparate items having either very little meaningful relation to each other or none at all. It is a justifiable inference that, if interpolated events produce the forgetting of series of this kind which, by the time they have been learned, have been woven by the *S* into a partially meaningful organization, interpolated events will also produce the forgetting of materials which are meaningful to the *S* at the beginning of the learning period. This inference remains, however, at the level of hypothesis, and a more complete evaluation of the theory that all forgetting is a function of interpolated events waits upon experimental tests with a wide sample of representative meaningful materials. The present paper reports an experiment on the influence of interpolated learning upon the retention of a section of one representative sample, viz., poetry.

A brief review of the major learning materials previously employed in experiments upon retroactive inhibition will serve to place the present experiment in its proper context and perspective. In the work which gave impetus to all later studies of the problem, Müller and Pilzecker[2] used nonsense syllables and a number of later investigators have followed this precedent.[3] Lists of meaningful words come second in frequency of use,[4] and after them lists of numbers,[5] memory spans of consonants and digits,[6] lists of figures of varying design,[7] chess patterns,[8] and mazes.[9] It is difficult, on account of the widely different experimental conditions under which these materials have been used, to make valid comparisons of the relative susceptibilities of the different materials themselves to retroaction. The fact of chief importance for the present purpose is, however, that all have been shown to be susceptible to some degree of disintegration from interpolated learning. With the possible exception of chess patterns, moreover, all are series of items which possess for the learner no large amount of initial organization or meaningful interrelation. There are no facts available which constitute direct evidence of the susceptibility of materials possessing such meaningful interrelation or unity to retroactive inhibition.

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[1] McGeoch has shown that disuse, as such, is of no significance as a primary conditioning factor of forgetting, and that interpolated events are the most important one of the two primary factors of which forgetting is a function. See J. A. McGeoch, Forgetting and the law of disuse, *Psychol. Bull.*, 28, 1931, 670; and the complete paper of the same title, *Psychol. Rev.*, 39, 1932, 352-370.

[2] G. E. Müller and A. Pilzecker, Experimentelle Beiträge zur Lehre vom Gedächtniss, *Zsch. f. Psychol.*, Erg. Bd. 1, 1900, 1-300.

Method

Experiment I. In this experiment 12 lines (3 stanzas) from each of two different poems have been employed as the materials for original learning. The poems are *John Gilpin's Ride* (Poem I) and *Alice Brand* (Poem II) and the stanzas used are from those given in the Foster and Tinker Manual.[10] The experiment has been conducted with groups of *Ss* and, in order to prevent the *Ss* from copying from each other, one poem was used with those in the odd-numbered seats and the other with those in the even-numbered seats. This precaution, together with the close watch maintained by *E* during the writing of the recalls, has reduced the variable of copying very close to zero. Each section of 12 lines was presented to *S* mimeographed on a single sheet and 5 min. were allowed for study.[11] Immediately following this period of study, *S* wrote a recall for which 5 min. were allowed. After 15 min. from the end of this recall period *S* wrote a second recall, for which 5 min. were given. One week later, at the same hour, a third 5-min. recall was written. This procedure was a constant for all groups and under all conditions.

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[3] M. Foucault, Introduction à la psychologie de la perception. Expériences sur l'oubli ou sur l'inhibition régressive, *Rev. des Cours et Conférences,* 21, 1913, 444-454; R. Heine, Über Wiedererkennen und rückwirkende Hemmung, *Zsch. f. Psychol.,* 68, 1914, 161-236; J. E. DeCamp, A study of retroactive inhibition, *Psychol. Monog.,* 19, 1915, (no. 84), 1-69; E. C. Tolman, Retroactive inhibition as affected by conditions of learning, *Psychol. Monog.,* 25, 1917 (no. 107), 1-50; E. S. Robinson, Some factors determining the degree of retroactive inhibition, *Psychol. Monog.,* 28, 1920, (no. 128), 1-57; J. G. Jenkins and K. M. Dallenbach, Obliviscence during sleep and waking, this JOURNAL, 35, 1924, 605-612; E. B. Skaggs, Further studies in retroactive inhibition, *Psychol. Monog.,* 34, 1925, (no. 161), 1-60; A. Dahl, Über den Einfluss des Schlafens auf das Wiedererkennen, *Psychol. Forsch.,* 11, 1928, 290-301; N. Y. Cheng, Retroactive effect and degree of similarity, *J. Exper. Psychol.,* 12, 1929, 444-449; J. A. McGeoch, The influence of degree of learning upon retroactive inhibition, this JOURNAL, 41, 1929, 252-262; The influence of degree of interpolated learning upon retroactive inhibition, *ibid.,* 44, 1932, 695-708; L. M. Harden, The effect of emotional reactions upon retention, *J. Gen. Psychol.,* 3, 1930, 197-220.

[4] W. Hinrich, Zur Frage der rückwirkenden Hemmung, *Zsch. f. Psychol.,* 99, 1926, 389-392; P. L. Whitely, The dependence of learning and recall upon prior intellectual activities, *J. Exper. Psychol.,* 10, 1927, 489-508; J. A. McGeoch and W. T. McDonald, Meaningful relation and retroactive inhibition, this JOURNAL, 43, 1931, 579-588; Tolman, Skaggs, and Harden, *opp. citt.*

[5] Tolman, Robinson, and Harden, *opp. citt.*

[6] E. S. Robinson, The "similarity" factor in retroaction, this JOURNAL, 39, 1927, 297-312; and L. M. Harden, A quantitative study of the similarity factor in retroactive inhibition, *J. Gen. Psychol.,* 2, 1929, 421-430.

[7] Dahl, *op. cit.*

[8] DeCamp, Robinson, and Skaggs, *opp. citt.*

[9] L. W. Webb, Transfer of training and retroaction, *Psychol. Monog.,* 24, 1917, (no. 104), 1-90.

[10] W. S. Foster and M. Tinker, *Manual for Instructors: Experiments in Psychology,* 1929.

The variables under experimental control are, of course, the interpolated events which fill the first 15-min. interval. In the control or rest condition the *Ss* took the Seashore pitch discrimination test for 15 min., writing their judgments on a previously prepared record sheet. This is far from a 'rest' condition in the sense of complete relaxation, but it clearly requires no learning activity, the factor which we wish to eliminate in this condition. One must use some device to prevent rehearsal by untrained *Ss* during the control interval. In the first work condition, *S* began to study another 12-line section of the same poem immediately following the first recall of the original material. He studied this interpolated poem for 5 min., immediately wrote for 5 min. a recall thereof, and then took for 5 min. the Seashore pitch discrimination test. This condition differs from the rest condition only in that the *Ss* studied an additional 12 lines of poetry and recalled them during the first 10 min. of the 15-min. interval. Under the second work condition, immediately after the first recall of the original material *S* began to study two lists of 10 nonsense syllables each.[12] At the end of 5 min. he was allowed 5 min. for recall, after which he took the Seashore test for 5 min. The variable here is, of course, the learning and recall of the nonsense syllables.

A clear understanding of the conditions will be facilitated by the following outline in which each step in order is enumerated. The initials OM and IM refer to the original and interpolated materials, respectively.

Rest Condition	Work Conditions 1 and 2[13]
Study OM 5 min.	Study OM 5 min.
Recall OM 5 min.	Recall OM 5 min.
Pitch Test 15 min.	Study IM 5 min.
Recall OM second time 5 min.	Recall IM 5 min.
7-day Interval	Pitch Test 5 min.
Recall OM third time 5 min.	Recall OM second time 5 min.
	7-day Interval
	Recall OM third time 5 min.

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[11] The instructions for the study of the poetry were: "On the other side of this sheet you will find 3 stanzas of poetry. You will be given 5 min. in which to study them. Use any method you wish, but be sure to have studied each stanza about equally by the end of the 5 min. period. Waste no time, but study the stanzas with all the concentration which you can command."

[12] The instructions for studying the nonsense syllables were: "On the small strip of paper are 2 lists of nonsense syllables. You will be given 5 min. in which to study them. Spend the first 2 min. on the first list and when the signal is given go to the second list, and spend 2 min. on it. When a second signal is given review both lists for 1 min." All instructions were read twice to the *Ss* slowly and deliberately.

[13] The two work conditions are exactly the same in outline. In the first the interpolated material consists of 12 more lines of the same poem as the one which constituted the original material. In the second the interpolated material is 20 nonsense syllables in two lists of 10 each. All *Ss* learned the same syllables.

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At the beginning of the experiment with each group each *S* was presented with all of the material which he would need, including blank sheets for the writing of the recalls, clipped together in order, with the proper designation written on the back of each sheet. At each successive step *S* had only to place the sheet at which he had been working face down at the bottom of his packet of material, turn over the next sheet at the proper signal, and proceed. It was possible, thus, to adhere rigorously to the time schedule planned. At the close of the second recall the *Ss* were given careful and rigid instructions against rehearsal. They were also asked to record whether they had ever seen or heard the poems before; and the records of those who reported that they had were discarded.

The recalls of the poetry were scored in three ways: (1) number of lines correctly recalled, which is the score upon which most emphasis will be placed, since it is the most exact and rigorous measure; (2) number of lines the meaning of which is correct; and (3) number of lines the meaning of which is partially correct. In both (2) and (3) objective criteria were applied. A line was placed under (2) only when synonyms were substituted for the original words or when very slight rearrangements of words left the meaning intact. A line was included under (3) if 3 or more connected words, carrying part of the meaning of the line, were given. The nonsense syllables interpolated in the second work condition were scored in terms of number correct regardless of position.

In computing the degree of retroactive inhibition, it is necessary to deal with differences between percentages of recall rather than with differences between mean scores at the delayed recalls, on account of the differences obtaining between degrees of learning as measured by immediate recall. It is possible to compute percentages of recall either in terms of the raw scores or in terms of the means of the percentages yielded by each individual *S*. In this experiment the two methods yield results which are the same in tendency and the results from the former only will be presented. The latter has the advantage that sigmas may be found directly in terms of percentage of recall, but in our data a few cases yielded very high percentages of recall, thus distorting both means and sigmas to a degree which is misleading if the percentages are computed individually instead of from the means. A typical case of distortion occurs when *S* has scores on immediate recall of one line correct, 8 correct as to sense, and 2 partially correct, while on a delayed recall he has 3 lines correct, 4 correct as to sense and 3 partially correct. It is easily understandable that two of the lines which were correct only in meaning on the first recall should be correct in wording also at the second recall without assuming rehearsal,[14] but such a *S* would have a recall of 300%, a huge deviation from the mean of the individual recall scores. The use of the raw scores in such cases more conservatively and accurately represents the facts.

Experiment II. The rest and one work condition, with interpolated learning of poetry in the latter, have been repeated with different groups but under conditions duplicating those of Experiment I save that the recall after 15 min. has been omitted from both conditions. We are, thus, able to find by a comparison of the recalls after 7 days in the two experiments whether the distributed practice afforded by the recall after 15 min. has affected the degree of inhibition after 7 days.

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[14] Cf. W. Brown, To what extent is retention measured by a single recall? *J. Exper. Psychol.*, 6, 1923, 377-382.

Results

Experiment I. The data for each of the two poems used will be presented separately. This will not only permit comparison of the two, but, since the results are confirmatory of each other, will give increased assurance to the conclusions. Table I gives the mean numbers of lines correctly recalled, the sigmas of the means, the percentages of recall computed from these means, together with the percentages of retroactive inhibition found by subtracting the percentage recalled under work from the percentage recalled under rest and dividing the remainder by the percentage under rest. Percentage of recall is always found with the mean at immediate recall, which is thus treated as a measure of learning, in the denominator.

TABLE I

Lines Correctly Recalled: Means, Sigmas, Percentages of Recall and of Retroactive Inhibition

			Time of Recall						
			Immediately	After 15 min.			After 7 days		
Poem	Condition	N	Mean	Mean	% Rec.	% R.I.	Mean	% Rec.	% R.I.
	Rest	35	8.65±.57	7.43±.62	85.9		5.08±.65	58.7	
I	Work (Poet.)	31	6.96±.58	5.84±.60	83.9	2.3	3.84±.66	55.1	6.1
	Work (N.S.)	27	8.07±.49	7.11±.60	88.1	+2.5	3.85±.71	47.7	18.7
	Rest	30	7.63±.61	7.03±.64	92.1		5.96±.64	78.1	
II	Work (Poet.)	37	7.16±.50	6.19±.51	86.4	6.1	4.24±.55	59.2	24.2
	Work (N.S.)	28	7.00±.58	5.64±.60	80.5	12.5	4.39±.50	62.7	19.7

Recall after 15 min. is high under all of the conditions studied and there is only a very slight amount of retroaction present. When poetry is the interpolated material, 2.3 and 6.1% of inhibition appears. When nonsense syllables are the interpolated material, the first poem shows 2.5% better retention than under rest, and the second shows 12.5% of loss.[15] The differences between the means of the rest and work conditions, both at immediate recall and after 15 min., are not statistically reliable, and the means of the percentages of recall computed for each *S*, while showing the same tendency, lack statistical reliability. It may be concluded that the interpolated learning of 12 additional lines of the same poem decreases after 15 min. the recall of the 12 lines first learned by very small and statistically unreliable amounts. The fact that this small amount of inhibition occurs with each poem, however, lends support to the reliability of the tendency.

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[15] The interpolated poems gave immediate recalls, lines correct, of 7.5±.58 and 7.7±.46, respectively. The corresponding recalls of the nonsense syllables under the second work conditions were 8.5±.71 and 9.8±.94.

When, however, 7 days elapse between the two recalls, the percentages of retroaction in the first work condition increase to 6.1 and 24.2. The differences involved are still not statistically reliable, but the agreement of the two poems weighs in favor of a tendency toward an increase in inhibitory effects with an increase in the elapsed time.

The interpolation of the learning of 20 nonsense syllables is followed by a slight facilitation effect in the case of the first poem and by 12.5% of inhibition in the case of the second. The lack of agreement makes interpretation difficult. It may be that degree of retroaction is a function of the poem used, particularly of its difficulty. Clearly the second poem tends to be learned somewhat less well and to have the greater susceptibility to inhibition. The lessened recall which appears after the interpolation of nonsense syllables may be valid.

TABLE II

Lines with Meaning Wholly Correct and Lines Partially Correct: Percentages of Recall and of Retroactive Inhibition

			Meaning Correct				Meaning Partially Correct			
			After 15 min.		After 7 days		After 15 min.		After 7 days	
Poem	Condition	N	% Rec.	% R.I.	% Rec.	% R.I.	% Rec.	% R.I.	% Rec.	% R.I.
	Rest	35	125.3		135.7		208.3		170.8	
I	Work (Poet.)	31	104.1	16.9	111.8	17.6	143.7	31.0	108.4	36.5
	Work (N.S.)	27	119.8	4.3	98.8	27.1	145.6	30.1	177.7	4.0
	Rest	30	125.0		101.8		103.7		172.6	
II	Work (Poet.)	37	112.0	10.4	95.0	6.6	121.4	+17.0	123.7	28.3
	Work (N.S.)	28	224.8	+79.8	200.0	+96.4	86.5	16.5	130.4	24.4

More important, however, is the fact of complete agreement in the increase in retroaction at the 7-day interval. Both poems, under both conditions of interpolation, yield considerably greater percentages of inhibition at the longer interval and the amounts following the interpolation of the syllables are not consistently larger or smaller than those following the interpolation of the poetry.

We shall inquire next into the influence of interpolated learning upon the two other scores, viz., lines correct in meaning but not wholly correct in verbal statement and lines partially correct in meaning. These scores are of secondary importance and it will be sufficient to present only the percentages of retention and of retroaction (Table II). There is in most cases a gain in these scores from the immediate to the delayed recalls which represents, presumably, a shift from complete toward partial correctness. This shift occurs more often and usually to a greater degree under the rest condition than under the work conditions. This brings it about that, in lines correct in meaning, the interpolated learning of poetry produces 16.9 and 10.4% of inhibition after 15 min., and 17.6 and 6.6% after 7 days. The interpolated learning of nonsense syllables is followed by 4.3% of inhibition in the case of Poem I and by a facilitation effect of 79.8% with Poem II. After 7 days these figures have shifted still farther in their respective directions. In lines partially correct, Poem I suffers a considerable degree of inhibition from both interpolations after the 15-min. period and a still greater amount from interpolated poetry after 7 days. After this interval the inhibition from the syllables is smaller than it was after 15 min. Poem II is recalled better after the learning of the interpolated poetry than after the non-learning (rest) period and worse after the learning of the syllables. In both cases inhibition is relatively large after 7 days.

In 5 cases out of 6 the interpolated learning of poetry has produced at least a small amount of inhibition at recall after 7 days. It is necessary to conclude that poetry is, under these conditions, slightly susceptible to retroactive inhibition after a 15-min. interval and that these inhibitory effects become more pronounced after 7 days. In 4 cases out of 6 the interpolated learning of nonsense syllables is followed after 15 min., likewise, by decreased recall of the original material, and in the same percentage of cases this decrease becomes more marked after 7 days. Poetry is, thus, also susceptible to inhibition from the interpolated learning of nonsense syllables and this inhibition is of the same order of magnitude as that produced by the interpolation of lines from the same poem.

The conclusions just drawn are supported more clearly by the data in terms of lines wholly correct, which is the most rigid and directly interpretable method of scoring, than by the two secondary methods. The exact significance of these latter methods is less easy to see on account of the complex relationships between them and between each of them and lines totally correct. In general, however, they support the conclusions drawn from the primary method. The retroaction which occurs in terms of perfect lines probably operates to produce more partial scores and, since the partial scores (meaning correct and meaning partially correct) also suffer retroaction, the effect has clearly been cumulative in character, much greater than that which is represented in any single score, and, in one sense, the sum of the effects produced in all three. It follows that the conclusions drawn may be accepted with confidence.

Experiment II. This experiment is an exact reproduction of the rest condition and of the first work condition of the preceding experiment, save for the single exception that the recall after 15 min. has been omitted under both conditions. It is possible that the increased retroaction after 7 days may have had this recall as one of its major conditions; or, as is equally possible, the recall after 15 min. may have acted in the opposite direction to diminish the amount of the increment in retroaction. This experiment is designed to discover whether either of these possibilities is actual and whether the recall under discussion has had any important effect upon degree of retroaction after 7 days.

The results from the primary method of scoring appear in Table III. The percentages of inhibition are larger, in the case of each poem, than are the corresponding percentages found in Experiment I. They are 29.1 and 38.0 as compared with 6.1 and 24.2. The means of the immediate recalls of the two experiments are not as nearly identical as one could wish, but there is no apparent reason for assuming that the small differences present are the major conditions of the results. It may be concluded from the two experiments that the poems used are, at recall after 7 days, clearly susceptible to inhibition from the interpolated learning of similar poetry and that the degree of inhibition is greater when there is no intervening recall after 15 min. The recall after 15 min. operates to diminish the inhibition at the subsequent recall after 7 days.[17]

TABLE III

LINES CORRECT: MEANS, SIGMAS, PERCENTAGES OF RECALL AND OF RETROACTIVE INHIBITION[16]

	Poem I					Poem II				
Condition	N	Immed. Rec.	Rec. 7 days	% Rec.	% R.I.	N	Immed. Rec.	Rec. 7 days	% Rec.	% R.I.
Rest	33	7.84±.60	4.45±.58	56.7		31	6.42±.54	3.29±.63	51.2	
Work	31	8.29±.59	3.35±.64	40.2	29.1	30	6.30±.50	2.00±.49	31.7	38.0

The secondary scores are shown in Table IV. Again only the percentages need be given. The percentages of retroaction in terms of meaning correct are 20.7 and 40.7 and are, thus, larger than those found in Experiment I. Lines partially correct show less inhibition for Poem I than in the preceding experiment and greater inhibition for Poem II. When it is noted that in this experiment Poem I suffers a considerably larger degree of inhibition of perfect lines than it did in Experiment I, 29.1% as against 6.1%, the failure of partial lines to follow the general tendency and to show an increased retroaction becomes inconsequential. With this single unimportant exception these secondary measures support the conclusion drawn from measurement in terms of lines perfectly recalled.

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[16] The immediate recalls of the 12 lines of interpolated poetry are 8.03±.57 and 7.56±.53.

[17] The experiments afford no evidence upon the manner in which this recall operates to reduce inhibition at the subsequent recall, but it is a plausible speculation that it does so by virtue of the rehearsal or relearning which it provides.

TABLE IV

LINES WITH MEANING WHOLLY CORRECT AND LINES PARTIALLY CORRECT: PERCENTAGES OF RECALL AND OF RETROACTIVE INHIBITION

Condition	Poem I				Poem II			
	Meaning Corr.		Partially Corr.		Meaning Corr.		Partially Corr.	
	% Rec.	% R.I.	% Rec.	% R.I.	% Rec.	% R.I.	% Rec.	% R.I.
Rest	77.2		132.2		50.6		98.3	
Work	61.2	20.7	112.1	15.2	30.0	40.7	51.6	47.5

DISCUSSION

We have found that poetry is susceptible to a small degree of inhibition from the interpolated learning of poetry of the same kind, when recall occurs 15 min. after the original learning, that this susceptibility is increased considerably after 7 days, and that it is greater after 7 days when there has been no intermediate recall after 15 min. The interpolated learning of nonsense syllables has the same general effect as does the interpolation of poetry when recalls occur after 15 min. and again after 7 days. These results raise three questions which require discussion. (1) What is the importance of the fact that poetry is susceptible, even though only slightly when the interval is but 15 min., to a decrement from interpolated learning? (2) What interpretation is to be put upon the increase in degree of inhibition with increasing time interval? (3) In the light of the relation, demonstrated with other materials, between similarity and retroaction the rough equality between the amounts of inhibition following interpolations of poetry and of nonsense syllables is anomalous and calls for explanation. This question obviously involves the transfer and perseveration theories of retroactive inhibition. We shall deal with these three in order.

A major part of our problem was to discover whether poetry is at all susceptible to disintegration from interpolated learning and, thereby, to make one test of the hypothesis that interpolated experience is a major condition of forgetting. We have found that it is susceptible and have, to that extent, increased the generality of the hypothesis. The interpolated learning does not, of course, account for all of the forgetting which has measurably taken place. In the rest condition, without interpolated learning, a decrement occurs; it is only less in extent than that found under work. This decrement under rest may be a function of the events which filled the interval—the process of discrimination, the inevitable flux of events arising from extra- and intra-organic stimulation, and from associative sequences. That everyday experience of this general kind has an inhibitory effect Jenkins and Dallenbach have shown,[18] and it is reasonable that the decrements under the rest condition, and those which remain over and above the effects of interpolated learning under the work condition, are a result of such experience. There is also the possibility that a blocking of perseveration and, perhaps, other factors have been operative. The important thing here is, however, that, whatever other conditions may also be present, poetry is susceptible to inhibition from interpolated learning and that, by virtue of this fact the generality of the phenomenon of retroaction is extended and its importance as a condition of forgetting thereby increased.

The fact of the slight amounts of inhibition after 15 min. offers a problem, also, but one which reaches beyond the boundaries of the present experiment and into the wider issues of mental organization and of psychological system. Into these issues we shall not venture. It is sufficient here to recall that the already available facts upon curves of retention lead one to expect a smaller decrement in the case of poetry than in that of the less organized serial materials and that the fact of the slight decrement from interpolation is consistent with these other facts. To ask why poetry should suffer so small a loss is tantamount to asking why meaningful material is forgotten less rapidly than is non-meaningful, an inquiry which points beyond the intent of our discussion.

We may now turn to the second problem, that of the interpretation of the increase in retroaction from 15 min. to 7 days. There are at least four conditions to be considered as possible factors in the bringing about of this result.

(a) It is possible that the increase after 7 days is a statistical artifact. This would be readily thinkable on some such grounds as the following. Let the interpolated learning inhibit the recall of one line of poetry after 15 min. and let this line be inhibited permanently so that it is not recalled again. Then let whatever other factors bring about forgetting operate equally upon the activities learned under rest and under work. The result would be a constant difference between the two conditions at successive points in time, but a steadily increasing percentage of in-tions from the same poem are, by any criterion, more similar to each other than are one such section and 20 nonsense syllables. The two sections of poetry are, moreover, sufficiently different to prevent any supposition that the learning of the second constituted a continued practice at the first and, thus, a situation from which inhibition could not be expected. It is, also, far from likely that the learning of the interpolated lines was accompanied by a running back over or rehearsal of the 12 lines previously learned. The rhythm of the two sections is the same, but the *Ss* were not told that the two sections were from the same poem and the narrative is sufficiently loose to prevent ready detection of the fact that it is continued in the second section. The recall of the interpolated section was, furthermore, as good as that of the original one, a result hardly to be expected if time had been spent reviewing the original one. These facts, together with the additional one that the *Ss* were learning under a time limit and would have no reason for going back to rehearse the poem previously learned and recalled, make the comparison of the results of the two work conditions valid. They offer, then, equivalent amounts of inhibition from the interpolation of two activities which differ very widely in their similarity to the original activity. These two interpolations have results of the same character after both 15 min. and 7 days.

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[18] Jenkins and Dallenbach, *op. cit.*

This failure of our results to conform to the tendency of other materials offers, in itself, an important problem, at the solution of which one may now only guess. In experiments upon retroactive inhibition, connected and meaningful materials have been almost never studied and to what extent one is justified in expecting from them results which are directly comparable to those yielded by disparate series of items is unknown. Our results raise, however, two general questions concerning inhibition: (1) whether similarity is the major condition of its appearance, as the transfer theory indicates, and (2) what constitutes similarity, in the context of retroaction experiments. These are not new problems. They have been present in most of the previous work. Our results merely serve to extend them and to make them still more puzzling.

The fact that nonsense syllables are as potent to inhibit poetry as is more poetry of the same kind is a difficult one for the transfer theory as that theory is usually stated, since, if transfer is at the basis of retroaction, similarity and inhibition should, over a wide range, be positively correlated. It is probable that relative degrees of organization must be considered in the theory.

It may be that relative degrees of organization of the original and interpolated materials are very important in determining amount of retroactive inhibition and that, when both materials possess a fairly high degree of internal organization, as is probably the case when both are sections of poetry, retroaction is, other things being equal, at a minimum. It may be, further, that a low degree of organization in the interpolated material, for example, permits a more ready confusion between it and the original material and hence a greater amount of inhibition. That is, degrees of organization may constitute a factor coordinate with similarity in an interpretation of the transfer theory. In this experiment the nonsense syllable lists were doubtless less well organized into a series than was the interpolated poetry and, as a result, they may have transferred more readily. If we score the nonsense syllables in terms of number correctly recalled and in correct position, the mean scores become less. This indicates a loose internal organization, although such proof is scarcely needed to show that syllable lists do not have as closely knit a pattern, even when well learned, as does poetry. The factor of degree of organization may have, in the present experiment, obscured differences produced by similarity.[21]

On the other hand, the perseveration theory is favored by this result. It is an essential part of this theory that the important condition of retroactive inhibition is the diminution or complete blocking of perseveration by the interpolated activity. The crucial aspect of this activity should be its intensity, while its similarity to the original activity should be inconsequential. According to this theory, therefore, the interpolated learning of nonsense syllables might be expected to exercise an inhibitory effect fully comparable to that produced by the interpolated learning of poetry. It is believed, however, that before this apparent contradiction of the transfer theory and support of perseveration may be given adequate evaluation, both theories, and more particularly the transfer theory, must be more carefully analyzed. Until more experimental work, directed at such an analysis, has been done, theorizing is futile.

CONCLUSIONS

(1) When a section of the same poem as the one used for original learning is employed as interpolated material, small amounts of retroactive inhibition occur in recall after 15 min. Susceptibility to inhibition is considerably greater after 7 days than after 15 min., and is enhanced when there is no intermediate recall after 15 min.

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[21] We are indebted to Dr. Arthur W. Melton, of Yale University, for the suggestion that differences in the organization of the materials may obscure differences in inhibition resulting from similarity.

(2) The use of nonsense syllables as material for interpolated learning produces roughly the same amount of inhibition as does the interpolation of poetry. This fact, as it stands, favors the perseveration theory and contradicts the expectation from the usual statements of the transfer theory. Before, however, the results can be confidently interpreted, both theories require further analysis.

(3) The fact that poetry is susceptible to a decrement from interpolated learning increases the generality of this decrement-phenomenon and adds support to the theory that retroactive inhibition is a major condition of forgetting.

THE AFTER-EFFECT OF VISUALLY OBSERVED MOVEMENT

By Louise Ward Gates, Brown University*

The present paper records the results of certain experiments upon the after-effect of visually observed movement.[1] After-effects of certain forms of visually observed movement have long been noted, especially those which give the appearance of movement in the opposite direction, or in some other way opposed to the previously observed stimulus-movement.

HISTORY

The experimental investigation of this phenomenon has been undertaken by physicists, physiologists, and psychologists. The history of this work from the first observation by Aristotle up to 1911 has been given by A. Wohlgemuth.[2] This author summarizes the work of many other investigators.[3]

The most important work done upon this problem since 1911, the date of the publication of Wohlgemuth's monograph, may be summarized as follows:

Wohlgemuth's comprehensive investigation included the repetition of many previous experiments and the confirmation in general of the after-effect of "apparent movement in the opposite direction."[4] It included also some 34 additional experi-

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[1] The writer wishes to express her gratitude to Professor Leonard Carmichael of the Psychological Laboratory of Brown University under whose guidance this investigation was carried out.

[2] A. Wohlgemuth, On the after-effect of seen movement, *Brit. J. Psychol., Monog. Suppl.,* 1, 1911, 1-117.

[3] J. Plateau, Vierte Notiz über eine neue sonderbare Anwendung des Eindrücke auf der Netzhaut, *Pogg. Annalen,* 80, 1850, 287-292; H. K. Müller, Die Beobachtung von Tiefeneffekten bei die binokularen Bewegungnachbildern, *Zsch. f. Sinnesphysiol.* 59, 1928, 157-165; J. J. Oppel, Neue Beobachtungen und Versuche über eine eigentümliche noch wenig bekannte Reaktionstätigkeit des menschlichen Auges, *Pogg. Annalen,* 99, 1856, 540-561; V. Dvorak, Versuche über die Nachbilder von Reizveränderungen, *Ber. der Wien. Akad. der Wiss.,* 61, 1871, 257-262; *Helmholtz's Treatise on Physiological Optics,* trans. 3rd ed. by J. P. C. Southall, III, 1925, 248, 270-281; S. P. Thompson, Optical illusions of motion, *Brain,* 3, 1880, 289-298; Optical illusions of motion, *Pop. Sci. Mo.,* 18, 1881, 519-526; H. P. Bowditch and G. S. Hall, Optical illusions of motion, *J. Physiol.,* 3, 1881, 296-301; E. Budde, Ueber metakinetische Scheinbewegungen und über die Wahrnehmung von Bewegungen, *Arch. f. Anat. und Physiol.* (Dubois Reymond), 1884, 127-152; S. Exner, Einige Beobachtungen über Bewegungsnachbilder, *Centralbl. f. Physiol.,* 1, 1888, 135-140; Über optische Bewegungsempfindungen, *Biol. Centralbl.,* 8, 1888, No. 14; A. Borschke and L. Hescheles, Über Bewegungsnachbilder, *Zsch. f. Psychol.,* 27, 1902, 387-398; A. von Szily, Bewegungsnachbild und Bewegungskontrast, *ibid.,* 38, 1905, 81-154; Zum Studium des Bewegungsnachbildes, *Zsch. f. Sinnesphysiol.,* 42, 1908, 109-114; T. Kinoshita, Zur Kenntniss der negativen Bewegungsnachbilder, *ibid.,* 43, 1909, 420-433; Über die Dauer des negativen Bewegungsnachbildes, *ibid.,* 434-442.

[4] Wohlgemuth, *op. cit.,* 27.

ments, pertaining to the influence of such factors as brightness, extent, direction, and velocity of the stimulus-movement, and the portion of the visual field stimulated, upon the after-effect. An "apparatus of moving fields" was devised, which consisted of two horizontal parallel-stripe fields, permitting simultaneous presentation of two vertical movements in opposite directions. With the spiral disk Wohlgemuth found that colors of the same brightness produced the same after-effect; that green and yellow spirals gave a marked after-effect in dim as well as in bright light, showing the equal importance of rods and cones; and that the after-effect resulted even when the area of objective movement was nearly zero. An elaborate network of subcortical and cortical movement centers was suggested to explain the phenomenon.

Kinoshita's study of the duration of the movement after-effect was extended by Takei,[5] who found this feature of the effect to be definitely related to the color of the objective movement as well as to the speed, direction, and intensity of light in that movement. Using 8 colors of equal chroma and brilliance, he obtained the longest duration with yellow, and the shortest with violet, a result suggesting that the intrinsically more brilliant color may produce the longer duration.

Hunter found that the after-effect varied with the stimulus-object.[6] On the basis of this and other observations he maintained, therefore, that a single explanation cannot be given for all instances of the effect, but that the three following factors, variously combined, explain the several phenomena: (1) eye-muscle strain, due to the reflex tendency of the eye to follow the movement, (2) suggestion, an associative factor, and (3) a retinal factor, probably in the form of fading after-images on the retina. Certain observations which led to the postulation of these three factors were: the faintness of the phenomenon when observed in the unstimulated eye (due to the absence of a retinal factor), the voluntary control of the effect (due to association), and the inability of the subject to secure an after-effect after being trained to control it (due to habits of eye-strain set up in the process of control). The phenomenon of the spiral disk he considered, on the other hand, to be predominantly retinal in origin, since no transfer of the effect to the unstimulated eye took place, and since the observation of superimposed opposite movements yielded a normal and intense after-effect. The retinal factor is probably a streaming phenomenon moving in the opposite direction to that of the stimulus-movement.[7] Hunter, with his factor of suggestion, provides the interpretation which his retinal theory demands if it is to explain how after-images are seen as movement in a specific direction.

Wohlgemuth's statement that "no after-effect is produced by an objective movement occupying the whole visual field" has been refuted by Thalman.[8] Under conditions, at first identical with and later similar to Wohlgemuth's, he obtained a normal opposite after-effect. The conditioning factor in this case was the duration of the stimulus-movement and not the position of fixation or rate of objective movement.

In a brief summary Ehrenstein[9] attempted to refute Hunter and to support Szily with positive results, which showed transfer of the effect, obtained with a logarithmic spiral, to the unstimulated eye. He also found the duration of the effect to be dependent upon the duration of the stimulus-movement.

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[5] T. Takei, Über die Dauer des negativen farbigen Bewegungsnachbildes, *Zscb. f. Sinnesphysiol.*, 47, 1913, 377-381.

[6] W. S. Hunter, The after-effect of visual motion, *Psychol. Rev.*, 21, 1914, 245-277.

[7] Hunter, Retinal factors in visual after-movement, *ibid.*, 22, 1915, 479-489.

[8] W. A. Thalman, The after-effect of seen movement when the whole visual field is filled by a moving stimulus, this JOURNAL, 32, 1921, 429-441.

[9] W. Ehrenstein, Versuche über die Beziehungen zwischen Bewegungs- und Gestaltwahrenehmungen, *Zscb. f. Psychol.*, 96, 1925, 305-352.

Granit found a complex relation between the extent of the stimulus-movement and the duration of the effect.[10] When the extent was decreased by changing the size of the disk the duration decreased, but when it was decreased by increasing the observation-distance the duration increased slightly. Granit concluded that inhibitory activity of the cones upon the activity of the rods explains the phenomenon.

The following year Granit furnished further support for this view.[11] He found the optimal visual angle for the effect to be 2-4° involving an area which was practically rod-free. Moreover, the greatest decrease in duration occurred with a very slight degree of dark-adaptation, a result showing that the rods exerted their greatest inhibitory influence upon the cones without, however, completely overcoming them.

Fuchs has demonstrated the influence upon the movement after-effect of (1) the projection-figure, and (2) the projection ground.[12] Certain stimulus-figures, such as a picture of human beings walking, lent themselves particularly to the production of the phenomenon. A radii disk, projected after observing the spiral disk, had a tendency to show the radii as curved slightly. Fuchs concluded that the effect depends upon (1) a peripheral factor determining the direction of the stimulus-movement, and (2) a central, or perceptual factor, consisting in the "structure" of the projection-ground, and the influence of the observer's past experience with this projection-ground.

Müller observed certain depth effects as introspectively characteristic of the after-effect when projected either on a stationary, or moving, projection ground.[13]

One form of 'motor theory' based on the activity of the accommodation muscles has recently been proposed by Leiri to explain the spiral after-effect.[14] The phenomena of apparent increase and decrease in size of a visual object, known as macropsia and micropsia respectively, take place normally when a change in accommodation is produced by the approach or withdrawal of an object. In the case of the spiral the apparent approach produces, according to Leiri, an enlarged retinal image which becomes a stimulus for a reflex increase in accommodation. When the objective rotation ceases this stimulation is gone and the reflex activity stops. There continues, however, an "accommodation-innervation" which has been compensating for the reflex activity during rotation and which now produces the after-effect until the eye becomes adjusted to the new degree of accommodation. The theory, if correct, explains a transfer of the effect to the unstimulated eye, since the two eyes are closely associated in the act of accommodation.

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[10] R. Granit, Über eine Hemmung der Zapfenfunktion durch Stäbchenerregung beim Bewegungsnachbild, *Zsch. f. Sinnesphysiol.*, 58, 1927, 95-110.

[11] Granit, On inhibition in the after-effect of seen movement, *Brit. J. Psychol.*, 19, 1928, 147-157.

[12] F. Fuchs, Experimentelle Studien über das Bewegungsnachbild, *Zsch. f. Psychol.*, 106, 1928, 267-315.

[13] Müller, *op. cit.*

[14] F. Leiri, Über optokinetische hervorgerufene visuelle Täuschungen, *Gr. Arch. f. Ophth.*, 119, 1928, 719-732; Gibt es einen extralentikularen Akkommodationsmechanismus in Auge?, *ibid.*, 121, 1928, 213-224.

In an extensive treatise Durup[15] has surveyed the more important experimental findings in the light of theories proposed to explain them, reiterating Hunter's view that independent explanations must be given for the various forms of the phenomenon. The author confirms previous descriptions of the Plateau spiral effect under the usual conditions of observation in which fixation is held on the center of the disk. This author's report of the transfer of this effect to the unstimulated eye with particular stimulus-distances adds one more to the many conflicting results on this problem. A neuro-muscular factor is suggested to explain the central processes, and retinal processes to explain the peripheral phenomenon.

Grindley[16] refutes Granit's claim that the inhibitory relation between the rods and cones gives rise to the movement-after-effect. He reasons that since red stimulates chiefly cones and blue chiefly rods, color should then affect the after-effect as measured by its duration. Each of several colors was used as stimulus (1) for a rod-free area and (2) for a larger area. According to Granit the ratios of the durations in the two cases should be greatest for red, but the differences were negligible, and Granit's results are interpreted as possibly due to the change in the perceptual configuration accompanying the change in the observation-distance.

An investigation by Sturm[17] upon the direction of the after-effect movement found its starting-point in Hunter's statement that under certain experimental conditions "it is the apparent direction of the real movement and not the actual direction which determines the after-movement."[18] Hunter had found that a series of oblique stripes placed across the opening in front of the movement gave a perception of the oblique movement during rotation and an opposite oblique after-movement. In order to test the 'purity' of the after-effect, *i.e.* its independence from the form of the projection-field, Sturm added three new control-situations, and four types of effect resulted: (1) reversal of the perceived movement, (2) opposite of the primary movement, (3) resultant of the two above, and (4) alternate. Two principal perceptual factors are proposed to explain these facts: (1) the "field" factor which functions when the presence or absence of the oblique grating in the projection-field affects the effect, and (2) the "reversal" factor which explains results affected by a grating during movement.

Method and Procedure

The present investigation was undertaken in an effort to study further some aspects of the observed after-effects of spiral movement. In order experimentally to answer certain questions a new form of presentation was chosen for detailed study. The stimulus was two identical spiral disks rotating at a known speed in opposite directions. These spirals were so arranged that they could be viewed together or singly by one eye, or together or singly by both eyes. A more detailed description of the apparatus and procedure follows.

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[15] G. Durup, Le problème des impressions de mouvement consecutives d'ordre visuel, *Année psychol.*, 29, 1928, 1-56.

[16] G. C. Grindley, The relation between the rod and cone mechanisms in the after-effect of seen movement, *J. Physiol.*, 69, 1930, 53-59.

[17] M. Sturm, A study of the direction in the movement-after-image, *Psychol. Forsch.*, 14, 1931, 269-293.

[18] W. S. Hunter, Retinal factors in visual after-movement, *Psychol. Rev.*, 22, 1915, 257.

Apparatus. Two white disks, 8 in. diam., upon which were drawn identical black spiral stripes, were mounted upon a two-spindle reducing apparatus and rotated by a motor. The centers of the two disks were approximately 11 in. apart. A black cardboard screen, placed behind the disks, concealed the apparatus and served as background for the disks. The disks themselves were the projection-ground for the after-effect, as this situation necessitated no shift in fixation.

O viewed the disks through small cardboard tubes from a distance of approximately 5 ft. These tubes prevented the two monocular fields of vision from overlapping. They were held at the level of *O*'s eyes by metal clamps and supporting rods. *O* then maintained his gaze halfway between the two disks, both during the objective movement and after its cessation.

A second set-up, adopted during the course of the experiment, simplified fixation but destroyed the two mutually exclusive visual fields which the first set-up had provided. The tubes were discarded and in their place a large rectangular box, extending from *O* to the disks, was used. The two ends of the box were open. Its dimensions were such that at one end it coincided with the cardboard background to which it was attached, while at the other end sat *O*. The sides were made of uniform black cloth, of a dull finish to prevent reflection from the 50-watt lamp which was clamped in a reflector to the inside of *O*'s end of the box. Two small cardboard screens (11 x 18 in.) were hinged to the inside of the ceiling of the box and could be lowered to a vertical position to conceal the disks. A single disk could thus be presented as stimulus. A tiny white cardboard cross, pasted on the center of the black background, served as a fixation-point.

A suitable stopping device consisted of a single lever so arranged that its operation both broke the electric circuit and arrested instantly the rotation of the disks.

The experiment was conducted in a dark room and light was furnished by a single 50-watt lamp suspended from the ceiling directly behind *O*.

The 90 *Os* were instructors in psychology and men and women graduate and undergraduate students in Brown University, all of whom were taking work in the department. All whose records were used in the study were naïve with respect to the problem under consideration.

Procedure. The experiment consisted of three parts. The first two were chiefly for purposes of control and comparison with the third. Common to all three situations were the identical fixation-point and the resulting use of indirect vision exclusively. These conditions made possible a direct comparison of the after-effects as they occurred in the three parts respectively.

In Part I of the experiment *O* was to view the right disk in clockwise rotation, monocularly, with the right eye. He did so by fixating the 'center point' with that eye, keeping the left eye closed. Similarly in Part II he was to view the left disk in counter-clockwise rotation, with the left eye. In both experiments a single disk was observed by a single eye. In Part III the two disks were viewed simultaneously by the two eyes respectively. The central fixation-point was held as before and the disks were rotated in their respective directions.

O viewed the rotating disk or disks for a period of 45 sec. He was instructed to report as accurately and completely as possible what he saw with respect to the disk during that time. Upon cessation of the rotary movement he was to maintain his fixation and continue to describe what he saw until all was normal, *i.e.,* as it had been before the disks were rotated. Verbatim reports were made of *O*'s observations during and immediately after the trial.

The speed and duration of the rotary movement were held constant throughout the experiment. The speed (62 rotations of the disk per min.) and the 45-sec. duration were fixed upon after preliminary observations which showed them to be most favorable to the production of a definite after-effect. The duration was selected also with a view to preventing undue fatigue of the eyes. A stop-watch was used for timing the movement.

RESULTS

The following results of the experiment are based upon the individual verbatim reports referred to in the above section.

The results from the first two experimental situations, in which a single disk was viewed indirectly by a single eye, extend to indirect vision the findings of previous investigators, who had employed fixation on the center of the disk itself. Thus, observation of the right disk produced essentially an experience of withdrawal and shrinking of spiral coils, which in the after-effect changed to an approach and expansion of these coils. In corresponding manner the left disk, rotated in the opposite direction, gave an appearance of approach during movement and withdrawal in the after-effect. It may be seen from Table I that 79.4% of the *O*s reported an after-effect for the right-eye-right-disk-situation, and 70% for the left-eye-left-disk-situation. Second trials, given to *O*s who had failed to obtain the after-effect, raised these results to 85% and 79% respectively. (See Table I.)

TABLE I

NUMBER AND PERCENTAGES OF OBSERVERS OBTAINING AFTER-EFFECT

The figures are numbers and percentages of Os who served in the given experimental set-up. R = situation of clockwise rotation for right eye only. L = situation of counterclockwise rotation for left eye only. RL = situation combining R and L. A = set-up A (53 Os). B = set-up B (37 Os).

	Excluding second trials						Including second trials					
	R	R%	L	L%	RL	RL%	R	R%	L	L%	RL	RL%
A	34	64.2	27	50.9	39	73.6	37	69	32	60	44	83
B	35	94.6	33	89.2	36	94.6	37	100	36	98	37	100
Av.		79.4		70.0		84.1		85		79		91.5

It seems from the present study, however, that the movement experience, both during rotation and in the after-effect, is more complex than has previously been supposed. Rather than a simple movement and its reversal, there really occurred movement experiences which varied greatly among *O*s and between different reports of the same *O*. Four types of movement were reported which, although they usually occurred in combination, were nevertheless independent and isolable. A few excerpts from individual reports will serve to illustrate these various types:

(1) The spiral rotated to the right or left, according to the direction of rotation.

"The lines go around to the right."

(2) The entire disk, or the spiral lines within the disk, seemed to shrink (or expand).

"It seems to be growing larger now."
"The disk stopped dead still; suddenly got smaller and smaller."

(3) The disk was seen to move away from (or toward) *O*.

"It is coming way out (toward me); it is going to hit me; it comes nearer and nearer and nearer."
"The disk seems to come right up toward my eye."

(4) The spiral lines moved toward the center (or periphery) of the disk.

"Opposite effect to that of the other eye; the spirals are going in toward the center."
"The circles went in toward the center of the disk."

TABLE II

FREQUENCY OF OCCURRENCE OF THE FOUR TYPES OF MOVEMENT DURING AND AFTER ROTATION

The figures are percentages of *O*s obtaining an after-effect. R = situation of clockwise rotation for right eye only. L = situation of counter-clockwise rotation for left eye only. RL = situation combining R and L. m = perceived movement. ae = perceived after-effect.

	Rm	Rae	Lm	Lae	RLm	RLae	Total m	Total ae
Rotary	47.4	14.5	41.0	16.8	35.8	5.3	41.3	11.3
Expansion	14.6	31.7	29.8	15.3	8.4	8.2	17.6	16.8
Approach	59.2	85.4	53.6	78.8	66.6	93.3	59.1	85.8
Toward periph.	51.3	17.3	51.5	12.0	21.6	5.4	11.5	11.7

The report of a single one of these types of movement was taken as the criterion of the existence of the after-effect, a fact which should be remembered when consulting the tables. The relative frequency with which the four types of movement were reported, during movement and in the after-effect, may be seen from Table II. It will be noted that the withdrawal and approach movements of the disk were most frequently noted, and may be considered as particularly characteristic of the after-effect. Eighty-six percentage of the *O*s who obtained an after-effect reported this type of movement in contrast to the 11%, 16%, and 11% who reported the other three types.

The interesting results of the study from a theoretical standpoint were obtained, however, from Part III, in which the two quite distinct and independent visual fields of the two eyes were combined into what we may call a monocular-binocular situation, in Set-up A. If the normal after-effects of the two eyes continued to occur under these conditions, the fact should have important consequences for a theoretical explanation of the phenomenon.

It was found that during rotation each of the two disks retained its characteristic types of movement, and that at the cessation of the rotation the usual after-effects resulted simultaneously. It may be seen from Table I that 74% of the *Os* obtained this double after-effect and that this figure was raised to 83% by adding second trials. The following excerpts from individual reports illustrate the nature of this after-effect:

> "The right is coming toward me, and the left is going away."
>
> "I have a strong sensation of the right coming toward me and the left fading away; it lasted longer with the two eyes."
>
> "The right disk is coming out of the distance and the left is going off into the distance."

This result held also in Set-up B, in which situation the two visual fields overlapped and both disks were viewed by both eyes simultaneously. Ninety-five percentage of the *Os* obtained this after-effect, or 100% if second trials are considered.

The number of *Os* who obtained this double after-effect, as presented in Table I, suggests that the contrast of movements in this situation heightened the likelihood of an after-effect. The after-effect was obtained by a larger proportion of *Os* in this situation than in either of the two monocular situations. Moreover, certain *Os* obtained this effect although they had failed to obtain either of the monocular effects. It is true, however, that practice in observation and report may influence this finding. The influence of the two visual fields upon each other appears in the *Os'* descriptions, since such reports were made as: "The two disks move in opposite directions," and "The right disk comes toward me, but the left goes away." In this situation as well as the first two parts, the tables show that a greater proportion obtained the after-effect under the conditions of Set-up B than under those of Set-up A. This is probably explained by the greater ease of fixation in this situation.

The after-effect has been traditionally known as an opposite or negative after-image of movement. Hunter considered certain of his experimental data according as they showed an "s.-a.-m." or after-movement in the same direction as the primary movement, or an "op.-a.-m." or after-movement in the opposite direction to that of the primary movement. Such reversals were also noted in this study. Certain *Os* reported: "I get the opposite effect." A comparison, however, of the number of types of movement reported during rotation and in the after-effect suggests that the after-effect is more than a simple contrast phenomenon. Whereas during rotation a combination of types of movement usually occurred, the after-effect was more frequently dominated by a single movement, the approach or withdrawal of the disk. This fact is brought out to some extent in Table II which indicates, for example, that in the RLae-situation 93% of the *O*s reported approach and withdrawal, while only 5–8% reported the other three types of movement. During the RLm-situation less importance could be given to a single type of movement. The RL-situation shows this fact most clearly. In this case the after-affect nearly always consisted of the single type of movement, the approach and withdrawal of the disk.

The introspective report of the nature of the after-effect has already been shown to vary with the individual. Depth effects, for example, were observed by some *O*s and not by others. Rough estimations on duration and intensity also showed great individual differences. Duration varied from a momentary perseveration of the experience of rotation to an after-effect of over a minute. The intensity varied from an almost imperceptible change of movement to a sudden and striking effect which completely surprised the *O*.

A small number of supplementary observations were made in which a slight modification of Set-up B permitted both disks to be viewed in simultaneous rotation by a single eye. Under these conditions all *O*s obtained an after-effect similar to that obtained in Part III of the main experiment. The opposition of the two movements was as marked in this case as when both eyes were used. These results show that the normal after-effect results when two regions of a single retina are stimulated by spatially separated and oppositely rotating spirals.

SUMMARY

(1) An after-effect of movement was demonstrated on a rotating spiral disk under conditions of monocular and indirect vision.

(2) Four types of movement were variously reported, any one of which served as criterion for existence of this after-effect. These may be characterized as (1) rotary movement, (2) expansion and contraction, (3) approach and withdrawal, and (4) movement of the spiral lines toward the periphery or center of the disk.

(3) The after-movement was predominantly a movement toward or away from the *O* (type 3 above), depending upon the direction of rotation of the disk observed. The after-affect most frequently consisted of this single

type of movement in comparison to the experience of rotation where two or more types of movement were usually observed.

(4) Simultaneous observation by the two eyes respectively of two identical spiral disks, spatially separated and rotating in opposite directions, was followed by an after-effect of the opposing movements of the two disks, one toward and one away from the observer. This after-effect occurred in certain instances in spite of the *O*'s previous failure to obtain both, or even a single, monocular after-effect.

(5) This binocular after-effect occurred also when the two visual fields were allowed to overlap.

(6) Individual differences were noted with respect to the intensity and duration of the after-effect, and its introspective nature.

(7) Simultaneous observation by a single eye of the two rotating spiral disks was followed by the after-effect observed in Part III of the main experiment.

THEORETICAL DISCUSSION AND CONCLUSIONS

Many theories have been formulated to explain the movement-after-effect. Critical reviews of these theories appear in the writings of Wohlgemuth, Hunter, and Durup, and need not be included here. The present writer will attempt simply to evaluate those theories which have either been shown inadequate, or have been supported and strengthened by the results of the present investigation, and on this experimental basis suggest what seems to be the most satisfactory explanation of the spiral illusion.

Previous theories may be grouped as (1) Central Theories, including those which hold the phenomenon to be due primarily to processes within the central nervous system, and (2) Peripheral Theories, which hold the phenomenon to be predominantly retinal or muscular in origin.

Hunter's explanation in terms of retinal activity was formulated principally on the basis of the author's inability to demonstrate the transfer of the after-effect to the unstimulated eye, and the occurrence of a normal after-effect when two opposite movements viewed monocularly were superimposed. Certain facts, however, seem to render such a theory inadequate for the spiral phenomenon. In the first place, Ehrenstein and Durup have since demonstrated the denied transfer.[19] Secondly, the many possible types of movement shown by the present study to characterize the after-effect virtually turn the theory into a central theory, since in its present form it fails to explain, except by an associative factor, how the fading retinal after-images may be interpreted now as movement away from *O*, and now as shrinking of the disk. However, such a simple central view is apparently not in accord with the facts early demonstrated by Bowditch and Hall[20] and since confirmed by most investigators that the phenomenon is not only striking to the *O*, but occurs in spite of the distraction of his attention from the stimulus movement.

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[19] W. Ehrenstein, Versuche über die Beziehungen zwischen Bewegungs- und Gestaltwahrnehmungen, *Zsch. f. Psychol.*, 96, 1925, 305-352; G. Durup, Le problème des impressions de mouvement consecutives d'ordre visuel, *Année psychol.*, 29, 1928, 1-56.

From the results of the present study it seems that a more satisfactory explanation of the spiral phenomenon is to be found in the activity of the visual musculature during and immediately after observation of the rotary movement. Of muscular theories previously formulated those based upon the coördinate action of the extrinsic eye muscles have long been adversely criticized because they fail to explain the basic fact of the perception of radial movement, which movement demands that coördinate movements occur in all directions simultaneously.

This difficulty has been recognized by Leiri who, therefore, bases his explanation on the delicate processes of the accommodation mechanism rather than on changes in the larger musculature of the external eye muscles. But Leiri, too, fails to explain the two simultaneous opposite after-effects of the two eyes which the present experiment demonstrates, since his "voluntary accommodation innervation" involves a close coördination between the activity of the two eyes. He seems not to have been acquainted with Hunter's demonstration of the after-effect when two oppositely rotating spirals were superimposed, for such a finding involves definitely the independent activity of the two eyes.

In this paper it may be very tentatively suggested that the mechanism by which the illusion occurs seems in some respects to be basically of the nature of the effector events posited, upon the basis of experimental work in various psychophysiological fields, by such writers as Münsterberg, Delabarre, Washburn, Holt, and Langfeld. As Langfeld clearly says in discussing the general nature of perception and motor phenomena, perception takes place only upon the occurrence of some motor response "incipient" if not overt. These responses, while not ordinarily a part of direct awareness, as such, are nevertheless according to this theory indispensable antecedent events for every perceptual experience. In the narrower field of visual perception Langfeld answers the criticism raised against eye-movements as an explanation of perceptual illusions, by pointing out that such a theory as he holds "does not call for actual movement, but merely a specific change in the efferent system."[21] It is thus possible, he continues, "that a change in muscle tonus produced by the antagonism of the muscles involved is a physiological cause of such phenomena."[22] Langfeld suggests further that the motor responses bringing out the phenomenon need not be restricted to the muscles of the eye, but, by a process of conditioning, may take place in any muscles of the body.

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[20] H. P. Bowditch and G. S. Hall, Optical illusions of motion, *J. Physiol.*, 3, 1881, 296-301.

Returning to the spiral phenomenon, we see how such a view suggests an explanation of the experimental findings of this study. During the observation of the objective movement the continued stimulation of the eyes by the rotation of the disks sets up certain very definite changes in the tonus of the eye-muscles. These changes probably occur both in the external muscles, and, more especially, in the internal muscular systems of the mechanisms of accommodation and convergence. Upon cessation of the movement (in this experiment after 45 sec.), these well established changes are completely upset. The stimulus-situation which created them is suddenly removed, and the return of the muscles involved to their normal state at once begins. Those muscles in which an increase in tonus had occurred now gradually relax, those in which the tonus was correspondingly decreased now return to their former state of greater tonicity. This adjustment, of course, involves activity of the central nervous system. The result perceptually is an experience, only part of which is determined by activities initiated in the retina. During objective movement the changes reported might be those accompanying apparent decrease in size and withdrawal and then, for example, the return in certain instances seemed like a complete reversal, a swelling out toward the *O*, while in certain other instances the return to balance led to other introspectively reported phenomena.

The simultaneous occurrence of the two opposite after-effects in the two eyes is thus seen as possibly taking place because the two eyes are presented with different stimulus-situations. The tonus of the muscles under these conditions is affected independently of the muscles of the other eye and of the other muscles in the same eye, and the return of the two sets of muscles to normal thus gives rise to the two opposite experiences. In the light of this view it is not surprising that occasionally the simultaneous occurrence of the two opposite muscular events is accompanied by a strong opposition in the binocular after-effect, even when the changes of tonus in the individual eye had been too slight to produce the monocular after-effect.

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[21] H. S. Langfeld, A response interpretation of consciousness, *Psychol. Rev.*, 38, 1931, 101.

[22] *Loc. cit.*

In the same manner as described above it is possible for a single eye, stimulated by two different stimulus-movements, which affect respectively the tonicity of different muscles, to perceive simultaneously two opposite experiences accompanying the return to normal of the two patterns of tonic activity. This view accounts for the supplementary experimental finding that either eye, observing both disks, obtained an after-effect of both disks moving in opposite directions.

The theory accounts also for a previous experimental finding, namely that the form of the after-effect depends to some extent upon the ground on which it is projected. The phenomenon is not so much one of a definite movement as it is one of expansion or contraction of the visual field. Therefore, whatever stimulus-object is presented to the 'unbalanced' eye may cause a definite experience, in part dependent upon the after-effect in the optic mechanism and in part due to the present stimulus-situation.

In the view of the present writer, therefore, it may be said that a motor theory of the sort outlined above is adequate to explain the facts determined in the present experimental study of the after-effects of visually observed movement.

COMPARATIVE ACCURACY IN THE LOCALIZATION OF CUTANEOUS PRESSURE AND PAIN

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Recent attempts which have been made to determine the comparative accuracy of cutaneous localizations of pressure and of pain have resulted in an apparent deadlock. On the one hand, Ponzo[1] published experimental results which indicate that the average error of localization for pain is slightly smaller than that for pressure. In connection with this finding, he pointed out that it is biologically more essential that an organism be able to localize accurately injurious than non-injurious stimulations. On the other hand, Von Skramlik[2] and Mayer[3] have more recently presented experimental results which are inconsistent with those of Ponzo. According to their reports, stimulations of pain were localized with a lower degree of accuracy than were stimulations of pressure. Although the tables of Ponzo revealed only a small degree of difference in the errors of localization for pain and pressure, the results of Von Skramlik and Mayer indicated that the error for pain is considerably larger than that for pressure.

The controversial discussion which followed the appearance of these publications was intended to provide an explanation for this contradiction in experimental findings. Several points of difference, such as difference in method and in mode of stimulation have been specified as possible sources for this inconsistency in experimental results. Ponzo used the method to which the name of Weber is attached. The *O* kept his eyes closed throughout the trial and localized the point of stimulation by means of a pointed instrument. He was permitted to move the instrument about on the skin until he was satisfied with the localization. Von Skramlik and Mayer, however, used the method of Volkmann, which required the *O* to open the eyes after the stimulation in order that the localization might be made with the aid of vision.

In order to determine whether the contradiction in these experimental results

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[1] M. Ponzo, Recherches sur la localisation des sensations tactiles et dolorifiques, *Arch. ital. de biol.*, 55, 1911, 1.

[2] E. von Skramlik, Lokalisation der Empfindungen bei den niederen Sinnen, *Zsch. f. Sinnesphysiol.*, 56, 1925, 135 ff.

[3] B. Mayer, Beitrag zur Lokalisation von Schmerzempfindungen, *ibid.*, 56, 1925, 141-153.

actually was a function of difference in method, Mayer[4] performed additional experiments in which localizations of pain and of pressure were made by both of the methods just mentioned. Her results are in both cases in harmony with her original finding. The average error for pain localizations was larger in both cases, and in about the same degree in each case. She concluded that difference in method cannot be designated as the foundation for this conflict in results.

In all of the studies mentioned above, a hair mounted in a Von Frey esthesiometer was used in the stimulation of pressure. For the arousal of pain, Ponzo used a sharp metal needle, but Von Skramlik and Mayer used a thistle (*Stacheln von Carduus acanthoides*). Von Skramlik[5] asserted that a needle does not consistently arouse pure pain but a complex of pain and pressure sensations, and Mayer[6] cited Von Frey as authority for ruling out the needle as the instrument for the stimulation of pain. In her own work, Mayer found that the needle aroused pure pain in less than 20% of the stimulations. It is her belief, therefore, that Ponzo failed to excite pure pain, and that his *O*s were called upon to localize, for the most part, pressure-pain complexes, which Ponzo took to be pain. The controversy is thus reduced to a matter of applying the term pain to essentially different cutaneous qualities. In order to test this hypothesis, Mayer, in the same study in which she tested the possible influence of difference in method, required her *O*s (Dr. Strughold and herself) to decide into which of three classes of sensation each of the stimulations by the thistle (*Stachelborste*) belonged—(1) pure pressure, (2) pure pain, or (3) pressure-pain complex. Calculation of the average error for each of these classes revealed that the error of localization was largest for pain and smallest for pressure-pain complex. The average error for pure pressure was slightly larger than that for pressure-pain complex, but decidedly smaller than that for pure pain.

In view of these additional determinations, Mayer and Von Skramlik are apparently convinced that the conflict is really resolved, and that they have established the fact that the average error of localization is greater for pain than for pressure. However, it would seem desirable that these experiments be repeated, particularly in view of the fact that in her second study Mayer had only two *O*s, and that in the controversy with which we are here concerned Von Skramlik merely came forward with a brief polemical paper.

EXPERIMENT I

As a first step in the direction of making a contribution toward the clarification of certain points underlying this controversy, we made localization trials under conditions essentially like those in the last mentioned work of Mayer. A horse-hair, mounted in a Von Frey esthesiometer, was used in the stimulation of pressure. The hair had a tension strength of 1 gr./mm. Since the thistle which Mayer used in the stimulation of pain (*Stacheln von Carduus acanthoides*) was unavailable, we used instead a spine from the purple top thistle (*Carduus lanceolatus L*) which is commonly found in the eastern part of the United States. In her earlier work, Mayer also used a different thistle (*Onopordon bracteatum*).[7]

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[4] Mayer, Über die Ortsbestimmung von reinen Schmerzempfindungen, *ibid.*, 58, 1927, 32-37.

[5] Von Skramlik, Zur Frage der Lokalisation von Schmerzempfindungen, *ibid.*, 58, 1927, 28-31.

[6] Mayer, *op. cit.*, 1927, 34.

The stimulations were made on the back of the hand. Before making the trials, 10 ink spots were placed on the back of each hand at the points at which *E* expected to stimulate. The ink spots were placed neither over blood vessels nor at other points of prominence. In a first series of trials, the 10 ink spots were placed rather close together, within an area of about a square inch just posterior to the union of the index and middle fingers. We made series of ten trials alternately on the two hands, thus providing between series a period for partial recovery from fatigue. We alternated the kind of stimulation which was used at the beginning of successive sittings. At odd sittings, the esthesiometer was used in the first series of trials, and at even sittings the thistle was first used. There was regular alternation of type of stimulation after every 10 trials within the sitting. Localization was made with the aid of a pointed pencil. *O* was permitted to move the pencil to and fro on the skin until he had localized as accurately as possible. The eyes were kept closed throughout stimulation and localization periods. The localizing hand, provided with the pencil, was kept at a designated spot on the table, 15 in. from the hand upon which stimulation was to be made. The localizing hand was not moved from the resting position until the stimulating instrument had been withdrawn by *E*. In order to keep kinesthesis, in general, as nearly constant as possible, the *O* was required to keep both feet on the floor throughout the sitting.

Two of the 8 *O*s were members of the staff in psychology, and the other 6 were undergraduate majors in the department. A period of training in localization preceded the experiment proper.

With regard to the comparative adequacy of the two stimuli in arousing the specific qualities with which we are here concerned, it is clear that the hair is more satisfactory in that the stimulations made by it never resulted in complications with the other quality. These stimulations always produced simple pressure only. The thistle, however, frequently produced a pressure-pain complex—an experience in which the qualities of pressure and pain were combined. As already noted, the needle has been found to be unsatisfactory for the arousal of pain because it frequently arouses pressure. Von Frey designated the thistle as more satisfactory than the needle on the ground that in most of these stimulations no pressure was observed. Although the pressure component is greatly reduced by the use of the thistle, nevertheless our *O*s reported that usually a faint fine contact was experienced just a moment before the quality of pain appeared. However, when the pain quality appeared, it tended to mask the pressure component.

Moreover, the thistle did not at all times arouse a quality of pain of the same character. Certain pains were sharper, more lively and more stinging in character than others. Sometimes there was a suddenness in the manner in which the sensation of pain emerged to a full quality of clear pain; at other times, a number of graded changes were perceived as the quality became clearer and brighter. There was distinctly less variability in the character of the pressure qualities than in those of the pain qualities. With regard to those stimulations which resulted in pressure-pain complexes, the *O*s reported that as a general rule the two qualities were clearly distinguishable. Frequently an *O* reported incipient pain, specifying that this element was the brighter component in a pressure-pain complex. At other times, pain was reported as occurring along with a dull, dragging pressure. There were only a few instances in which the quality of pain and that of pressure were so intermingled that the *O* could not clearly distinguish between the two qualities in the complex.

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[7] Mayer, *op. cit.*, 1925, 143.

The average errors of localization for both kinds of stimulation—pressure and pain—are presented in Table I. The averages were determined from 50 trials with each stimulus on each hand.

The table shows that, for the most part, the average error of localization for pain is smaller than that for pressure. There are exceptions for 2 *O*s on the right hand and for a single *O* on the left hand. Thus, in 13 of the 16 instances, the average error for pain is smaller than that for pressure.

TABLE I

SHOWING THE AVERAGE ERROR OF LOCALIZATION IN MM. FOR PRESSURE STIMULATION BY THE HAIR OF A VON FREY ESTHESIOMETER AND FOR PAIN STIMULATION BY THE THISTLE ON THE BACK OF THE HAND, WHEN THE POINTS OF STIMULATION ARE CLOSE TOGETHER

	Left Hand		Right hand		Both hands combined	
O	Pressure	Pain	Pressure	Pain	Pressure	Pain
Ba	4.30	4.66	4.24	2.98	4.27	3.82
Be	7.10	4.73	3.63	3.10	5.36	3.91
Br	3.28	3.15	4.40	3.71	3.84	3.43
J	6.03	5.36	7.63	5.80	6.83	5.58
M	3.46	3.23	2.86	2.56	3.16	2.89
P	4.90	3.00	4.26	3.23	4.58	3.11
W	5.13	4.00	3.96	4.93	4.54	4.46
Z	3.66	3.30	3.13	3.35	3.39	3.32
Av.	4.73	3.92	4.26	3.70	4.49	3.81

When the averages of the two hands of each *O* are combined, the average error for pain is smaller in every case. Although the difference between the averages for pressure and for pain is small in a number of cases, it is nevertheless apparent that pain tends to be localized slightly more accurately than pressure.

EXPERIMENT II

In order to determine whether, in the foregoing work, the fact that the 10 ink spots were close together had any influence upon these results, we made a series of trials in which the 10 spots were more widely scattered over the back of the hand. It seemed possible that, in this experiment, the *O* may have developed the habit of localizing in a restricted area, and that differences in the magnitude of the two errors may have been minimized in consequence of the fact that the localizing instrument was placed regularly in the same limited field. Table II embodies the averages for the localizations with ink spots more scattered. Each average in this table was computed from 100 trials.

The table reveals that the average error of localization for pain is smaller than that for pressure at every point of comparison. Moreover, the difference, although again not great, is a matter of greater comparative significance. It is evident that the scattered arrangement of points of stimulation is more favorable to the demonstration of relative accuracy of localization of these two qualities.

TABLE II

Showing the Average Error of Localization in Mm. for Pressure Stimulation by the Hair of a Von Frey Esthesiometer and for Pain Stimulation by the Thistle on the Back of the Hand, When the Points of Stimulation are Scattered

	Left hand		Right hand		Both hands combined	
O	Pressure	Pain	Pressure	Pain	Pressure	Pain
Ba	5.44	3.94	3.90	3.20	4.67	3.57
Be	4.51	3.58	5.10	3.62	4.80	3.60
Br	4.75	3.08	4.79	3.41	4.77	3.24
J	4.26	3.00	4.42	3.24	4.34	3.12
M	3.80	3.58	3.90	3.48	3.85	3.53
P	4.04	3.10	4.58	3.60	4.31	3.35
W	5.20	4.22	6.72	4.17	5.96	4.19
Z	4.80	3.17	4.87	2.74	4.83	2.95
Av.	4.60	3.45	4.78	3.43	4.69	3.44

With regard to the comparative accuracy of localization on the right and left hands, we note that the *Os* tend to confirm Dallenbach's recent finding[8] that the difference is essentially negligible, and his postulate that one may safely predict the magnitude of the error of localization on one hand in terms of the results of trials made on the other. The slight discrepancies which appear in the table between localizations on the right and left hands of an *O* are distributed, indicating that certain *Os* were slightly more accurate in localizations on the right and others on the left hand.

EXPERIMENT III

In the foregoing experiments we followed the traditional procedure of using the Von Frey esthesiometer in the stimulation of pressure. For the stimulation of pain we used a thistle. In the present experiment we endeavored to determine in some degree the extent to which pressure sensations enter into the experiences which arise when stimulations are made by the thistle, and whether the pressure component in such stimulations differs with variation in intensity of application of the thistle.

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[8] K. M. Dallenbach, A comparative study of the error of localization on the finger-tips, this JOURNAL, 44, 1932, 327-331.

A series of 10 stimulations were made on each hand with heavy or maximal application of the thistle (application in which the thistle punctured the skin), then 10 light stimulations were made in such a manner that the thistle point made only a slight indentation on the skin surface. Experimentation continued until 250 localizations at each of these two intensities had been made. The error of localization was measured in the same manner as in the previous work. Again, stimulations were made alternately on the two hands in order to provide periods for partial recovery from fatigue. At successive sittings as well as in successive series in the same sitting, we alternated the type of stimulation. If heavy stimulation was used first in a given sitting, light stimulation was used first at the next sitting. After each trial the *O* was required to specify into which of the following three classes the quality of sensation belonged: (1) simple pressure, (2) pressure-pain complex, and (3) simple pain.

TABLE III

SHOWING THE NUMBER OF THE THREE CUTANEOUS QUALITIES (PRESSURE, PRESSURE-PAIN COMPLEX AND PAIN) WHICH WERE REPORTED IN 250 LIGHT AND 250 HEAVY STIMULATIONS WITH THE THISTLE ON THE BACK OF EACH HAND

O	Light stimulation						Heavy stimulation					
	Left hand			Right hand			Left hand			Right hand		
	Pr.	C.	Pa.	Pr.	C.	Pa.	Pr.	C.	Pa.	Pr.	C.	Pa.
Ba	206	39	5	194	52	4	1	31	218	1	20	229
Be	214	27	9	215	27	8	8	5	237	12	6	232
Br	196	49	5	200	46	4	1	19	230	0	10	240
J	204	45	1	210	37	3	8	20	222	13	18	219
M	178	60	12	171	63	16	1	44	205	1	40	209
P	184	54	12	185	46	19	9	23	218	4	16	230
W	191	53	6	199	49	2	13	35	202	7	34	209
Z	212	34	4	215	26	9	7	54	189	6	57	187
Total	1585	361	54	1589	346	65	48	231	1721	44	201	1755

Table III reveals the manner in which the 250 light and 250 heavy stimulations on each hand were classified by each of the 8 *O*s. It will be noted that more than 80% of the heavy applications resulted in simple pain. Light stimulations produced simple pressure more frequently than any other quality, but in not quite so large a percentage of cases as that in which heavy stimulation produced simple pain. It is, accordingly, not safe to assume that a thistle will arouse the same quality of sensation every time it is applied to the skin, even when the stimulations are made at about the same intensity. Approximately 15 to 20% of the stimulations, varying somewhat from *O* to *O*, resulted in pressure-pain complex.

The average errors for the three qualities—simple pressure, simple pain and pressure-pain complex—are presented in Table IV. Turning first in that table to the combined averages of the 8 *O*s, it will be noted that the average error for pressure-pain complex is smaller than that for either of the other qualities in three of the four points of comparison. To this extent we have confirmed the finding of Mayer, already referred to, that pressure-pain complexes are localized with a higher degree of accuracy than are either simple pain or simple pressure. However, in the fourth case in the combined averages, the average error for simple pain is the smallest. This result is positively inconsistent with the contentions of Mayer and Von Skramlik, who found the error for pain to be greatest of all. If we compare the average errors for the two qualities of simple pain and simple pressure, it will be seen that that for the former is less than that for the latter in every case. This result also runs counter to the trend of the work of Mayer and Von Skramlik.

TABLE IV

SHOWING THE AVERAGE ERRORS OF LOCALIZATION FOR THE THREE CUTANEOUS QUALITIES (PRESSURE, PRESSURE-PAIN COMPLEX AND PAIN) INTO WHICH 250 LIGHT AND 250 HEAVY STIMULATIONS WERE CLASSIFIED

	Light stimulation						Heavy stimulation					
O	Left hand			Right hand			Left hand			Right hand		
	Pr.	C.	Pa.	Pr.	C.	Pa.	Pr.	C.	Pa.	Pr.	C.	Pa.
Ba	5.02	4.48	3.40	4.04	4.30	3.25	8.00	5.25	4.75	2.00	5.45	3.70
Be	4.50	4.03	3.33	4.80	3.59	3.50	5.00	3.80	3.96	3.91	3.50	4.01
Br	3.99	3.04	2.80	4.95	3.76	4.25	6.00	2.94	3.15	—	5.80	3.52
J	4.52	3.06	2.00	4.97	4.24	4.33	4.25	4.75	4.11	4.46	4.66	4.01
M	4.17	3.46	4.25	3.61	4.33	3.62	5.00	3.59	3.56	4.00	4.10	3.54
P	4.21	3.64	4.25	4.10	3.50	4.42	3.44	3.60	3.80	3.00	3.87	3.50
W	4.76	4.28	5.33	6.57	5.18	10.00	4.46	2.77	3.96	7.71	7.05	5.95
Z	3.95	3.97	6.00	3.71	2.38	2.44	5.42	3.12	3.11	5.83	2.92	3.13
Av.	4.39	3.74	3.92	4.59	3.91	4.47	5.19	3.72	3.80	4.41	4.66	3.92

By comparing the average errors of the particular *O*, it will be noted that in eleven of the thirty-two points of comparison the average error for pressure is smaller than that for pain. To this extent this table contains results which are apparently out of harmony with the results in our other tables. However, Table IV needs to be studied in connection with Table III, which gives the number of trials upon which each average in Table IV is based. By referring to Table III, it will be found that wherever the error for pressure in Table IV is smaller than that for pain, the average of one or the other is determined from a very limited number of trials, sometimes from a single determination. The comparisons in these 11 cases are therefore not very reliable. In the other 21 points of comparison, however, the average error for pain is smaller than that for pressure. In these 21 points, the number of cases is sufficient, generally, for reliable comparison.

If cutaneous pain is more accurately localized than pressure, a larger number of perfect localizations, or localizations with zero error, might be expected to appear in the pain columns than in those for pressure. Accordingly, we determined the number of such localizations which fell to each of the three qualities. Table V shows the result of this enumeration.

Although there are as many or almost as many perfect localizations for pressure as for pain at several points of comparison for particular *O*s, nevertheless all of the *O*s taken together amassed a significantly greater number of localizations with no error in the pain than in the pressure stimulations. Since fewer perfect localizations fell in the pressure-pain complex columns, it may at first glance appear that this result is at odds with our finding that these localizations are generally made with higher accuracy than those of pressure in particular, and in general than those of pain also. However, reference to Table III will show that in this experiment relatively fewer pressure-pain complexes were aroused than experiences of simple pressure and simple pain. Thus, the table of perfect localizations is composed from a relatively large number of pain and pressure stimulations and from comparatively few stimulations of pressure-pain complex. By comparing the totals in Tables III and V, it will be observed that the 8 *O*s made 205 perfect localizations for pressure in a total of 3266 trials, 83 perfect localizations for pressure-pain complex in 1139 stimulations, and 298 perfect localizations for pain in 3595 trials. Thus, there was one perfect localization in about every 15 in the case of pressure stimulations, one in about every 13 in the case of pressure-pain complex, and one in every 12 in the case of pain. Again, the results indicate that the quality of pain has an advantage as compared with pressure in regard to the matter of accuracy of localization.

TABLE V

SHOWING THE NUMBER OF PERFECT LOCALIZATIONS (NO ERROR) WHICH FELL TO EACH OF THE THREE CUTANEOUS QUALITIES (PRESSURE, PRESSURE-PAIN COMPLEX AND PAIN)

	Left hand			Right hand		
O	Pressure	Pressure-pain	Pain	Pressure	Pressure-pain	Pain
Ba	9	2	10	17	1	20
Be	15	2	25	11	5	24
Br	13	4	27	9	6	20
J	7	4	11	7	4	13
M	9	10	9	15	7	14
P	14	3	15	9	3	17
W	11	7	20	6	2	12
Z	29	12	31	24	11	30
Totals	107	44	148	98	39	150

If it is justifiable to infer that more perfect localizations should appear in the column of that quality which is most accurately localized, it would seem to follow that larger errors should tend to appear in the column of that quality which is least accurately localized. Accordingly, we have arranged a table which reveals the largest errors that fall to each of the three classes of qualities in the 250 light and 250 heavy stimulations by the thistle.

Table VI indicates that the largest error for the pressure-pain complex is almost invariably smaller than that either for simple pain or simple pressure. The comparison of largest errors between pressure and pain shows no significant difference, although, in general, that for pressure is slightly greater than that for pain. The evidence which this table brings forward tends to support, even if only to a slight degree, our previous findings with regard to the comparative accuracy of localizations of simple pain and simple pressure.

TABLE VI

SHOWING THE DISTRIBUTION OF LARGEST ERRORS OF LOCALIZATION, WITH THE NUMBER OF TIMES THE ERROR OCCURRED, IF MORE THAN ONCE, INDICATED IN PARENTHESES

	Left hand			Right hand		
O	Pressure	Pressure-pain	Pain	Pressure	Pressure-pain	Pain
Ba	14, 13 (4)	12, 11, 10	14, 12	12(2), 11, 10(4)	11(2), 10(2)	13, 11
Be	12(4), 11(4)	10(2)	11(3), 10(7)	13, 12(3)	10	11(5), 10
Br	14, 12(2)	10(3)	11, 10	13, 11(6)	10(2)	10(3), 9
J	13, 12(6), 11(4)	11, 10	12(4), 11(2)	14(2), 12(2), 11(7)	10	13, 12 11(3)
M	13, 12, 11(3)	12, 11, 10	12, 11(3), 10	13, 10	10(4)	11(2), 10(3)
P	13, 12, 11	12, 11, 10	13, 12, 11(3)	15, 10(5)	10	14, 12(2), 11
W	12, 10(9)	10(2)	14, 11, 10(5)	20, 15, 12(10), 11(10)	12(2), 10(7)	14(2), 12(9), 11(6)
Z	13(2), 12(2), 11	10(3), 8	12(2), 11, 10(2)	11(3), 10(8)	10, 9	10(6), 9(5)

Our results seem to provide a partial substantiation for the positions of both of the parties to the controversy referred to at the outset. The work of Ponzo is confirmed in regard to the point that the error of localization for pain is smaller than that for pressure, and also in respect to the matter that the difference between the errors of localization for pain and for pressure is small. Our results tend to corroborate in a general way the finding of Mayer that the error of localization for pressure-pain complex is smaller than that for either pressure or pain.

THEORETICAL DISCUSSION

If one kind of cutaneous stimulation can be localized with greater accuracy than another, there must be some foundation for any difference which occurs in this respect. Ponzo, as we have already pointed out, explained his discovery that the error of localization for pain was smaller than that for pressure in connection with the argument of *biological necessity,* that is to say, it is more essential to the well-being and survival of an organism that pain be accurately localized than pressure. *Practice* cannot be alleged as a principle of explanation here because we have much more practice in localizing pressure than in localizing pain.

If a complete picture were available in regard to the events which take place in the nervous system in these three kinds of stimulation, a satisfactory explanation of the differences in respect to accuracy of localization might be given. However, until all of these facts are determined, we shall be required to infer the probable basis for the difference in terms of the few isolated facts which are at our disposal.

Obviously the after-sensation which is experienced after the removal of the stimulus and during the period of localization is the chief guide to localization in stimulations of touch. The after-sensation may be said to represent a continuation of stimulation after the removal of the stimulus. Since localization is made chiefly in terms of the after-sensation, and since there is a difference in the accuracy of localization of the three qualities which the thistle aroused, it would seem to be probable that the composition of the after-sensations and the nature of the conditions of stimulation underlying them must be different in the three cases.

One or two recent findings point to the probability that there is a difference in the conditions of stimulation in these three qualities. Piéron[9] has shown that the rate of propagation of nerve impulses in a cutaneous nerve varies with the kind of stimulation. Erlanger, Gasser, and Bishop[10] have shown that during stimulation a nerve bears impulses travelling at different rates of speed. Now pressure-pain complex results from a combination of two kinds of stimulation—pain and pressure. The cutaneous nerve thus bears impulses which travel at different rates in such a stimulation, and the specific impulses for pain and for pressure may also differ in other ways which are as yet undetermined. The mode of integration of these impulses in the cortex would seem to be essentially of a different order than that which operates in the integration of a volley of impulses which are relatively homogeneous in respect to rate of propagation and other characters. It is clear that the greater accuracy of localization in pressure-pain complex may find its explanation in the difference in type of cortical integration.

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[9] H. Piéron, Nouvelles données sur la dissociation des douleurs cutanées, *J. de Psychol.,* 28, 1931, 279-282.

[10] J. Erlanger, H. S. Gasser, and G. H. Bishop, The compound nature of the action current of nerve as disclosed by the cathode ray oscillograph, *Amer. J. Physiol.,* 70, 1924, 624-666.

The assumption that the integration of more diverse kinds of impulses provides the basis for improved accuracy in localization is not so adequate in explaining the difference in accuracy of localization in the two simple qualities of pressure and pain. In both of these cases, supposedly a homogeneous volley of impulses pass over the nerve during stimulation. However, it is possible that there is greater variability of impulse rates around the modal rate or a greater difference in the character of the impulses in the case of pain than in that of pressure. If it should eventually be determined that this is the case, the assumption that greater accuracy of localization is correlated with greater diversity of the body of impulses which are being integrated would serve to explain the difference in accuracy of localization in pain and pressure stimulations also.

Investigators who are concerned with problems which find their setting in the field of touch are called upon to relate the results of their experimental efforts to one or the other of two theoretical hypotheses, which have been labelled the *Qualitative* and *Quantitative* theories of touch. This is not the place or time to evaluate the relative merits of these two theories, but inasmuch as neither of them has completely displaced the other, it would seem to be proper to allocate to each theory those facts which seem most clearly to fall to it.

Those who adhere to the qualitative theory contend that pain and pressure arise from the stimulation of a different kind of receptor. Defenders of the quantitative theory apparently take the position that pain results from maximal stimulation of the same receptors which give rise to pressure when stimulated at lower levels of intensity.

While more intense stimulation tended to be correlated with the production of pain and lighter stimulation with the arousal of pressure, and greater amount of stimulation was correlated with slightly increased accuracy of localization, nevertheless the subjective descriptions seem to be chiefly in harmony with the qualitative theory. This was particularly indicated in the work in which the sensations that were aroused by the thistle were classified according to quality into three groups. Although the *O*s experienced difficulty at first in classifying these experiences, they were soon able to make prompt judgments in regard to the class into which the experience belonged. These judgments were made in terms of characteristics which were specific to each of the three designated qualities. The quality of pain was distinguished by greater brightness and liveliness, and by a stinging or penetrating quality. As compared with pain, pressure was always designated duller. Some of the pains were brighter than others, and so were some of the pressures duller than others. However, the linear series of dull pressures, if they may be said to represent a simple uni-dimensional series, did not seem to be continuous with the series of bright pain qualities. Pressure did not seem to be a pain that had lost its brightness, nor did pain appear to be a pressure that had acquired some degree of brightness. Each of these two qualities was definitely distinguished from the other, and there was little evidence that the two experiences had any characters in common. Furthermore, the pressure-pain complex was not described as an intermediate between pressure and pain, but in general as an experience in which these two distinctive qualities occurred side by side.

SUMMARY

The experimental results which are presented above indicate that stimulations of pain are localized with a slightly greater degree of accuracy than stimulations of pressure. Stimulations of pressure-pain complex are localized, in general, with a degree of accuracy slightly greater than that for either of the simple qualities of pain or pressure. The number of perfect localizations is slightly greater for pain and pressure-pain complex than for pressure, and largest errors of localization tend to be greater in the cases of pressure and pain than in that of pressure-pain complex.

With regard to theory, the suggestion is made that the greater degree of accuracy in localization in the case of pressure-pain complex may find its explanation in the fact that two kinds of impulses are here being integrated in the cortex, whereas in the case of pain or pressure the impulses which arise from stimulation are more homogeneous in character. Although a body of relatively homogeneous impulses pass over the nerve in stimulations of simple pain and simple pressure, it is still possible that there is greater variability in the volley of impulses for pain than for pressure to account for the greater accuracy of localization in stimulations of pain.

AN EVALUATION OF SOME RESULTS FROM THE KNOX CUBE TEST

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This study attempts to evaluate some results obtained by giving the Knox Cube Test to public school children. It is concerned with the possible uses of the test in clinical and educational settings for normal children, and with questions of validity and reliability. It deals also with the relationships between this performance test, the Stanford-Binet scale, and measures of school progress.

Historical review. In 1914 Knox first published his scale for estimating mental deficiency, based upon work with immigrants at Ellis Island.[1] In this battery was included the Cube Test, which seems to have attracted more interest and attention than any other single unit of the scale. Knox claims that the test assumes no teaching; that it measures inherent power, reasoning, judgment, sense of form, perseveration, attention, and coöperation. Since his standardization is based on the results of 400 suspected mentally deficient subjects, and since we are primarily interested in the test in its application to normal children, we are not particularly concerned with the results he then obtained.

The following year Pintner published his revision of this test.[2] He refined the method of presentation and expanded the test, making it more than twice its original length. He then standardized his revision, using 867 normal children, a few adults, and 463 feeble-minded individuals. He writes, "the test appeared to me, at first seeing it applied, to be an excellent one in many ways. Without attempting to enter into a useless discussion as to the actual mental processes involved, we may say in a general way that it depends largely upon imitation, at the same time affording every opportunity for other factors involving intelligence to assert themselves."[3] The basis for this interpretation is not clear from the evidence presented.

In 1918, an examination of freshmen at the Carnegie Institute of Technology, led Rachofsky to make a study of speed of presentation of the test as a factor in ease of recall.[4] He used colored blocks instead of the standard black ones, and allowed

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[1] H. A. Knox, A scale based on the work at Ellis Island for estimating mental defect, *J. Amer. Med. Assoc.*, 62, 1914, 741-747.

[2] R. Pintner, The standardization of Knox's Cube Test, *Psychol. Rev.*, 22, 1915, 377-401.

[3] *Op. cit.*, 377.

[4] L. M. Rachofsky, Speed of presentation and ease of recall in the Knox Cube Test, *Psychol. Bull.*, 15, 1918, 61-64.

his *Ss* (junior and senior boys in high school) two trials on each test. He divided his *Ss* into 7 equal groups. The test was given to the first group at the rate of one tap per half second; with the second group the rate was one tap per three-quarters of a second; with the third, one tap per second; and so on up to the seventh, with which the rate was one tap per two seconds. He concluded that the slower the examiner presents the test the fewer the number of errors. Ease of recall seems to be a continuous function of speed of presentation; but he suggests that if the speed of presentation were slowed down still further, a U-shaped curve would result due to the process of forgetting. He finds that it is more serious to give the test too fast than too slow, and suggests that the best speed for presentation is one tap per one and a half seconds. He finds in his experimentation that imagery is predominantly kinaesthetic, associated with either visual imagery for colours or auditory imagery for color names. A few bright children numbered the cubes. "Incidentally," he says, "one of the explanations of the excellence of the Knox Cube as a mental test is the premium it pays to resourcefulness and ingenuity."[5] These, again, are impressions rather than conclusions based on experimental evidence.

In January of this year, Reymert and Hartman published a study of the Knox Cube Test in which it was their purpose to emphasize the qualitative aspect rather than only quantitative results.[6] The *Ss* used were 50 children between the ages 13–14 yr., and 100 adults between 21–30 yr. These *Ss* were given the test 8 times at two-week intervals. The actual learning from trial to trial was slight, but greatest between the first and second trials. The average intercorrelation of all the trials was 0.36. The first trial correlated least with the successive trials. The number of items would have to be increased 25 times to give a reliability of about 0.90. The qualitative results were indefinite. It was found that procedures for solving the test varied from individual to individual, and in the same *S* from trial to trial. "The most superior method is probably 'the organization of the cubes tapped into a pattern of units' (recall as a whole)."[7] In their general conclusions, the authors say, "in spite of seemingly adverse facts against using the Knox Cube Test as a measure of ability, it may still prove useful in helping to differentiate subnormal levels of general intelligence."[8]

The test has been included in several intelligence scales, such as the Army, Gaw, Pintner-Patterson, Universal, and Knox Performance Scales; and it has long been employed in clinics. Opinions differ, however, as to its value. At the present time the weight of opinion seems to be against the test. Wells writes that "the Knox Cube Test was among the first performance tests to be standardized for age-levels, but it is significant for age-level only as a member of a considerable group of tests. Much depends upon the process of memorizing the pattern. Overt responses give relatively little clue as to what this has been. Visual, auditory, and kinaesthetic processes readily enter. The test may be regarded as one of intelligence in so far as high scores reflect systematic use of such clues."[9]

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[5] *Op. cit.*, 63.

[6] M. L. Reymert and M. L. Hartman, A qualitative and quantitative analysis of a mental test, this JOURNAL, 45, 1933, 87-105.

[7] *Ibid.*, 104.

[8] *Ibid.*, 105.

[9] F. L. Wells, *Mental Tests in Clinical Practice*, 1927, 138.

From the Judge Baker Foundation in Boston comes the following evaluation: "In our experience the variability of performance on repeated trials has led us to believe that the test is unreliable. Having formerly used it extensively, we now employ it very seldom."[10]

It will be seen from the foregoing that comparatively little systematic research has been done with this particular test: The interpretations of its validity that have been given, are, in the main, speculative; and, as with most performance tests, the exact purpose underlying its use is hard to discover. The question of reliability has not received much attention, although Reymert's study suggests it to be low, and attributes the source of unreliability only to the inadequate length of the test. It was with a view to throwing further light upon these characteristics of the test, that the evaluation of results here presented was undertaken.

METHOD AND PROCEDURE

Subjects. The *S*s used in this experiment were pupils at Regal Road Public School, Toronto. They ranged in age from 5 to 15 yr.; and in grade from Kindergarten through Senior IV.[11] When the study was begun, Knox scores were available for 559 pupils, who had been examined by the Mental Hygiene Research Division during the past five years. Some 150 further cases were examined by the writers.

Method of examination. The examinations were conducted with equipment similar to that used by Pintner. This consists of 4 one-inch cubes, and a fifth cube used for the tapping process. The only variation here adopted was that the 4 cubes were mounted on a black strip which held them in a constant positional relationship, 2 in. apart. The strip of cubes is placed on the table between the *E* and *S*. After each line (set of taps) has been given, the fifth cube is placed in front of the child, equidistant between the third and fourth cubes, but nearer the child.

The following 12 lines are Pintner's revision of the original Knox Test, and are the lines used in this experiment:

Line	Blocks tapped	Line	Blocks tapped	Line	Blocks tapped
1	1234	5	1432	9	13124
2	12343	6	1423	10	143124
3	12342	7	13243	11	132413
4	1324	8	14324	12	142341

Instructions. The standard instructions were used. Before the test, *S* was told, "Watch carefully and then do as I do," and then after a line has been tapped by *E*, he was asked to "Do that." In no case tested by the writers was there any confusion or misunderstanding of the directions.[12]

Scoring. The total score on the Knox Cube Test consists of the number of lines correctly imitated.

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[10] A. F. Bronner, W. Healy, G. M. Lowe, and M. E. Shimberg, *A Manual of Individual Mental Tests and Testing,* 1928, 196.

[11] In the Toronto Public Schools there are 7 grades above the Kindergarten, called respectively Junior and Senior First, Second, Junior and Senior Third, Junior and Senior Fourth.

[12] Pintner suggests that the Knox Cube Test would be equally effective if the directions were entirely omitted. He cites a few cases where no directions were used, but where the test was given with satisfactory results (*op. cit.,* 378).

RESULTS

(1) Relationship between Knox Cube scores and chronological age. The average total Knox Cube scores for the various ages are given in Table I. The data on which this table is based were selected from the case-histories at the school, and from the results of tests administered by the writers, so that 50 children of each age-group were included; and also so that the age-averages for the groups fall at exactly 6, 7, 8, etc. yr. The chronological and mental age ranges were also made comparable from group to group by this method of selection.

Table I indicates an increase in the average number of lines passed from the younger to the older age-levels, but it is evident that this increase is very gradual. There are 9 age-levels, and only a differentiation of 4 in the average number of lines passed from the younger to the older levels. This would seem to suggest that the material is not finely graded in difficulty. It is interesting to note that these findings agree closely with those obtained by Pintner.

TABLE I

RELATION BETWEEN KNOX CUBE SCORES AND AGE

Chronological Age		Mental Age		No. Lines Passed			Pintner's Results
Av.	Range	Av.	Range	Av.	Range	S.D.	
6	5·7–6·6	6·2	3·7–7·8	3.36	0–8	1.82	4.22
7	6·7–7·6	7·0	4·6–9·1	5.34	2–9	1.78	5.12
8	7·7–8·6	7·11	4·9–10·0	5.22	1–8	1.53	5.60
9	8·7–9·6	9·0	6·4–11·0	6.20	3–11	1.63	5.59
10	9·7–10·6	9·11	7·4–12·6	6.02	2–10	1.79	6.29
11	10·7–11·6	10·10	7·11–13·1	6.14	3–11	1.65	6.68
12	11·7–12·6	12·0	8·6–14·2	7.04	3–10	1.51	6.66
13	12·7–13·6	12·11	9·2–14·8	6.64	3–10	1.44	6.66
14	13·7–14·6	13·11	10·8–16·0	6.92	3–10	1.36	7.55

(2) Relationship between Knox Cube score and mental age. The Stanford Revision of the Binet Test was given to each pupil together with the Knox Cube Test at the same interview. The distributions of mental ages and Knox scores appear to be fairly normal, as the following data show.

Binet Mental Age	Frequency	Knox Scores	Frequency
3 and 4 yr.	6	0 and 1	7
5 and 6 yr.	51	2 and 3	46
7 and 8 yr.	115	4 and 5	119
9 and 10 yr.	119	6 and 7	191
11 and 12 yr.	92	8 and 9	75
13 and 14 yr.	57	10 and 11	7
15 and 16 yr	5		

Correlation coefficients were calculated between mental age and Knox Cube score for the age-groups separately, using the Pearson product-moment formula. The results were as follows.

Age	r	P.E.	Age	r	P.E.	Age	r	P.E.
6	0.41	0.08	9	0.31	0.09	12	0.25	0.09
7	0.38	0.08	10	0.32	0.08	13	0.41	0.08
8	0.25	0.09	11	0.48	0.07	14	0.41	0.08

These coefficients of correlation are all positive, and would, therefore, seem to indicate that there is some relationship between Knox Cube score and mental age. The correlations, however, are relatively low, and no definite relationship appears between the size of the correlation and the age-groups. It is true that the range of talent in each group is comparatively small; but even when this is offset by correlating the entire group, the correlation becomes only 0.58, P.E. 0.02.

(3) *Relationship between school grade and Knox Cube score.* Approximate measures of relationship were sought by correlating the score in the Knox Cube Test with school grade at the time the test was given. The values obtained are compared in Table II with similar values for the mental test.

TABLE II

CORRELATION OF THE SCHOOL GRADE WITH KNOX CUBE SCORES AND WITH MENTAL AGE

Age-group (in yr.)	Knox Cube score and school grade		Mental age and school grade	
	r	P.E.	r	P.E.
6	+0.21	0.09	+0.37	0.08
7	+0.33	0.09	+0.55	0.07
8	−0.48	0.07	+0.63	0.06
9	+0.42	0.08	+0.72	0.05
10	+0.16	0.09	+0.59	0.06
11	+0.43	0.08	+0.67	0.05
12	+0.11	0.09	+0.49	0.07
13	+0.22	0.09	+0.51	0.07
14	+0.09	0.09	+0.67	0.06
All	+0.39	0.03	+0.73	0.01

It will be noted that the coefficients obtained with scores in the Knox Test are decidedly lower, and much more variable, than those obtained with the mental test.

(4) *Reliability of the Knox Cube Test.* It is difficult to obtain a measure of reliability on this test without actually repeating the test. Since this was impracticable with the group here under consideration, the method

of correlating scores on odd and even lines was adopted. While, particularly with a test of this length, this is not very adequate, the results are given in Table III, and some further indication of reliability appears in a later section. It will be noted that the values given in Table III vary considerably, and are all low.

TABLE III

Coefficients of Reliability
(Correlating odd and even lines)

Age-group (in yr.)	r	P.E.
6	0.42	0.08
7	0.48	0.07
8	0.39	0.08
9	0.35	0.08
10	0.46	0.07
11	0.25	0.09
12	0.28	0.09
13	0.47	0.07
14	0.22	0.09
All	0.28	0.03

(5) Difficulty of the test material. Lack of differentiation in score for the different age-levels suggested that it might be fruitful to examine the material for difficulty value. Table IV shows the percentage passing each line of the test at the different age-levels.

The results are irregular. For instance, we find that line 4 of the test seems more difficult for 9-yr. old children than line 5, while the reverse is true for the 10-yr. group. Likewise, line 11 seems easier than line 12 for the 10-yr. group, while at 11 yr., line 12 appears to be easier than line 11.

TABLE IV

Percentage Passing Each Line of the Test at the Different Age-Levels

Age (yr.)	Line											
	1	2	3	4	5	6	7	8	9	10	11	12
6	92	66	54	26	40	18	8	10	8	4	0	6
7	100	96	80	50	66	58	14	22	26	6	2	12
8	98	94	94	52	58	60	12	20	24	2	0	6
9	100	100	92	68	80	68	30	10	30	6	6	4
10	100	96	92	72	64	78	42	16	20	8	12	4
11	98	100	92	70	78	80	38	28	20	8	4	12
12	100	98	92	78	76	82	48	38	28	20	20	12
13	100	100	92	74	78	88	44	30	26	6	14	4
14	100	100	98	80	78	95	58	30	44	8	14	14

If we transpose the percentages of Table IV into sigma values ranging from —3 sigma to +3 sigma, as has been done in Fig. 1, an interesting fact is brought out. While there is fluctuation in the difficulty value of the lines, nevertheless there seem to be four fairly definite groups, namely, 1, 2, and 3; 4, 5, and 6; 7, 8, and 9; 10, 11, and 12. Although these lines change around on the scale at different age-levels this difficulty grouping seems to be constant. This is more definitely reflected when we combine all the age-groups.

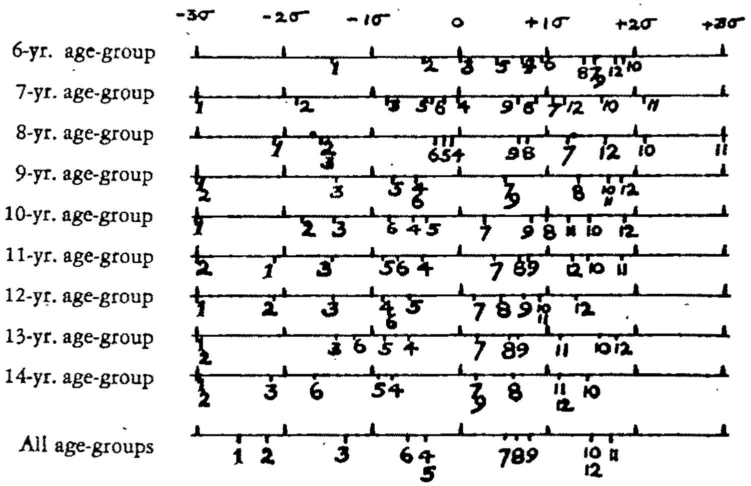

FIG. 1. SIGMA DIFFICULTY SCALE
The difficulty of every line of the Knox Cube Test is shown in sigma values for every age-level.

Analysis of the lines themselves also seems to point to the same grouping. The first 3 lines are 1234, 12343, and 12342. The next 3 lines are 1324, 1432, and 1423 (means and extremes). The next 3 lines are 13243, 14324, and 13124 (five digits). The last 3 lines are each composed of 6 digits.

These values seem to point to the fact that we have in the Knox Cube Test material about 4 levels of difficulty. If this is the case, then the test is, at best, only a very crude instrument for measuring. It may differentiate the extremes of the group, but it is useless for fine distinctions. No doubt a test of 4 difficulty levels serves the purpose for which this test originated—to distinguish feeble-minded from normal individuals. Such a test is not, however, of very great value when applied to school children, if interpreted merely on the basis of the total score obtained.

(6) Analysis of errors. A qualitative study of the nature of errors was undertaken to see if the types of errors made might be classified, and if

such a classification might reveal data regarding the individuals tested. The procedure adopted was to determine the frequency with which any particular faulty response was made at the various age-levels; and the results of thus analysing the responses are summarized in what follows.

From the seventh line to the end of the test, no definite pattern of errors was recognizable. Apparently the individual got the pattern of the test line as a whole, and reproduced it correctly, or else failed to do so and gave only a random response.

At the 5-yr. level the outstanding characteristic seemed to be to repeat the first line (1234) as a response to subsequent tests. This error seemed to be again indicated on line 2 at the 6-yr. level, but dropped out for the older levels.

Line 4 (1324) revealed an error pattern of 1342, indicated as early as the 5-yr. level, and carried through to the 14th. yr. At 5 yr., 5 out of the total of 23 errors made on this line were of this pattern. This error increased in frequency until at the 14-yr. level 5 out of the 7 errors made were of this type.

Line 5 (1432) revealed an error pattern of 1423 in 4 out of 19 errors at the 5-yr. level, and the pattern increased in frequency until at the 14-yr. level, 6 out of the 7 errors were of this type.

Line 6 (1423) showed an error pattern of 1432 in 13 out of 28 errors made at the 5-yr. level, while the only error made at the 14-yr. level was of this type. This is an interesting error especially because 1423 is the correct response for the test line immediately preceding.

From giving the test, two general observations may be made: (1) at the lower age-levels, there was a noticeable tendency to respond to a given line by a series of random hits rather longer than the given line; while at the higher age-levels, the wrong responses tended to be shorter than the test line; (2) recognition of the *S*'s failure to reproduce a line correctly was realized by the *S* more often at the older levels. Any response seemed to give satisfaction to the younger children, while the older were more self-critical.

The main indication of the above data is that a systematic construction of patterns of response, with careful attention to the relations involved, might lead to a more adequate scale of difficulty, and to increased knowledge of the nature of the processes involved in performing these test-items.

(7) Examination of a group of Third Grade pupils. In selecting a group of *S*s for more intensive examination, the Third Grade level was chosen. The results of the correlations quoted above did not indicate any particular level of the school where Knox scores would have greater significance than at other levels. Pupils from a Junior and a Senior Third class were therefore employed purely as a matter of convenience.

Seventy-five cases were tested with the Knox Cube Test,[13] and for these were available recent scores on the National Intelligence (Group) Scale. The latter had been given three months earlier, and the mental age scores were accordingly calculated as on the date of giving the Knox. The measures of school progress employed were those deemed to be of greatest importance at this level; namely, arithmetic, literature, reading, and writing. The scores on all six variables were translated into sigma deviation units from the mean of the class, and the intercorrelations are shown in Table V.

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[13] Exploratory experimentation had previously been undertaken by the writers with a view to seeing if the technique of giving the test could be improved (a) by

TABLE V

INTERCORRELATIONS OF THE KNOX CUBE SCORES, INTELLIGENCE, AND SCHOOL MARKS

	(A)	(B)	(C)	(D)	(E)
(A) National					
(B) Knox Cube	−.09				
(C) Literature	.33	.12			
(D) Arithmetic	.48	.06	.32		
(E) Reading	.23	−.02	.56	.17	
(F) Writing	.04	.11	.39	.29	.06

Preliminary analysis of this table by Thurstone's Multiple Factor Method[14] suggested that the major factor underlying the National Test and school marks was less critically represented by the Knox scores; and that probably some other factor was more important in the latter case. Accordingly it was decided to add to the above table, correlations involving certain other psychological tests of known validity, and to analyze the results for other factors. In this way some indication might be obtained of the objective validity of the Knox test.

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varying the time of presentation, and (b) by varying the method of scoring. In regard to the former, a group of 44 children were selected from Junior and Senior Third. This group was divided into two sub-groups approximately equal in age, grade standing, and intelligence as measured by the Stanford-Binet. The first group was given the Knox Cube Test at the usual speed of one tap per second. The second group was given the test at the speed of one tap per half second. In neither case was *S* controlled in his speed of response. The results indicated that as far as score was concerned, there was no difference due to the time of giving. The range of scores in both cases was 4 to 9. The average scores of the two groups were 6.8 (sigma 1.89) and 7.0 (sigma 1.58) respectively. Because it would be necessary to experiment extensively and to make a new standardization before we could be justified in changing the speed of presentation, we decided to use the standard speed in this experiment.

Concerning the method of scoring, several revisions were considered: (1) weighting the lines according to difficulty; (2) counting the number of correct digits in the correct patterns; (3) counting the number of correct digits in correct patterns plus digits correct (in the right place) in incorrect patterns; and (4) counting the number of correct patterns plus (relative-degree-of-accuracy) score in incorrect patterns. The fact that there appeared to be only four levels of difficulty in the test suggests that no such revisions would constitute significant improvement, unless a more finely graded scale were first constructed. In the experiment being discussed above, the standard scoring method was employed.

[14] L. L. Thurstone, Multiple factor analysis, *Psychol. Rev.*, 38, 1931, 406-427.

The psychological tests included, were (a) tests of 'perseveration,' and (b) tests of 'speed of execution.' The former consisted of a battery of four tests such as were used by Pinard;[15] the latter involved the speed of writing numbers, letters, etc., in known sequences, such as 1, 2, 3, 4, etc. These batteries had proved to involve factors other than *g* (as indicated by Spearman's tetrad technique) and to have high reliability.

The additional correlations thus obtained were as follows:

	(A)	(B)	(C)	(D)	(E)	(F)	(G)
(G) 'Perseveration'	.28	−.07	−.01	−.03	.02	−.26	
(H) 'Speed of Execution'	−.02	.04	−.04	.16	.11	.15	.11

Thurstone's technique then gave the following loadings of factors underlying these eight variables.

	Factor		
	I	II	III
(A)	.57	.40	.22
(B)	.34	−.51	.11
(C)	.78	.17	−.14
(D)	.67	.15	−.29
(E)	.57	.21	.30
(F)	.54	−.04	−.40
(G)	−.20	.83	.33
(H)	.23	−.12	.70

While the unreliability of the Knox Cube scores, and the comparatively unknown nature of school marks, make this form of analysis very questionable, with the present data—particularly since Thurstone's technique offers as yet no way of determining the probable-error values of the 'loadings'—some preliminary indication of what Knox scores represent may be read into the results. The Knox Test appears to measure the first factor less adequately than does the National Test, and than it measures the second factor. The first factor is presumably *g*; the second may conveniently be designated *p* ('perseveration'), although it will be noted that Knox scores are inversely related to the perseverative tendency. 'Speed of execution,' the third factor, seems to bear little relationship to this test.

(8) Relationship between Knox Cube scores and misdemeanors. The misdemeanor records of the group of 75 Third Grade children were studied to see if score on the Knox Test was related to frequency and nature of misdemeanors.[16] The Knox Test results were divided quite arbitrarily into

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[15] W. Pinard, Tests of perseveration, *Brit. J. Psychol.*, 23, 1933, 5-19.

[16] These records are based on the daily observation of classroom behavior. Their form has been reported elsewhere (W. E. Blatz and E. A. Bott, Studies in mental hygiene, *Ped. Sem.*, 34, 1927, 552-582).

3 groups. The following results came from a study of misdemeanors according to these groupings:

Knox score	Number of cases	Frequency of misdemeanors	Misdemeanors per capita
9–11	11	30	2.72
6–8	45	55	1.22
3–5	19	34	1.79

This seems to indicate that children who obtain extreme scores on the Knox Test show higher frequency of misdemeanors in the classroom. The group scoring high on the test has the greatest number of misdemeanors per capita.

Misdemeanors which could be listed under the classification of "lack of application" *e.g.* restlessness, inattention, carelessness, laziness, day dreaming, etc.) were next studied, with the following results:

Knox score	Number of cases	Frequency of misdemeanors	Misdemeanors per capita
9–11	11	12	1.09
6–8	45	17	0.38
3–5	19	16	0.84

Here again, children with extreme Knox scores show higher frequency of misdemeanors of this type.

CONCLUSIONS

(1) The Knox Cube Test gives scores which increase slightly with age.

(2) There is a positive but low correlation at every age-level between Knox scores and Binet mental age.

(3) The relation between Knox scores and school grade is variable and never as high as the relation between Binet mental age and school grade.

(4) The reliability of the test is low.

(5) There are approximately only four difficulty levels in the material of the test.

(6) Preliminary analysis indicates that the Knox Test measures general intelligence, but only poorly. It suggests the presence of a second factor which is related to 'perseveration.'

(7) There is some indication of a relationship between behavior and the Knox Test in that the extremes on the test show higher frequency of misdemeanors.

THE RELATION OF SATURATION TO THE SIZE OF THE RETINAL IMAGE

By S. Smith Stevens, Harvard University

It has been observed that the saturation of visual sensation changes with an alteration of the size of retinal area stimulated and also with the position of the image upon the retina. The variation of saturation with the size of the stimulated area was noted by Charpentier in 1886.[1] He reported that, if a red and a green are placed side by side and matched for intensity, a change in their relative values follows upon any subsequent change in the size of the field occupied by the two colors. If the field is enlarged the green appears brighter, and *vice versa.*

The phenomenal changes in colors accompanying a shift in the position of the retinal image was investigated by Whitman in 1899.[2] He arranged an apparatus permitting the exposure of Milton Bradley colored papers at various angles with the line of regard, and he measured photometrically their relative brightnesses. Using the brightness of yellow-green, which remained constant at all angles, as the unit of measure, he found the relative values of red and green-blue to be as follows:

	0°	30°	50°	70°
Red	.514	.275	.194	.165
Green-blue	.529	.680	.717	.717

In other words, as a red stimulus is moved from the center to the periphery of the visual field, there is a decrease in brightness; but if the stimulus is green-blue, an increase in brightness results.

Another but closely related aspect of this phenomenon is the observation by Troland[3] that the Purkinje phenomenon does not hold in the fovea itself, but it reversed. There are many differences between foveal and peripheral vision reported by numerous investigators, most of whom convey the impression that the area exhibiting the characteristics of foveal vision ends abruptly where the periphery begins. Their assumption seems to be

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* Accepted for publication June 18, 1932.

[1] Aug. Charpentier, Sur une condition physiologique influençant les mesures photometriques, *Lumière électrique,* 21, 1886, 165.

[2] F. P. Whitman, On the brightness of pigments by oblique vision, *Science,* 9, 1899, 734-736.

[3] L. T. Troland, The absence of the Purkinje phenomenon in the fovea, *J. Franklin Institute,* 182, 1916, 111 f.

that, granted an adequate technique, a sharp line of demarcation could be drawn around the fovea. The results of the present experiment lead one to question this notion.

The author began the investigation at the suggestion of Professor Boring with the aim of discovering the causes and the exact nature of a phenomenon common enough to those experienced in the use of rotating color-mixers. It is readily observed that in mixing the complementaries, red and blue-green, the resulting gray is pure only if one stands at a certain distance from the color-wheel. Any change in this distance results in a change in the gray, which becomes reddish as the distance increases or grows bluish-green as one approaches the color-mixer. The relation of this phenomenon to that observed by Charpentier is obvious; its relation to the experiment by Whitman is a little less obvious but no less intimate; and its importance for the theory of the retinal mechanism is vital.

TECHNIQUE

A motor-driven color mixer, set behind a black screen which included an Aubert diaphragm, constituted the essential apparatus. The color-wheel consisted of a large disk, 11 in. in diam., on which were fastened the Milton Bradley colored papers red and blue-green, with 180° of each color exposed. Covering the center part of this large disk was a smaller one, 5¼ in. in diam., made up of 300° of black and 60° of white paper. Rotation of the disk, then, produced a gray by reason of the red and blue-green mixture and also a gray by means of the black and white mixture. The latter gray served as a criterion for the purity of the former.

The color-wheel was illuminated by a cluster of 3 lamps, one 75-watt and two 60-watt, type C-2, Mazda lamps, suspended behind the screen slightly above the aperture. It was found necessary to use direct current in these lamps to avoid the stroboscopic shadows.

The screen was set 8 in. in front of the color-mixer. The size of the Aubert diaphragm, through which the *O* could see approximately equal amounts of both the larger and smaller disks, could be regulated by the pulling of two strings held by the *O*.

The *O*'s chair was set upon a miniature 'flat car' mounted on a track, which made it easy to alter his distance from the screen. A constant position of the *O*'s head was assured by a head-rest. The diaphragm and the flat car provided, then, two means of changing the size of the retinal image which resulted from the *O*'s fixating of the color-wheel.

The *O*'s *Aufgabe* was either that, given a certain position in front of the screen, he should change the size of the aperture until a homogeneous field resulted (until the gray of the inner and the outer disks was indistinguishable), or that, given a certain sized aperture, he should alter his distance until the grays were matched. Both methods gave thoroughly consistent results, but the latter procedure was subjectively easier and yielded a relatively smaller probable error in successive judgments. Therefore, in the major part of the experiment, the aperture was fixed at 1.0, 2.5, and 4.0 in. for the side of the square opening, and the *O* was required to determine the distance at which the best match occurred. This judgment is not at all difficult, since the gray of the smaller disk assumes a tinge of contrast color whenever the color of the larger disk is altered by a change in the *O*'s position. As the *O* recedes from the screen the color of the larger disk becomes reddish and that of the smaller one turns into a bluish green. Approaching the screen causes a reversal of this effect.

The next step was to introduce into the black and white disk a small sector of red or blue-green and to note again the distance at which the *O* experienced a homogeneous field. The red sector increased, while the blue-green sector decreased, this distance by an amount dependent upon the size of the sector. It is then possible to compute the size of the retinal image which obtains when the *O* experiences a homogeneous field.

Twenty-seven *O*s, mostly university students, were used. Two of these were color-blind and could not observe the phenomenon; but from the remaining 25 it was possible, incidentally, to determine the distribution of the effect in such a sample, as measured by the distance from the screen at which the individuals were able to report a homogeneous field. The resulting distribution curve resembles closely the Pearson type III, being positively skewed. The more thorough data, however, were obtained from 2 *O*s of whom the author (*St*) was one and P. M. Zoll (*Z*) the other.

RESULTS

Two facts are evidenced by the data obtained. (1) The saturation and hue of a visual experience is dependent upon the size of the retinal area stimulated, and upon its position. (2) For a constant increment of change in saturation a constant proportion of the area stimulated must be added.

The first point is merely a verification of facts already known, but the second is an expression of the nature of the function which holds between saturation and the size of the retinal image. The results from which this function is derived are shown in Tables I and II.

The color increments added to the center disk in Table II are twice as large as those of Table I. Therefore in order to make the ratio $\Delta a/a$ comparable for the two cases, the former should be divided by 2. The meaning of the difference between these two ratios is discussed below in connection with the slopes of the curves of Fig. 1.

The dimension of the retinal image for each increment of color in the center disk is the average of 18 observations in the case of *St* (6 for each size of aperture) and 6 observations in the case of *Z*. The author repeated the observations on himself using colored papers of slightly different saturation and brilliance and obtained the same type of function, as is shown by Fig. 1. All observations were made in a random order.

TABLE I

Dimensions of the Retinal Image Obtained from St for Different Amounts of Blue-Green and Red in the Center Disk in Order to Match the Inner and Outer Disks

($\Delta a/a$ is the ratio of the decrement in area, for each decrement of blue-green, to the original area of the retinal image)

Color of center disk	Amount	Distance in in. of O from screen for apertures			Av. diam. retinal image (in mm.)	One-half area of retinal image (in mm.^{2})	$\Delta a/a$ Av. = .412
		1 in.	2.5 in.	4 in.			
Blue-green	24°	2.7	6.7	11.0	5.535	15.318	
	20°	4.3	10.0	13.0	3.951	7.805	.490
	16°	7.0	12.5	17.8	2.837	4.024	.484
	12°	9.8	17.8	26.7	1.961	1.923	.522
	8°	11.5	23.8	33.5	1.557	1.212	.368
	4°	13.5	28.3	40.0	1.312	.861	.289
	0°	17.5	33.2	48.3	1.076	.579	.327
Red	4°	22.8	45.8	63.7	.805	.324	.440
	8°	31.7	59.8	83.0	.607	.184	.432
	12°	39.8	83.0	120.7	.441	.097	.472
	16°	62.7	112.7	169.3	.308	.047	.515
	20°	77.0	146.0	209.7	.245	.030	.361
	24°	90.3	175.0	257.7	.204	.020	.333
	28°	120.0	222.7	316.3	.164	.013	.350
	32°	138.0	242.0	420.0	.127	.008	.384

TABLE II

Dimensions of the Retinal Image Obtained from Z for Different Amounts of Blue-Green and Red in the Center Disk in Order to Match the Inner and Outer Disks

($\Delta a/a$ is the ratio of the decrement in area, for each decrement of blue-green, to the original area of the retinal image)

Color of center disk	Amount	Distance in in. of O from screen for apertures			Average diam. of retinal image (in mm.)	One-half area of retinal image (in mm.^{2})	$\Delta a/a$ Av. = .423
		1 in.	2.5 in.	4 in.			
Blue-green	24°	9.5	26.0	40.0	1.507	1.136	
	16°	14.0	42.0	59.0	1.000	.500	.528
	8°	18.5	59.0	83.0	.759	.288	.424
	0°	30.5	58.0	96.0	.556	.155	.461
Red	8°	39.0	86.0	131.0	.426	.091	.413
	16°	44.0	101.0	190.0	.342	.058	.362
	24°	57.0	145.0	232.0	.260	.034	.413
	32°	80.0	180.0	260.0	.208	.022	.363

Most significant of these results is the constancy of the ratio between the decrement in area necessary to neutralize a given increment of red in the center disk (Δa) and the area already under stimulation (a). This ratio shows that a logarithmic relation holds between the size of the retinal image and the amount of blue-green in the center disk. It is assumed that a given sector of blue-green is equal to the absence of the same amount of red and *vice versa,* since equal amounts of the two colors are exactly complementary under the light used. In other words, this relationship means

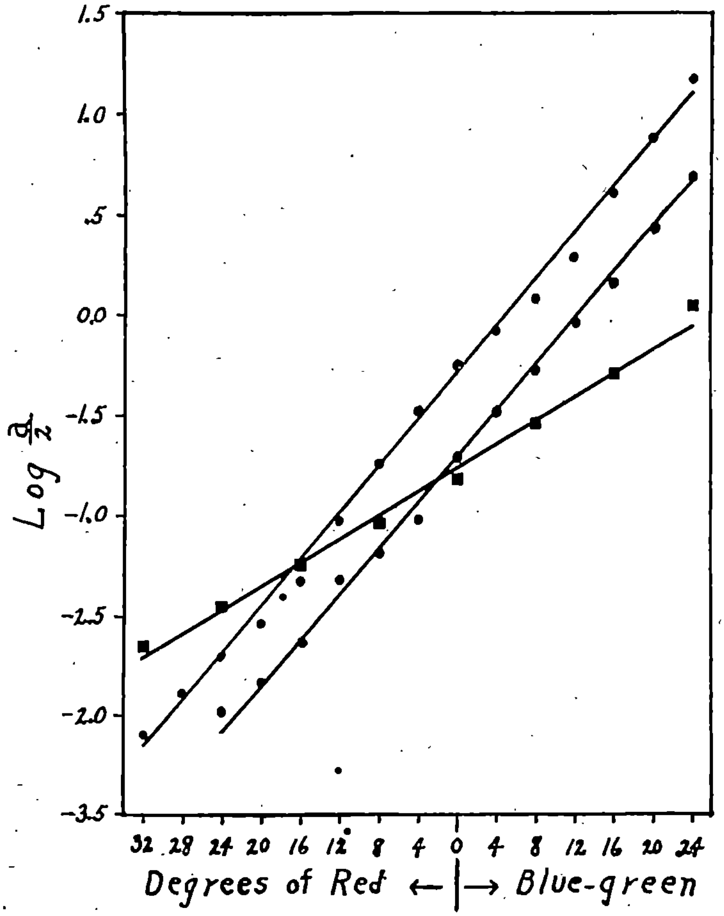

FIG. 1. THE RELATION BETWEEN THE AREA OF THE RETINAL IMAGE AND THE SIZE OF THE RED OR BLUE-GREEN SECTOR IN THE CENTER DISK OF THE COLOR WHEEL

The square points are for *Z*'s data; the others are for *St*'s. The lower line for *St* is from observations not tabulated in this report.

that the retina is relatively more sensitive to red than to blue-green at the center and that this sensitivity decreases, very rapidly at first, as one goes towards the periphery, and more slowly as one progresses outwards. The function representing the sensitivity of the retina to blue-green is the converse of that for red, as shown in Fig. 2. As the image spreads farther from the center of the fovea the amount of additional area needed to balance an increment of blue-green added to the center disk becomes greater, but, over the wide range tested, this amount is found to be a fixed proportion of the original area. The differential sensitivity of the retina to color follows a simple logarithmic law. This fact is easily observed qualitatively if one but shifts his point of fixation while looking at the

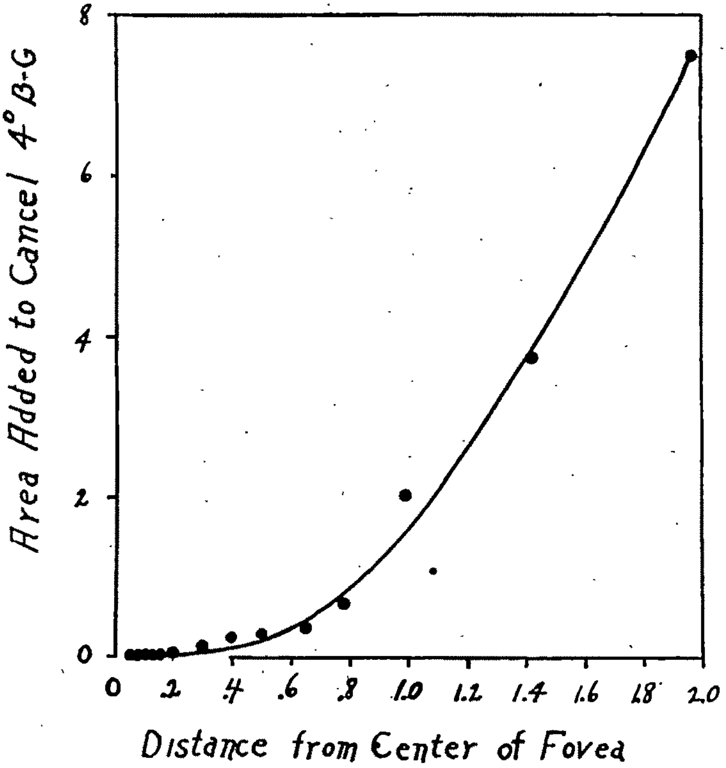

FIG. 2. CURVE SHOWING THE NATURE OF THE DISTRIBUTION OF COLOR SENSITIVITY IN THE RETINA OF *St*

The points represent the observed increments in area which must be added to balance the effect of an increment of 4° of blue-green in the center disk, when the area covered by the retinal image extends beyond certain distances from the center of the fovea.

homogeneous field of the color-wheel. A very slight angular shift causes the outer disk to become bluish-green and the inner one to become reddish by contrast; but, as the fixation point becomes farther removed from the line between the inner and outer disks, the effect of additional angular increments grows less and less. The importance of this fact in relation to the problem of the anatomical distribution of cones is obvious.

It will be noted from the data of Table I that the logarithmic relation holds for measurements which take us well within the fovea. (Table II does not show such small areas; the acuity of the *O* was poor). If we assume that the foveal diameter is about 0.3 mm., it is evident that, since the last four determinations are for areas smaller than the total area of the fovea, the logarithmic function reveals no sudden break or change which might be construed as marking off the limits of the fovea. Likewise there is no break at the boundary of the rod-free area, but only a continuous function over the range from 5.5 mm. diam. to 0.127 mm. diam. (Fig. 2). The conclusion follows that the peculiar distribution of sensitivity which accounts for this function is not only a peripheral phenomenon, as is usually held, but is also characteristic of the fovea itself.

The validity of Hecht's[4] assumption that the fovea contains equal numbers of the primary receptor units for blue, green, and violet would appear to be called in question by the present findings. If Hecht's hypothesis were true, and the numbers of the primary cones within the fovea were equal, there would still remain the question of their arrangement. The nature of the functions found in this experiment indicate that there would have to be a massing of the receptors for red at the center of the fovea with a decrease in their density as one approaches the periphery. Then it would follow that the receptors for red within that small central area of about 0.1 mm. diam. would outnumber the receptors for blue-green in the same area, *if* the numbers of the two receptors within the *entire* fovea were equal.

The slopes of the two curves (Fig. 1) obtained from *St* are approximately equal, as is to be expected, but the slope of *Z*'s curve is different. This means that when a given increment, say, 8° of blue-green, is added to the center disk, the proportional increment in retinal area necessary to cancel it is greater in *St*'s than in *Z*'s case. (This is also shown by the magnitudes of the two ratios $\Delta a/a$, in Tables I and II.)

In other words, the variability of sensitivity as one goes from the fovea to the periphery is greater for *Z* than for *St*. If one assumes that the perception of color is due to specific receptor units, these differences in slope would mean that the density per unit area of the cones for red and blue-green shows greater variability from place to place in *Z*'s retina. Or, in terms of the chemical process involved, it would seem to mean that in his case the absorption coefficient for red is less uniform over the whole retina.

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[4] Selig Hecht, The retinal processes concerned with visual acuity and color vision, *Howe Lab. of Ophthal. Bull.*, 4, 1931, 1-54.

THEORY

Two general types of hypothesis can be offered for the explanation of this phenomenon. One possibility is that the visual experience depends directly upon the point at which a dynamical equilibrium is reached within the area stimulated. Thus, it might be that when a large area is affected, the resulting equilibrium is such that the predominating sensation is blue-green. Here an analogy suggests itself: the point of equilibrium of a chemical solution depends upon the concentration of the ingredients. In the same way stimulation of a larger area may render available proportionately more of some substance like rhodopsin, which will cause a change in concentration and a consequent displacement of the point of equilibrium of the reversible photochemical reaction, resulting, thereby, in a blue-green rather than a red sensation.

The other kind of hypothesis takes account of the individual cones and explains the phenomenon in terms of the anatomical distribution of distinct functioning elements, with no reference to their interaction. The picture presented in this case is relatively simple. However, Adrian and Matthews[5] conclude from their work on the eel that the retina cannot be thought of as a mosaic of sensitive points, each connected with its own nerve fiber and acting independently of the rest. To the present author, therefore, it seems inevitable that the truth of the matter lies somewhere between the extremes of these two hypotheses, or at any rate that the best hypothesis for the present is one recognizing the effect of both the nature of the anatomical distribution of cones and the possibility of their mutual interaction after the manner of *Gestalt* dynamics.

In addition to the assumption of separate functional units peculiarly distributed on the retina and capable of interaction, the question of threshold must be reckoned with. To the author it seems well, in view of the following facts, to accept the theory of a frequency distribution of thresholds as put forth by Hecht[6] and to assume, in addition, that the form of the distribution is similar, within a given area, for the cones for red and for blue-green. The evidence for this latter hypothesis is the fact that a change in the intensity of the illumination reaching the color-wheel causes no loss of homogeneity to the field seen through the aperture. If, when the *O* is at a distance from the screen that will render the match between the grays of the inner and outer disks as perfect as possible, the intensity of illumination is decreased by moving the source away from the color-mixer or increased by reflecting light back on to the disks by means of mirrors, then the differential effect, if any, on the two grays is unobservable. This result is found for an intensitive range equivalent to halving and doubling the original intensity.

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[5] E. D. Adrian and R. Matthews, The action of light on the eye, *J. Physiol.*, 63, 1927, 378-414.

[6] Hecht, *op. cit.*, 4-6.

There arises, however, a very interesting effect if the intensity of only a small part of the field seen through the aperture is altered. Such a change can be made easily by having the *O* fixate a small black disk of about ½ in. diam. held in the center of the aperture in such a manner that it covers equal amounts of the inner and outer disks on the color-wheel. Then, when the small disk is removed, a white negative after-image is seen against the two parts of the field with the result that within the area of the after-image there is no longer a homogeneous gray, but the outer disk is seen as having a reddish tinge, just as would be the case if the size of the aperture itself were reduced. This effect must be due to intensity alone, because it is the same when a small bright spot of light, reflected from the illuminating source by a small mirror, is thrown back on to the revolving disks. Furthermore the phenomenon is more striking when the bright spot is small. Just as with the variation of the aperture itself, the degree of the predominance of red varies inversely with the size of the area stimulated.

Theoretically we can take these facts to mean that, at the original intensity of the color-wheel, not all of the cones are being stimulated, and that, as the intensity in any part of the field is increased, more of these inactive cones reach their threshold and begin to function. If the increase in intensity is applied to the center of the fovea where the receptors for red predominate in number, there will be more thresholds crossed for red than for blue-green, and red will be the predominating experience.

CONCLUSION

The factual findings of this experiment are:

(1) In mixing colors on a rotating color-mixer the saturation and hue of the visual sensation depend upon the size and the position of the retinal area stimulated.

(2) The relation between the saturation and the area of the retinal

image is a logarithmic function. If saturation changes by constant increments, the area must change by proportional increments.

(3) This function is continuous over a wide range and extends to a point well within the accepted anatomical limits of the fovea.

(4) The center of the fovea is relatively more sensitive to red than to blue-green, but at the periphery sensitivity to blue-green is relatively greater.

(5) Changes of intensity in the illumination of the entire field have no effect on the relative values of red and blue-green over the range tested, but changes within a small portion of the field have a differential effect upon the predominance of the two colors.

The theoretical assumptions supported by these facts are:

(1) Red and blue-green affect different types of cones or primary receptor units.

(2) These functional receptor units vary in density in different parts of the retina; the red is more concentrated at the center of the fovea and less concentrated in the peripheral areas, as compared with the blue-green.

(3) These units have intensitive thresholds of which the distribution is essentially the same for the two colors.

ANTICIPATORY PLACE-SKIPPING TENDENCIES IN THE MEMORIZATION OF NUMBERS*

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PART I

Ebbinghaus found a certain amount of remote forward association in memorizing lists of nonsense syllables.[1] His method of demonstrating this was to learn lists of syllables made from lists previously learned. For instance, a list made from every third syllable of lists previously learned was learned more quickly than the original lists, but less quickly than a list composed of every second syllable of previously memorized lists. Thus he found that associations were made not only between a given syllable and syllables located later in the list, but that the strength of these associations varied inversely with the distance.

Cason did not find any evidence for forward associations.[2] After learning a difficult prose passage, his *Ss* had just as hard a time learning a list of words composed of every other word in order from the passage (as compared with a list made up of separated words) as they did before learning the passage. After learning an easy prose passage or poetry, his *Ss* showed even greater difficulty in learning the lists of words composed of every other word in order from the passage (as compared with a list made up of separated words) than they did before learning the material. He found little evidence for backward associations.[3] After the rote memorization of lists of nonsense syllables and lists of familiar words, he found it in two out of three cases. He found an inhibition, or greater difficulty in learning the lists, after the memorization of logical prose material. In the lists of familiar words and nonsense syllables, the backward associations probably occurred as errors during the learning of the lists. If the same kind of material had been used for testing forward associations, similar results might have been obtained.

Hall used lists of 10 nonsense syllables.[4] The test material was composed of the odd-numbered syllables from one previously learned list and the even-numbered syllables from another previously learned list. Thus the syllables kept their same relative positions and the syllables from the same list were always separated by just one syllable from another list. When the test lists were given immediately after the learning of the original lists, there was no reliable difference between

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[1] H. Ebbinghaus, *Über das Gedächtnis*, 1885, 130-151.

[2] H. Cason, Specific serial learning: A study of remote forward association, *J. Exper. Psychol.*, 9, 1926, 315-316.

[3] Cason, Specific serial learning: A study of backward association, *ibid.*, 9, 1926, 226-227.

[4] M. E. Hall, Remote associative tendencies in serial learning, *J. Exper. Psychol.*, 11, 1928, 65-76.

the learning of these lists and the learning of the control lists. When the learning of the test material was postponed for a week, however, there was a reliable saving in the learning of the test lists as compared with the learning of the control lists. This work, therefore, harmonized the apparent differences between the results of Ebbinghaus and those of Cason. Cason had tested his immediately and Ebbinghaus had relearned his material after 24 hrs. Hall suggested that a moderate original learning with a period of forgetting may be necessary for the functioning of remote associative tendencies.[5]

Lumley,[6] using mazes and series of numbers or letters, found that many errors were anticipations of what was to come later in the series. The number of times any particular anticipation error was made seemed to be a function of its remoteness from the correct response. He found an inverse curvilinear relation between the degree of anticipation and the frequency of the anticipatory errors. He found no evidence for a similar backward association.[7]

In analyzing the errors made by 11 *Ss* in memorizing lists of 10 three-place numbers by the *anticipation* method, Mitchell found that the number just beyond the correct response was given in whole or part much more frequently than could be attributed merely to chance.[8] Due to the limited number of digits and their frequent repetition within a list, it was impossible to analyze the errors as parts of more remote numbers. Nevertheless, when a whole three-digit response was made that was identical with some number in the list, it was tabulated as an error of "Wrong Position in List."[9] There were 800 of these, at least 799 of which may be considered real anticipations and not merely chance, guesses, for the chances of guessing a three-place number correctly are only about one in a thousand. For this reason, this material is peculiarly well adapted for studying anticipations. In the maze material used by Lumley—and even in his two-place number series—the repetitions within the lists and the small number of possible responses make the chance anticipations so numerous that it is very difficult to segregate the real anticipations.

TABLE I

ANTICIPATION OF NUMBERS MADE BY 11 Ss IN LEARNING 552 LISTS

	Degree of Anticipation										
	1	2	3	4	5	6	7	8	9	10	Total
No. of Responses	23,396	478	77	48	48	44	56	25	15	9	24,196
% of 800 Antic. Errors		59.8	9.6	6.0	6.0	5.5	7.0	3.3	1.9	1.1	

Out of the 800 anticipatory errors found by Mitchell, 478 or 60% were the next number in the list beyond the correct response; in other words, 60% of these errors were anticipations of the second degree. This part of the present report is primarily an analysis of the remaining 322 responses which were numbers more remote in the lists. However, for completeness the correct responses, or those whose degree of anticipation might be said to be one, are also included in Table I. Since these compose 96.7% of the total number of anticipatory responses, the percentages for all the other 9 degrees of anticipation become comparatively too small

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[5] *Op. cit.*, 76.

[6] F. H. Lumley, An investigation of the responses made in learning a multiple-choice maze, *Psychol. Monog.*, 42, 1931, (no. 189), 26; Anticipation of correct responses as a source of error in the learning of serial responses, *J. Exper. Psychol.*, 15, 1932, 204.

[7] *Op. cit.*, *Psychol. Monog.*, 42, 1931, (no. 189), 28; cf. also Anticipation as a factor in serial and maze learning, *J. Exper. Psychol.*, 15, 1932, 333-334.

[8] M. B. Mitchell, Errors in the memorization of numbers, this JOURNAL, 45, 1933, 1-16.

[9] *Op. cit.*, 11 ff.

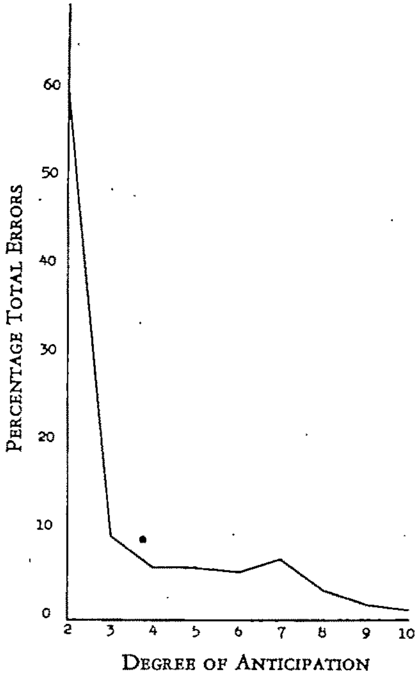

Fig. 1. Anticipation Errors Made by 11 *Ss* in Learning 552 Lists

to be distinguished graphically. Consequently, the correct responses are omitted from the graph. The percentages of the total 800 errors which each degree of anticipation furnishes are included in Table I and shown graphically in Fig. 1. Thus remote numbers do appear as a source of error in the memorization of lists of numbers, at least under the conditions of this experiment. The frequency of the appearance of a number in the list as an error seems to be dependent upon its distance from the correct number. In general, the more remote the number, the less frequently it appears as an error. The fluctuations seen in the graph are probably due to sampling errors.

The results of this experimentation give no evidence of backward associations. There was no time interval elapsing between the end of one trial and the beginning of the next; the last number of the list was the cue to give the first number in the list. Since the numbers were shown in a continuous series with no extra time between trials, any forward association could just as well be considered a backward association. For instance, the ninth degree of forward association would be the same as a backward association of the first degree, and the eighth degree of forward association would be the same as a backward association of the second degree. From the results of Ebbinghaus,[10] one would expect these backward associations of the first and second degree to be greater than the remote forward associations of the sixth and seventh degree. This would cause a rise at the end of the curve, making it somewhat U-shaped. This, however, is not the case; the eighth and ninth degrees of anticipation are smaller than the sixth and seventh degrees. The curve does not rise at the right hand side to indicate the presence of any backward associations—at least, as far remote as the sixth and seventh degree.

The results presented here are essentially in agreement with the remote forward associations found by Ebbinghaus, and the anticipation found by Lumley in the errors made by his *Ss*. It also confirms Hall's finding of remote associations formed during serial learning although he demonstrated their functioning after learning rather than during the learning. The seemingly different results of Cason are probably due to the relative meaningfulness of most of his original material as compared with his test material and the material used by other experimenters. The technique used in this work and in Lumley's does not involve a frustration of associative tendencies as does the technique of Ebbinghaus, Hall, and Cason in breaking up the material for relearning.

PART II

In Part I data are presented to indicate the presence of forward associations in the serial memorizations of numbers by the anticipation method. These were shown to exist by the presence of anticipatory place-skipping errors. In Part II such questions are asked as: At what stage of the learning process do they occur in the learning of the list? Where in the list do they occur? and Whence do they come? *i.e.* where should they occur in the list? These facts are of some significance in view of the theories of serial learning recently set forth by Lumley[11] and Lepley,[12] and considered in Part III.

The question might first be asked as to when these anticipatory or place-skipping errors occur. Are they most frequent when *S* is first beginning to memorize the list, when it is nearly learned, or somewhere in between? By taking all lists with a given number of trials, it was possible to find the number of place-skipping errors during each trial. For instance, 64 lists were found that required exactly 8 trials for learning. In the learning of these lists, 81 place-skipping errors were made; 24 of these were made in the fifth trial, while only 1 occurred in the first trial and none in the eighth. Similarly, when all the lists with 10 trials were analyzed, it was found that the greatest number of place-skipping errors was found in the fourth and sixth trials with 10 each, out of the total of 62. The first trial had only 2 and the last trial none, while the intermediate trials at the beginning of the list steadily increased and those beyond the middle decreased in number of place-skipping errors. The anticipatory place-skipping errors, therefore, seemed to occur most frequently, not near the beginning nor the end of the learning, but in the middle. The number of trials needed in the learning of any list ranged from 3 to 40, so that the number of errors falling under any of these classifications was very small. Even the number of trials occurring most frequently (described above) had only 81 errors to be distributed among 8 trials. Therefore, in this manner, only a rough indication could be obtained of the location of the greater number of these errors.

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[10] Ebbinghaus, *op. cit.*, 151-156.

[11] F. H. Lumley, Anticipation as a factor in serial and maze learning, *J. Exper. Psychol.*, 15, 1932, 341-342.

[12] W. M. Lepley, A theory of serial learning and forgetting based upon conditioned reflex principles, *Psychol. Rev.*, 39, 1932, 283-284.

To obtain more accurate information, the results for all lists were pooled. The learning time was divided into quarters, and the number of place-skipping errors made in each quarter for all lists was tabulated. (See Table II.) Practically the same number of place-skipping errors was found in the second and third quarters of the learning time, and together these two quarters contained two-thirds of all these errors. Hence the errors were most frequent in the middle of the learning.

In analyzing the errors made by these *Ss*, it was often found that there were differences between the *Ss* who learned rapidly and those who learned more slowly.[13] In the first place, although the time for learning was much greater for the slow group than for the fast group (as was also the total number of errors), the number of place-skipping errors was considerably greater for the fast group. This is in accordance with the theory set forth in that article, that the faster *Ss* were learning the numbers as wholes.[14] The fast group had 69% of their errors in the two middle quarters, with about the same number in each. The slow group also had more in these two quarters than in the other two, but the difference was not so great; only 62% were found in the middle. The discrepancy between the two groups is probably explained by the fact that as the number of trials decreased, the absence of place-skipping errors in the last trial, and the very small number in the first trial, became relatively more important in determining the per cent of the total number of such errors in the first and last quarters. For instance, the first trial was the whole first quarter, when only four trials were given, while it was only one-tenth of the first quarter when 40 trials were given. There is, therefore, some evidence that the

TABLE II

PLACE-SKIPPING ERRORS MADE IN EACH QUARTER OF THE LEARNING TIME FOR ALL LISTS

Group	Errors	Quarter				Total
		1	2	3	4	
Slow	No.	68	95	103	56	322
(5)	%	21	30	32	17	
Fast	No.	72	167	154	73	466
(5)	%	15	36	33	16	
Total	No.	141	265	261	133	800
(11)	%	18	33	33	17	

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[13] Mitchell, *op. cit.*, 6 ff.

[14] *Op. cit.*, 7.

place-skipping errors are more frequent in the middle of the learning time than at the beginning or near the end.

Each number that is a place-skipping error has two aspects. It is on occasion a stimulus and a response. It is a stimulus because it is on the drum once during each trial while *S* tries to give the next number in the list. It is also a response to a stimulus, the stimulus being the number exposed on the drum when it is given. Table III is a summary table giving both aspects of the errors for each quarter of the learning time and for the whole time. If we consider the stimulus aspect of these errors, *i.e.* their true position or position where they should have occurred within the list, we see that taking the whole learning time, the errors most frequently come from the seventh, ninth, first, and eighth positions respectively. The reason for the frequency of the first position is that the *Ss* usually learned the first number of the list most easily.[15] They often gave it in the tenth position, especially during the first quarter (frequently even during the first trial), thereby giving it

TABLE III

PLACE-SKIPPING ERRORS FOR EACH QUARTER OF LEARNING TIME

	Quarter	Errors	Position in List										Total
			1	2	3	4	5	6	7	8	9	10	
Stimulus Aspect	1	No.	73	5	9	8	8	6	11	5	7	9	141
		%	52	4	6	6	6	4	8	4	5	6	
	2	No.	26	11	15	27	24	26	43	23	40	30	265
		%	10	4	6	10	9	10	16	9	15	11	
	3	No.	3	8	14	31	26	36	42	44	39	18	261
		%	1	3	5	12	10	14	16	17	15	7	
	4	No.	1	2	11	8	23	7	29	21	21	10	133
		%	1	2	8	6	17	5	22	16	16	8	
	Total	No.	103	26	49	74	81	75	125	93	107	67	800
		%	13	3	6	9	10	9	16	12	13	8	
Response Aspect	1	No.	6	10	7	13	6	8	6	8	11	66	141
		%	4	7	5	9	4	6	4	6	8	47	
	2	No.	6	16	26	34	34	32	29	39	25	24	255
		%	2	6	10	13	13	12	10	15	9	9	
	3	No.	6	15	23	28	40	35	42	36	24	12	261
		%	2	6	9	11	15	13	16	14	9	5	
	4	No.	2	5	4	21	12	26	20	30	10	3	133
		%	2	4	3	16	9	20	15	23	8	2	
	Total	No.	20	46	60	96	92	101	97	113	70	105	800
		%	3	6	8	12	12	13	12	14	9	13	

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[15] Mitchell, The effect of serial position in continuous memorization of numbers, this JOURNAL, 45, 1933, 493-494.

one place too soon and making the most frequent type of anticipation error, that of skipping one place. From verbal reports given by the *S*s, it was evident they thought it was time to begin the second trial. Some of the *S*s resorted to quite overt methods of counting the numbers in order to avoid such errors at the beginning. Even then, during the first quarter of the learning time, the number of these errors, which were numbers belonging in the first position in the series, far exceeds those from any other position in the list. During the second, third, and fourth quarters, the numbers from the later positions in the list were more frequent sources of error. The numbers from the first position were frequent enough, however, to rank third place for the entire learning time.

When we consider the response aspect, the position in which the errors occurred, we see that during the second, third, and fourth quarters of the learning time, the fourth through the eighth positions contained the most place-skipping errors. During the first quarter, the tenth position contained the most place-skipping errors. This has been explained above; it contained the numbers from the first position in the list. They were frequent enough to have the errors in the tenth position rank second place in number of errors for the entire time.

We have seen whence the place-skipping errors came. With the exception of the first position in the list, they came from positions near the end of the list which were learned last.[16] Were these place-skipping errors made before the numbers were learned with their position? This question may be considered statistically from two points of view. In the first place, how frequently was the number given as an error before it had ever been given in its correct position? Of all the place-skipping errors, 385 or 48% occurred before the number had been given in its true position at least once. Thus just about as many place-skipping errors occurred before as after the number had been given correctly, at least once. This was also true when the 5 *S*s who learned the slowest were compared with the 5 who learned the fastest. In the second place, how frequently was the number given as an error after it had been completely learned as judged by the fact that it was given correctly in each succeeding trial? Of all these errors, 556 or 69.5% were made before the number was completely learned in its true position. Thus in the majority of cases, the place-skipping errors had their source in numbers not yet completely learned in their true position, but 30% of them did occur after the number had been learned. Since 48% of the errors occurred before the number was ever given correctly in its true position, and approximately 70% of the errors occurred before the number was completely learned, the difference between these per cents or 22% of the errors might be said to have occurred somewhere during the process of learning the number in its true position.

Summarizing the results, then, the place-skipping errors were shown in Part I to be inversely related to the remoteness from the true position. In Part II, they were shown to be most frequent in the second and third quarters of the learning time. The latter positions in the list (not including the last) were the most frequent location of both the true positions of the numbers in the list and the positions where the place-skipping errors occurred. The only exception was the number from the first position which was frequently given in the tenth position. The place-skipping errors usually occurred before the number was completely learned in its true position, but 30% of the errors occurred after the number had been learned.

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[16] *Op. cit.*, 493.

PART III

The foregoing results raise some theoretical considerations. The presence of remote anticipatory tendencies has great biological significance. As Hull has pointed out, there is a tendency, "where the conditioned stimulus precedes the unconditioned one in the conditioning process, for the reaction to be attracted forward toward the former. Under certain circumstances the reaction (after a number of reënforcements) may begin a considerable interval before the delivery of the unconditioned stimulus. In the case of certain defense reactions this may even result in the organism not receiving the nocuous unconditioned stimulus at all."[17] In the serial memorization of numbers, however, this tendency for the responses to be given too early proves undesirable and must be inhibited. The analogous process in conditioning reflexes Pavlov[18] calls the *inhibition of delay.*

Lumley found that in learning letter mazes, the *Ss* grouped the material, usually in verbal groups.[19] In serial learning of various kinds, *Ss* have grouped the material either arbitrarily or in some especially suitable manner. Lumley concludes that in serial learning, the *Ss* first made rough localizations as near the beginning, near the middle, and near the end.[20] As the learning progresses, they localize more exactly, so that as the classifications contain fewer units, the relative frequency of anticipating the nearer numbers becomes greater and greater. In other words, the ratio of the more remote anticipations to the near anticipations becomes less as the learning progresses.[21] On this hypothesis, it would seem, that *S* would be just as apt to make a response preceding the true response as one following the true response. For instance, if *S* has a rough localization of 6 letters as "near the beginning," then in trying to recall the fourth letter, he might give one of the first three as well as the fifth or sixth. Lumley failed to find any evidence for such backward associations. This lack of evidence seems rather damaging to his theory. The forward associations, however, were more favorable to the theory. In a later article,[22] he summarizes the results of experiments using letter mazes, two-place numbers, a paper maze, and a foot maze, all of which give rather convincing evidence of this change as the learning progresses.

If this is true for all serial learning, then it would seem that an analysis of the 800 place-skipping errors in this report should show a larger ratio for the remote anticipations to the near anticipations in the early part of the learning than in the latter part. Table IV gives a summary of all the place-skipping errors for each degree of anticipation. It also gives the percentage of the total number of errors for each quarter for each degree of anticipation. For instance, while there are only 98 place-skipping errors of the second degree of anticipation in the first quarter, that is 70% of the total place-skipping errors for the quarter. In the second quarter, there are 146 place-skipping errors of the second degree of anticipation, but this is only 55% of the total for the second quarter. The third and fourth quarters each have 59% of the errors classified as errors of the second degree. It is evident, therefore, that the total number of second degree anticipations does not show the expected increase by quarters of the learning time. Nor do the remaining degrees of anticipation show a consistent increase for the smaller degrees of anticipation and decrease for the larger degrees of anticipation. When the ratios for the last four degrees of anticipation (7, 8, 9, and 10) to the first four (2, 3, 4, and 5) are figured for each quarter, there is a rise from the first to the second quarter and a drop from the third to the fourth quarter. This corresponds exactly with the gross number of place-skipping errors found in each quarter. This also fits Lepley's "alternate conception of the nature of the excitatory and inhibitory phases of these remote tendencies."[22]

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[17] C. L. Hull, A functional interpretation of the conditioned reflex, *Psychol. Rev.*, 36, 1929, 499.

[18] I. P. Pavlov, *Conditioned Reflexes,* 1927, 41.

[19] F. H. Lumley, An investigation of the responses made in learning a multiple choice maze, *Psychol. Monog.*, 42, 1931, (no. 189), 20.

[20] *Op. cit.*, 28; Anticipation of correct responses as a source of error in the learning of serial responses, *J. Exper. Psychol.*, 15, 1932, 196-197.

[21] *Op. cit., Psychol. Monog.*, 42, 1931, (no. 189), 28; *J. Exper. Psychol.*, 15, 1932, 205.

[22] Lumley, Anticipation as a factor in serial and maze learning, *J. Exper. Psychol.*, 15, 1932, 342.

TABLE IV

NUMBER AND PERCENTAGE OF PLACE-SKIPPING ERRORS FOR EVERY DEGREE OF ANTICIPATION BY QUARTERS OF LEARNING TIME

	Quarter	Errors	Degree of Anticipation									Total	Ratio
			2	3	4	5	6	7	8	9	10		
All Errors	1	No.	98	16	8	1	4	7	3	1	3	141	.11
		%	70	11	6	1	3	5	2	1	2		
	2	No.	146	25	16	19	18	22	9	7	3	265	.20
		%	55	9	6	7	7	8	3	3	1		
	3	No.	155	20	14	21	13	20	9	7	2	261	.18
		%	59	8	5	8	5	8	3	3	1		
	4	No.	79	16	10	7	9	7	4	0	1	133	.11
		%	59	12	8	6	7	6	3	0	1		
	Total	No.	478	77	48	48	44	56	25	15	9	800	
		%	60	10	6	6	6	7	3	2	1		
Second Degree Errors Occurring in Tenth Position Omitted	1	No.	34	16	8	1	4	7	3	1	3	77	.24
		%	44	21	10	1	5	9	4	1	4		
	2	No.	128	25	16	19	18	22	9	7	3	247	.22
		%	52	10	6	8	7	9	4	3	1		
	3	No.	154	20	14	21	13	20	9	7	2	260	.18
		%	59	8	5	8	5	8	3	3	1		
	4	No.	79	16	10	7	9	7	4	0	1	133	.11
		%	59	12	8	6	7	6	3	0	1		
	Total	No.	395	77	48	48	44	56	25	15	9	717	
		%	55	11	7	7	6	8	3	2	1		

It was pointed out in Part II of this article, that many of the second degree anticipations occurring in the first quarter were located in the tenth position of the list and were due to the fact that the *Ss* thought it was time for the second trial to begin. There was no break between trials; the tenth number was the cue for the first number. The apparent discrepancy, therefore, between these results and those of Lumley, might be due to this continuous presentation of the material for memorization. In all of Lumley's experiments, the trials were quite distinct from the beginning and did not permit this type of error in the first trial. For this reason, the results have been retabulated (lower half, Table IV) omitting all the place-skipping errors of the second degree of anticipation occurring in the tenth or last position of the list. This reduced the percentage of second degree errors in the first quarter from 70 to 44. There is now an increase in the percentages of second degree anticipations from the first quarter to the third and, at least, no drop for the fourth quarter. This suggests that when the errors of the second degree occurring in the tenth position are omitted, the results agree with those of Lumley. In other words, when the data are considered as discontinuous learning, the more remote anticipations seem to decrease and the nearer anticipations to increase with practice. When the data were treated as continuous this phenomenon was not apparent, at least not in the first quarter of the learning.

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[22] W. M. Lepley, A theory of serial learning and forgetting based upon conditioned reflex principles, *Psychol. Rev.*, 39, 1932, 283.

Although Lumley's reports and this one all deal with serial learning, there are many differences. For the most part, he used mazes of various kinds, in which the number of choices at any one time was so limited that it was quite impossible to make any response that could not be considered an anticipation of a true response to follow. In maze learning *S* had to keep on trying after one error until he made the correct response. He was not shown the correct response after one error as he was in learning a list of numbers. This lack of punishment, as it were, in learning numbers might have made a difference in the results. Even Lumley's serial number learning was quite different from that described in this article. He used two-place numbers instead of three-place numbers. It was, therefore, much easier for his *Ss* to anticipate the number correctly by mere chance. This report considers only the anticipations in which all three digits were given correctly and in their proper order. This accounts for the fact that the place-skipping errors were only about 2% of the total number of errors made by the *Ss*. The errors in which only one or two digits were given also showed anticipation. For the second degree of anticipation this was much larger than could be attributed to chance.[34] Due to the small number of digits in our number system, there were so many repetitions in a series of three-place numbers, that it was impossible to consider the source of these responses beyond the second degree. Even in a letter maze, Lumley found it advisable to eliminate some errors from his totals because of this factor of repetition in the material.

With the exception of the foot-maze, Lumley did not use quarters of learning time in handling his material as was done in preparing Tables III and IV. He used rather arbitrarily chosen sections. "The first section contains all the errors made by the subject in the first trial. Here, as one would expect, there is no relation between the amount of anticipations and the number of units skipped. . . . The errors made on the second and subsequent trials were divided into the three sections as evenly as possible, and each point of the maze was considered separately to neutralize the effect of the preponderance of errors near the center of the maze."[35]

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[34] Mitchell, Errors in the memorization of numbers, this JOURNAL, 45, 1933, 8-16.

[35] Lumley, *op. cit., Psychol. Monog.*, 42, 1931, (no. 189), 23.

In an attempt to make the results of the present experiment more comparable with those of Lumley, the 800 place-skipping errors were divided into sections such as those just described (Table V). Since the *S*s in this experiment were not encouraged to guess during the first trial (as Lumley's were), but were merely told to study the numbers as they appeared, the errors in Section I consist primarily of the oft mentioned errors of the second degree of anticipation occurring in the tenth position. The errors in Sections II, III, and IV each consist of the place-skipping errors occurring in one-third of the errors made in learning any particular number.

TABLE V

PLACE-SKIPPING ERRORS BY SECTIONS FOR EACH DEGREE OF ANTICIPATION

Section		Degree of Anticipation									Total	Ratio
		2	3	4	5	6	7	8	9	10		
I	No.	18	1	1		2					22	0
	% 22*	82	5	5		9						
	% 1752†	.24	.01	.01		.03						
II	No.	109	14	10	2	7	9	2	2	3	158	.12
	% 158*	69	9	13	2	4	6	2	2	2		
	% 9447†	1.15	.15	.11	.02	.07	.10	.02	.02	.03		
III	No.	130	24	13	19	13	21	12	4	4	240	.22
	% 240*	54	10	5	8	5	9	5	2	2		
	% 9447†	1.38	.25	.14	.20	.14	.22	.13	.04	.04		
IV	No.	228	36	20	25	26	25	13	6	1	380	.15
	% 380*	60	9	5	7	7	7	3	2	.3		
	% 9447†	2.41	.38	.21	.26	.28	.26	.14	.06	.01		

* Percentage of the total number of place-skipping errors for the section.

† Percentage of the total numer of errors of all kinds for the section. "Ratio" is the sum of the place-skipping errors having a degree of anticipation equal to 7, 8, 9 and 10 divided by the sum of the space-skipping errors having a degree of anticipation equal to 2, 3, 4, and 5. Section I consists of all the errors in the first trial of the learning of any number. Sections II, III, and IV each consist of one-third of all the remaining errors made in the learning of each number.

For instance, if 6 errors were made after the first trial in learning a particular number and the second error was a place-skipping error, it would go into Section II no matter in what quarter of the learning time it occurred. Similarly, if the sixth error was a place-skipping error, it would go into Section IV, even if it occurred in the first quarter of the learning time.

Table V gives the number of place-skipping errors in each section for each degree of anticipation. There is a consistent increase in number of place-skipping errors from Section I to Section IV for every degree of anticipation except the tenth.[26] This is not surprising when it is considered that all three-digit responses increased in number as the learning progressed.[27] The place-skipping errors, as a part of these three-place responses, might be expected to increase. In Part II of this article, they were shown to be most numerous in the second and third quarters. The percentage of the total number of all errors for the section is given in Table V. The results indicate two things: first, they show the increase from Section I to Section IV corresponding to the increase in number of place-skipping errors; and secondly, they show what a small per cent they really are of the total number of errors. The largest number of errors for any degree of anticipation is 228 for the second degree in Section IV, and yet it is only about 2% of all the errors in the section. The percentages of the place-skipping errors in each section for each degree of anticipation do not show a consistent rise from Section I to Section IV.

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[26] Since the tenth degree of anticipation contains very few errors and amounts to reading off the number exposed on the drum, it is quite insignificant.

[27] Mitchell, Errors in the memorization of numbers, Doctorate dissertation on file in the Yale University Library, Appendix E.

The ratio of the last four degrees of anticipation to the first four degrees of anticipation do not show a decrease as the learning progresses. The ratios are 0, 0.12, 0.22, and 0.15. They do not seem to indicate a relative increase in near anticipations and decrease in remote anticipations. It was thought that when the second degree anticipations occurring in the tenth position were eliminated from these sections of learning the same results might be obtained as were apparently found when the quarters of learning time were thus treated. However, the ratios were changed to 0, 0.19, 0.24, and 0.15, which no more show a steady decrease from Section I to IV than those previously obtained using all the errors.

Evidence supporting Lumley's theory, therefore, can hardly be said to be obtained from place-skipping errors occurring in the memorization of three-place numbers. As a particular number is learned, there is an increase in place-skipping errors, but the increase in anticipation is distributed throughout all degrees of remoteness and not confined to the lesser degrees of anticipation. This study, however, is restricted to the errors containing a complete three-digit number. It does not include the anticipations of one- and two-digits alone. If it were possible to calculate them, they might well show a relative increase in near anticipations and decrease in remote anticipations as the numbers were learned.

SUMMARY

In learning lists of 10 three-place numbers by the anticipation method, 800 place-skipping errors were made.

(1) On the whole, the number of the errors for each degree of anticipation was an inverse curvilinear function of its distance from the true position in the list.

(2) No evidence was found for backward associations.

(3) The place-skipping errors were most frequent in the second and third quarters of the learning time as would be expected since at the beginning the *S*s would not know the numbers to give them in the wrong position, and at the end they would only give them in their correct positions.

(4) Most of the place-skipping errors occurred in the serial positions just beyond the middle of the list and were numbers with true positions also located there.

(5) Usually these errors occurred before the numbers had been completely learned in their true position, but, paradoxically, 30% of them occurred after the numbers had been learned.

(6) These data were quite unsatisfactory in giving evidence for Lumley's theory that the more remote degrees of anticipation decrease and the nearer ones increase as the learning progresses. There was instead a relative rise and then decline of remote anticipations.

(7) In learning a particular number, the place-skipping errors of most all degrees tend to increase as the learning proceeds.

THE RELATION OF AFFECTIVE TONE TO THE LEARNING AND RECALL OF WORDS

By M. M. WHITE and MARGARET McLEOD RATLIFF, University of Kentucky

Numerous experiments have been performed to determine the memory value of pleasant and unpleasant words. Of these Meltzer has summarized twenty-six.[1] The results of these experiments are markedly inconsistent. Such lack of agreement indicates that more experiments should be performed in order to establish the facts and if possible to explain the divergent conclusions of previously reported experiments.

EXPERIMENT I

Procedure. A list of 240 words, most of which were taken from the Pressey X-0 test, was given 150 students in beginning psychology with instructions to mark every word either as very pleasant, pleasant, indifferent, unpleasant, or very unpleasant. The students were urged to make their judgments quickly and not to revise any rating. Two weeks later the same students were again given this list of words and instructions. No reference was made to the fact that the words were the same on both occasions. The order of the words was reversed on the second presentation.

From the list of 240 words, 10 pleasant (P), 10 unpleasant (U), and 10 indifferent (I) words were selected. Pleasant and unpleasant words were those on which we found (1) agreement among all *Ss*, (2) agreement between the first and second ratings, and (3) at least one superlative rating by every *S* on one of the two occasions. Since it was impossible to select 10 indifferent words on the basis of these criteria, those words were chosen which most nearly satisfied these conditions. Because the inconsistency of marking makes the classification doubtful, no results are given concerning the indifferent words.

Table I shows the words used in all experiments to be reported. In one experiment no indifferent words were used.

Six different arrangements of the words were designed to offset the advantage of position in the series. In these arrangements the first, second, last, and second last positions were filled twice by P, twice by U, and twice by I words. Intermediate positions were filled by the P, U and I words in a random order which remained the same in all arrangements.

Of the 150 *Ss* who had rated the words, 42 volunteered as *Ss* for the second part of the experiment. They were arbitrarily divided into 6 groups, each group containing 4 men and 3 women. Every *S* in a group was individually presented with one of the 6 arrangements of words described above, by means of the Ranschburg memory apparatus. Instructions to the *Ss* were as follows.

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* Accepted for publication Oct. 31, 1932.

[1] H. Meltzer, The present status of experimental studies of the relationship of feeling to memory, *Psychol. Rev.*, 37, 1930, 127-139.

"I am going to show you 30 words which you are to try to remember. Each will be shown for about 2 sec., and the whole list of words will be presented until you are able to anticipate correctly all the words in the order in which they are shown: that is, you are expected to say aloud the word following the word being shown on the apparatus. Immediately preceding the presentation of the words, you will see numbers 1, 2, and 3. After the first presentation of the entire list, you will be expected upon seeing number 3 to say the first word; upon seeing the first word, to say the second word; and so on through the series until the entire list has been learned. I am going to keep a record of your responses; so speak clearly."

TABLE I

Pleasant	Indifferent	Unpleasant
flower	staple	fever
girl	cover	poison
laugh	ear	insult
sweet	floor	dead
father	bucket	disgrace
lover	answer	vomit
smile	inkstand	terror
beauty	paper	funeral
home	pedal	fear
eat	cotton	insanity

After each presentation 30 sec. elapsed before the words were again shown. Presentations were repeated until *S* correctly anticipated all the words on one trial, or until 30 presentations had been given. Nineteen *S*s anticipated all the words on one trial within the 30 presentations; 23 did not.

E calculated from his record: (1) the number of the trial on which each particular word was first recalled regardless of the correctness of the anticipation; (2) the number of the trial on which a particular word was correctly anticipated; and (3) the number of the trial after which *S* always correctly anticipated the word.

TABLE II

Number of Repetitions Required for Reproduction of Words

	First Anticipation				Beginning of consistent anticipation	
Av. trials	regardless of order		in correct position			
	P	U	P	U	P	U
Entire group	8.99	8.43	11.03	10.61	17.64	17.70
(S.E. Diff.)	(7.15)		(8.48)		(14.04)	
19 Ss	7.14	6.92	8.15	8.05	12.08	12.50
23 Ss	10.52	9.68	13.42	12.72	22.23	22.00
18 women	8.16	7.62	9.83	9.46	15.66	15.90
24 men	9.61	9.04	11.93	11.47	19.12	19.06

Half of the *S*s were asked to return to the laboratory after one, the other half after two weeks. They were not told for what purpose the request was made. On their return each was asked to write all the words remembered.

Results. The results are presented in Table II. The first line shows the results for the group as a whole in terms of the average number of trials required correctly to anticipate the words. The second line shows the same thing for the 19 *S*s who succeeded in correctly anticipating all words on one trial within 30 presentations. The third line shows the results for all the words correctly anticipated by the 23 *Ss* who were unable within 30 presentations to complete the learning task. The fourth and fifth lines present the data distributed according to sex.

The standard error of the difference for the group as a whole indicates that the differences in learning between the P and the U words are negligible.

Attention is called to the trends in the table as they relate to the hypothesis of active forgetting of U words, which we shall find is supported by the experiments reported below. While the U words on the average required fewer repetitions for initial recall for the group as a whole, the consistent recall in correct position began on the average earlier for the P words. The earlier consistent recall for the P words holds not only for the group as a whole, but for the 19 *Ss* who completed the learning task. When the correctness of position of the recalled word is ignored, however, both sexes required fewer trials to recall the U than the P words.

TABLE III

Comparison of Number of Words Recalled by 40 *Ss* According to Entire Group, Sexes, and Interval of Delay*

Period of recall	Av. no. words recalled		
	P	U	Diff.
End of learning period (40 *Ss*)	9.35	9.54	−.19
7 days later (18 *Ss*)	8.11	7.94	+.17
14 days later (22 *Ss*)	6.27	4.81	+1.44
End of learning period (18 women)	8.55	8.72	−.17
7 days later (10 women)	8.70	8.50	+.20
14 days later (8 women)	7.50	6.00	+1.50
End of learning period (22 men)	8.09	8.23	−.14
7 days later (8 men)	7.62	7.00	+.62
14 days later (14 men)	5.57	4.14	+1.43

* Two of the 42 *Ss* were unavailable for recall.

As a whole, nevertheless, Table II shows little or no difference between the average number of trials required to recall P and U words, if the recall is measured in terms of the 3 criteria enumerated above.

Table III shows the data for immediate and delayed recall. Although more U than P words were recalled immediately (9.5 U and 9.3 P), more P than U words were recalled at the end of 7 and 14 days. Furthermore, the preponderance of P over U words is greater after the longer period (a difference of 1.44 words after 14 days as compared to 0.17 words after 7 days).

How are we to explain this difference between immediate and delayed recall? The results, while in accord with the hypothesis of active forgetting, should be considered as little more than suggestive. We therefore made Experiments II and III, which follow immediately, in an attempt to test more thoroughly this hypothesis.

EXPERIMENT II

Procedure. Experiment I was repeated with the following changes. (1) The words were presented a given number of times and not until mastery. (2) The record was kept in terms of the number of words recalled. (3) Longer time intervals were used between immediate and delayed recall. (4) No I words were used. It is to be noted that the criterion of learning used was that which in the previous experiments yielded readier learning for the U words.

The *Ss*, 25 in number, were not taken from the 150 students who had rated the words. We felt, however, that since all of the 150 students had agreed in their marking of the words used, the chances were that these 25 *Ss*, also taken from the psychology classes, would give similar judgments of pleasantness and unpleasantness. To check this supposition, after the experiment was finished the *Ss* were asked to mark a list of 60 words which contained the stimulus-words used. That the difficulty which the *Ss* had experienced in recalling the words did not determine their ratings is indicated by the facts that (1) whereas only 20 of the 60 words were used in the learning, these students' ratings on the entire list of 60 agreed with the ratings of the 150 students; (2) the alleged P words which had been forgotten were not marked U.

TABLE IV

NUMBER OF PLEASANT WORDS RECALLED IN TERMS OF THE PERCENTAGE OF TOTAL WORDS RECALLED

	Interval in days				
	0	7	14	21	28
No. *Ss*	25	7	6	4	7
% P words recalled	52	62	73	72	72

Each of the 20 stimulus-words was typed on a 3 x 5 in. index card and presented in a different random order to each *S*. Each card was exposed for approximately 2 sec. The entire list was presented 5 times. The instructions to the *Ss* were

> "I am going to show you a list of 20 words which you are to try to remember. After you have seen the list 5 times I will ask you to write as many of the words as you remember."

Delayed recall was taken for 7 *Ss* after one week, for 6 after 2 weeks, for 4 after 3 weeks, and for 7 after 4 weeks. One *S* failed to report at the proper time. The *Ss* were not told that there would be a delayed recall.

Results. Table IV gives the P words recalled in terms of the percentage of the total number of words recalled. It will be noted that only 52% of the total words recalled immediately were P, whereas this percentage had increased to 73% after a delay of 14 days. After the 14-day interval there is a change of only 1% in the number of P words recalled.

Table V gives the number of *Ss* that recalled more P than U words for the various periods of delay: 0, 7, 14, 21, and 28 days. It also gives the data for those who recalled more U than P words, and for those who recalled an equal number of P and U words. If we consider the *Ss*, we find, as is to be expected from Table IV, that the number of *Ss* who recalled more P than U words increases through the delay of 14 days and not afterwards. Table V shows the same general trend as Table IV. We may conclude that regardless of whether one considers *Ss* or words, there is an increase in the preponderance of P words through the 14-day delay and not afterwards.

In the first experiment, combining all criteria of learning, we found no difference in immediate recall between P and U words. In this experiment a slight tendency for the P words to be remembered better was revealed. This apparent contradiction may be due to the fact that in one experiment the *Ss* were instructed to master, whereas in the other they were not given such instructions and none of them succeeded in mastering the series. If this reason is correct, we may have an explanation of the inconsistent results of the experiments on affectively toned words. To check this suggestion a third experiment was performed.

TABLE V

Ss' Recalls Compared

	Interval in days				
	0	7	14	21	28
No. Ss recalling more P words	12	5	6	4	6
No. Ss recalling more U words	8	1	0	0	1
No. Ss recalling same no. of each	5	1	0	0	0

Experiment III

Procedure. Two lists of 15 words each were read once to 239 students in beginning psychology, with instructions to learn. Presenting the words only once made complete mastery even less possible than in Experiment II. The purpose of this further reduction in presentations was to determine more fully the effect of partial learning on the forgetting of pleasant and unpleasant words. The students were taken in groups of approximately 20 each. One of the lists began and ended with P words; the other with U words. Each list contained 5 U, 5 P and 5 I words. The affectively toned words were those used in Experiments I and II.

TABLE VI

Comparison of the Words Recalled by 239 Ss

		Words recalled			
Relation of P and U words	No. Ss giving relation	P		U	
		Av.	σ	Av.	σ
P>U	135	6.48	1.17	5.85	1.18
P<U	65			Diff.=0.63	
P=U	39			σdiff.=0.11	

The students were asked to write all the words remembered immediately after the experimenter finished reading the list.

Results. The results are shown in Table VI. The table shows that the majority of *Ss* remembered more P than U words (135 compared to 65). The table also shows that more P than U words were remembered and that the difference in recall is statistically reliable, being 5 times its σ. It seems that if complete mastery is not required more P than U words are recalled. We cannot be certain of this conclusion, however, for in the first experiment, where instructions to master were used, the words were presented visually. Perhaps auditory presentation made the P words easier than the U words, though this seems improbable, especially since in Experiment II where visual presentation was used and the instructions were the same as in Experiment III, more P than U words were recalled.

Discussion

The results of these three experiments support a theory of active forgetting. In the first experiment, practically no difference was found between P and U words in the ease of learning. In the other two experiments a difference was found. Since the first experiment was the only one that contained instructions to master, and approximately 50% of the *Ss* did master, we believe that these instructions caused the difference in the results. Perhaps the mechanism which accounts for active forgetting of the unpleasant also causes the unpleasant to be accepted more quickly than the pleasant when once the organism realizes that the unpleasant is inevitable. This suggestion is borne out by the fact that in Experiment I, the U words were recalled earlier than the P words if the correctness of the position was ignored.

Several important points in these experiments should be emphasized. (1) Every *S* in Experiment I had judged the affective tone of the words on two different occasions. We are therefore safe in assuming that the material used was affectively toned for our *Ss*—an assumption which is made but not justified in several of the experiments summarized by Meltzer. (2) In Experiment I the P and U words were of practically the same difficulty in immediate learning, as evidenced by three related criteria. (3) More P than U words were remembered in delayed recall in every experiment. (4) Roughly, the longer the time interval, the greater the superiority of the P words over the U (Experiments I and II). (5) If the *Ss* are instructed to master completely there is little or no difference in the learning of P and U words.

These experiments suggest several reasons for the divergent conclusions of similar studies.

(1) Uncertainty of affective tone of experiences. Examples are those experiments in which the affective tone of the material is determined after the experiment only. Thus Fox had his *Ss* rate sonnets after they had attempted to learn them.[2]

(2) Considerable variation of the *S*'s acquaintance with the P and U stimuli. Examples are the experiments performed by Wohlgemuth[3] and Laird[4] in which recall of personal experiences was required. Thus event A might have been experienced recently 20 times and event B months ago. In other words the laws of frequency and recency should be considered as well as the law of intensity.

(3) Insufficient time interval before the measurement of recall. Chaney and

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[2] C. Fox, The influence of the subjective preference on memory, *Brit. J. Psychol.*, 13, 1923, 398-404.

[3] A. Wohlgemuth, The influence of feeling on memory, *ibid.*, 405-429.

[4] D. Laird, The influence of likes and dislikes on memory as related to personality, *J. Exper. Psychol.*, 6, 1923, 294-300.

Lauer[5] found practically no difference in the recall of P and U words, using a 5-day interval. We, using a delay of 7, 14, 21, and 28 days, found a rather large difference. We do not believe that active forgetting operates only after a lapse of time, but that the recall method is too crude to be used as a measure of such factors except over relatively long periods. The amount of time required to recognize the words or the number of repetitions required to relearn the words would give a more delicate measure. Lynch,[6] using a recognition technique, found on a carefully evaluated list a tremendous drop in negative critical words after an interval of a week. He also reports very little difference in recognition between the 2- and the 3-weeks delay. This is exactly what we found to be true, using, not recognition, but recall.

(4) Different instructions to *Ss*. Thus in Experiment I in which instructions to master were used, there was little or no difference in the learning of P and U words. In the other two experiments in which instructions to master were not employed we found a difference.

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[5] R. M. Chaney, and A. R. Lauer, The influence of affective tone on learning and retention, *J. Educ. Psychol.*, 20, 1929, 287-291.

[6] C. A. Lynch, The memory values of certain alleged emotionally toned words, *J. Exper. Psychol.*, 15, 1932, 298-315.

THE RELATION OF TONAL VOLUME, INTENSITY, AND PITCH

By PAUL M. ZOLL, Harvard University

The purpose of this study is, first, to exhibit the instability of tonal volume as a category of sensory discrimination, as shown in the literature and by new experiments of the writer and, secondly, to make a preliminary report of some observations which show the dependence of sensory intensity upon frequency.

Tonal volume. Observers can make judgments of tonal volume, when the frequency of the tonal stimulus is varied, and also when its energy is varied. An *O* can remain fairly consistent in his judgments under the conditions of the psychophysical experiment during a long series of experimental sessions, giving quantitative results that fall into psychometric functions and even larger systematic relationships.[1] On the other hand, the concept of tonal volume has been criticized as lacking psychological reality or at least scientific definiteness.[2] Since the limens for volume are always greater than the limens for pitch when frequency is varied and greater than the limens of intensity when energy is varied, it can be argued that volume is simply a term for a high degree of subjective certainty about pitch or intensity, as the case may be, or some similarly indeterminate category of difference. The measure of the value of such a concept is its stability under varying experimental conditions. If tonal volume can be measured and quantitative functions for it can be made out, and if such results can be verified from laboratory to laboratory, then volume has attained a definite descriptive status, and it is defined by whatever formulation in one published account enables a second investigator to duplicate the experiment and verify the results. On the contrary, if consistent quantitative results are not easily obtained, or if, having been obtained, they are not capable of verification by a new *E* with new *Os* in another laboratory, then it is plain that the original observations depend upon some factor that is not yet explicit in the formulation of the experiment, and that the concept as yet lacks scientific reliability and value.

Table I shows values of the volumic limen as obtained by various investigators for a standard in the middle register. As long as the determinations by Rich[3] and Halverson[4] were not supplemented by others it could be said that there was a fair amount of consistency, that tonal volume was a fairly stable category. The results

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* Accepted for publication October 15, 1932. This investigation was conducted under the direction of Professor E. G. Boring.

[1] *E.g.,* G. J. Rich (A preliminary study of tonal volume, *J. Exper. Psychol.,* 1, 1916, 13-22) showed that a series of volumic limens, each derived from a psychometric function, may approximate a logarithmic function of frequency throughout a wide range of frequencies (110-6400 cycles).

[2] Cf. H. Banister, Auditory theory, a criticism of Professor Boring's hypothesis, this JOURNAL, 38, 1927, 438-439.

[3] G. J. Rich, *op. cit.;* A study of tonal attributes, *ibid.,* 30, 1919, 121-164.

[4] H. M. Halverson, Diotic tonal volumes as a function of difference of phase, *ibid.,* 33, 1922, 526-534; Tonal volume as a function of intensity, *ibid.,* 35, 1924, 361-367.

of Gundlach and Bentley,[8] however, disturb this harmony. They find very small values for the interval of uncertainty, and the data depend upon a body of experimentation much greater than Halverson's, which was merely preliminary to other experiments. Then some of the writer's own results now fall in with the range that Gundlach and Bentley found. Altogether it can not be said that the table shows the kind of consistency wanted.

TABLE I

SUMMARY OF RESULTS OF VARIOUS INVESTIGATORS ON DIFFERENTIAL LIMENS FOR TONAL VOLUME

The last three columns show respectively the lower differential limen, the upper differential limen, and the interval of uncertainty

Investigator	Date	Instrument	Standard (cycles)	O	DL_L	DL_U	I.U.
Rich	1916	Variator	400	*B*	12.18	14.00	26.18
				D	12.66	14.88	27.54
Rich	1919	Variator with interference tubes.	550	*Da*			(26)*
				Di			(24)*
				G			(26)*
				W			(18)*
				Y			(20)*
				O	1.13	1.03	2.16
Halverson	1923	Variator	396	Av.			(28.6)*
Halverson	1924	Variator	396	Av.			(36.2)*
Gundlach and Bentley	1930	Variator	500	*B*	1.43	.62	2.05
				C	5.04	5.10	10.14
				F	.63	.13	.76
				G	.86	.78	1.64
				H	2.62	2.86	5.48
				I	3.02	3.45	6.47
				Mc	9.34	3.84	13.18
				M	.33	.57	.90
				S	2.44	3.41	5.85
				Z	1.47	.89	2.36
				Av.	2.72	2.17	4.88
Zoll (1)	1931	Variator	396	*F*	4.63	3.56	8.19
				V	4.02	2.37	6.39
				B	5.86	6.02	11.88
Zoll (2)	1932	Oscillator	345	*V*	9.32	6.75	16.07
Zoll (3)	1932	Oscillator	345	*El* *Ob*	Undeterminable; excessive irregularities.		

* Where only one DL is available, the value for it has been doubled and entered in parentheses in the I.U. column, to allow a comparison of all the results in this column.

The complete data of the writer's experiments on the dependency of tonal volume upon frequency are given in Table II.

In a first experiment (Table II-1) a Stern variator was used in order that the conditions of previous investigators might be duplicated. The standard tone was selected at 396 cycles. The method of constant stimuli was employed and the results were computed by Urban's tables. There were three trained *Os*: Mr. J. D. Frank (*F*), Dr. John Volkmann (*V*) and Professor E. G. Boring (*B*). The intervals of uncertainty proved to be much smaller than Rich's or Halverson's, although somewhat larger than those of Gundlach and Bentley. It is plain that the desired stability for tonal volume has not yet been achieved, although the results for these three *Os* are comparable and their volumic limens are clearly not to be confounded with limens for pitch, which the table shows for *F* and *V*.

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[8] R. Gundlach and M. Bentley, The dependence of tonal attributes upon phase, *ibid.*, 42, 1930, 519-543.

TABLE II

VOLUMIC LIMENS AS A FUNCTION OF FREQUENCY

The last five columns show the measures of precision (h) for the lower and upper limens, the values of the lower and upper differential limens (DL), and the interval of uncertainty (I.U.). At the bottom of the tables are given the psychometric functions for *El* and *Ob* since their limens could not, on account of variability, be computed in Experiment 3. All values entered in the table (except no. of series) are for cycles per sec. In Experiment 1 two pitch limens were determined for the sake of comparison with the volumic limens.

Exp.	Standard (cycles)	Category	*O*	No. of series	h_L	h_U	DL_L	DL_U	I.U.
(1)	396	Volume	*F*	100	.10	.10	4.63	3.56	8.18
			V	100	.09	.08	4.02	2.38	6.39
			B	50	.08	.01	5.86	6.02	11.88
		Pitch	*F*	35	.35	.36	2.11	1.27	3.38
			V	50	.70	.55	1.03	0.56	1.59
(2)	345	Volume	*V*	50	.51	.50	9.32	6.75	16.07
(3)	345	Volume	6 untrained Os: no judgments obtainable						
			El	25	No limens obtainable because of excessive				
			Ob	25	irregularities.				

	El							*Ob*						
Comparison Stimuli	360	355	350	345	340	335	330	360	355	350	345	340	335	330
Percentages Larger	80	48	28	12	68	64	100	36	28	0	0	100	16	88
Percentages Smaller	20	8	12	12	4	8	0	60	28	68	8	0	72	8

In the second investigation (Table II-2) the source of sound was a single ear-phone placed at the level of the ears 3 ft. behind *O*'s head; the ear-phone was activated by a General Radio 508-A audio-oscillator placed in the adjoining room. The arrangement of the oscillator with the accessory apparatus, as shown in Fig. 1, permits the preparation of two tones, a standard and a comparison tone, differing in frequency or energy (the voltage across the ear-phone delivering the sound) or both, before the presentation of either tone. A standard tone with a frequency of 345 cycles and an output energy of 5.0 volts was chosen. *V* was the only *O*. It will be noticed that in this case the volumic limen is over twice as large as that determined for the same *O* in the first experiment. This increase indicates again the instability of tonal volume as a category of report.

In the third experiment (Table II-3) the apparatus was the same as in the second, except that the *O*'s head was kept immovable with the ear-phone only one foot behind it, and that a standard tone was chosen of 345 cycles frequency and 2.0 volts output energy. In this work were used two trained *Os*—Dr. M. H. Elliott (*El*) and Mr. K. W. Oberlin (*Ob*)—and six untrained *Os*—Miss Duckering (*Du*), Miss Robinson (*Ro*), Miss Lathrop (*La*), Miss Biddle (*Bi*), Mr. Bolles (*Bo*), and Mr. Dearstyne (*De*). The attempt to determine volumic limens in this case was completely unsuccessful. The untrained *Os* could not understand what was meant by tonal volume; they identified volume with intensity or pitch or else were utterly unable to fix upon any criterion for judgment. The two trained *Os* were able, after some preliminary practice, to recognize volume and give judgments upon it, although *El* had considerable difficulty with his reports and had little assurance that the basis of his judgments was 'volume.' The results obtained for *El* and *Ob* were still too irregular to allow limens to be computed from them; the percentages of report by them are given at the end of Table II to show the nature of the inversions encountered.

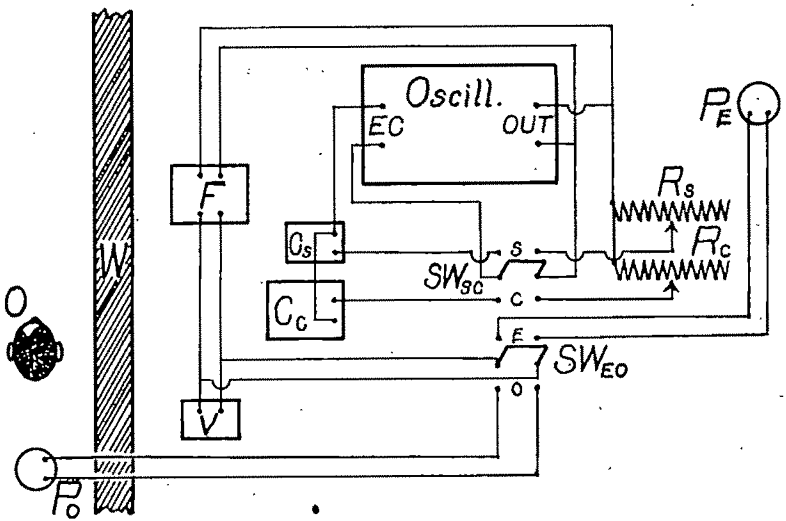

FIG. 1. OSCILLATOR CIRCUIT

Oscill.: audio-oscillator, 508-A.
Out: output leads from oscillator.
R_O: resistance for comparison stimulus.
R_S: resistance for standard stimulus.
EC: external capacitance leads from oscillator.
C_O: capacitance for comparison stimulus.
C_S: capacitance for standard stimulus.
SW_{SO}: double-throw switch, for standard stimulus (S) and for comparison stimulus (C).
F: low pass 500-cycle filter, to eliminate telephone diaphragm resonance at 800 cycles.
V: voltmeter across output to control intensity.
SW_{EO}: double-throw switch, for *E*'s phone (E) and for *O*'s phone (O)
P : Phone for *E*.
P : Phone for *O*.
O: observer.
W: wall of sound-proof room.

A consideration of the data of these three experiments—the variability, the irregularities, and even the indeterminateness of the volumic limen—indicates that some factor, implicit in and not completely controlled by the experimental procedure, has disturbed the consistency of the quantitative results.

Boring[6] has suggested that volume may depend upon the amplitude of the stimulus, intensity upon the energy of the stimulus, and pitch upon the frequency. Thus the apparent dependence of volume upon frequency might be due to the fact that for a constant expenditure of energy the amplitude would vary with the frequency. Such an hypothesis would explain the present situation where results are more consistent from *O* to *O* within the same experiment than from one experiment to another, since the expenditure of energy is constant but undetermined within a single experiment. But this hypothesis also requires a higher order of consistency when volume is investigated as a function of energy with frequency constant. Accordingly the writer undertook to repeat Halverson's results on the relation of tonal volume to intensity.[7]

In determining intensitive and volumic limens as a function of energy, a 6-volt audio-oscillator of a fixed 1000 cycle frequency was first used to activate an earphone placed behind the *O*. A slide rheostat inserted in the circuit permitted the presentation of weaker comparison tones only (as in Halverson's experiment); consequently, only lower limens could be procured. The changes in energy were read by a millimeter-scale on the rheostat from 0 mm. (the energy of the standard tone), to a minimal intensity of 200 mm. The resultant data are given in the first six lines of Table III.

The addition of another similar rheostat in a second part of this experiment made it possible to present more energetic as well as less energetic comparison stimuli. The limens, obtained from these results only in part by the use of Urban's tables, appear in the next four lines of Table III; those limens for which no *h*'s appear, were determined by Newhall's method of interpolation.[8]

In a second experiment the 508-A audio-oscillator with the apparatus described above for separate control of frequency and energy was used as the source of sound. With a standard tone of 345 cycles and 5.0 volts the volumic limen as a function of energy was determined for *V*. The results appear in the second section of Table III.

In a third experiment the 508-A audio-oscillator was used with a standard tone of 345 cycles and 2.0 volts. Since the six untrained *O*s could make no judgments at all upon volume, with variation either of frequency or energy, limens for intensity were procured instead. Satisfactory reports upon volume were, however, given by the trained *O*s, *El* and *Ob*. The results of this experiment appear in the third part of Table III.

It is not, of course, possible to compare the results, even of the first experiment, with Halverson's, since an arbitrary scale of mm., measuring a rheostat, gives no determinate statement of energy. Table III, however, shows at once that the desired consistency for these limens is not forthcoming. While the limens for volume are greater than the limens for intensity, there is great variability among the *O*s, and, what is even worse, the psychometric functions are so irregular and adventitiously variable, that in some cases it was not even practicable to compute the limens by Urban's method. We also see that untrained *O*s were again incapable of using volume as a consistent category of report.

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[6] E. G. Boring, Auditory theory with special reference to intensity, volume, and localization, *ibid.*, 27, 1926, 162-164.

[7] H. M. Halverson, *op. cit.*, 1924.

[8] S. M. Newhall, Linear interpolation *vs.* the constant process, this JOURNAL, 38, 1927, 390.

In short, all these experiments upon tonal volume, as a function of frequency and of energy, have exhibited the instability of tonal volume as a category of sensory discrimination.

Intensity. The irregularity of the results obtained for the discrimination of tonal volume as a function of frequency led to some observations upon the stability of sensory intensity with the variation of frequency. It is known that sensory intensity changes when the pressure of the stimulus (dynes/cm.^{2}) is kept constant and the frequency is changed.[9] In the frequency range of the present investigation the rule is: the greater the frequency the louder the tone if pressure is constant—a rule that resembles Helmholtz' "intrinsic intensity" of high pitches.

TABLE III

VOLUMIC AND INTENSIVE LIMENS AS A FUNCTION OF ENERGY

See Table II for the meaning of the symbols. In Experiment 1 energy was varied with a rheostat and values entered in the table are mm. of the rheostat scale. In Experiments 2 and 3 energy was measured by the output voltage of the oscillator and the values are in volts.

Exp.	Standard	Category	No. of series	O	h_L	h_U	DL_L	DL_U	I.U.
(1)	0 mm.	Volume	50	*F*	.01		162.64		
			50	*V*	.02		47.21		
			50	*B*	.02		27.76		
		Intensity	25	*F*	.01		43.02		
			25	*V*	.09		2.15		
			25	*B*	.03		9.81		
	0 mm.	Volume	30	*F*			157.00	200.00	
			25	*V*			68.00	78.00	
		Intensity	25	*F*			114.00	97.00	
			25	*V*	.03	.03	14.60	15.70	
(2)	5 v.	Volume	50	*V*	2.40	2.40	.36	.40	0.77
(3)	2 v.	Intensity	25	*Du*	4.23	4.63	.10	.11	0.21
			25	*Ro*	2.33	2.17	.12	.02	0.14
			25	*La*	3.79	3.39	−.01	.09	0.09
			25	*Bi*	2.27	2.83	.04	.11	0.14
			25	*Bo*	3.30	3.99	.15	.11	0.26
			25	*De*	3.51	3.60	.04	.15	0.19
		Volume	25	*El*	1.35	1.44	.57	.60	1.18
			25	*Ob*	3.39	3.72	.26	.22	0.48

The 508-A audio-oscillator with separate control of frequency and energy was used as in Fig. 1. A tone of 345 cycles and 2.0 volts was kept throughout as the standard. The *O*s were those of the third experiment upon volume.

In a first experiment reports on intensive differences were procured for *Bo, De, La,* and *El,* with comparison tones varying in frequency and of a fixed energy of 2.0 volts for both standard and comparison. If variation of frequency alone had no effect upon intensity, we should expect a large percentage of 'equal' reports for all stimuli with only a small number of equally distributed judgments of 'louder'

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[9] B. A. Kingsbury, A direct comparison of the loudness of pure tones, *Phys. Rev.*, 2 ser., 29, 1927, 588-600; D. MacKenzie, The relative sensitivity of the ear at different levels of loudness, *ibid.*, 2 ser., 20, 1922, 331-348.

TABLE IV

Percentages of Report of 'Louder,' 'Equal,' and 'Softer' as a Function of Frequency

The standard stimulus is 345 cycles at 2.0 volts; the comparison stimuli vary from 330 to 360 cycles at 2.0 volts.

O	Report	Comparison stimuli						
		360	355	350	345	340	335	330
Bo	Louder	84	88	0	0	56	0	76
	Equal	16	12	48	96	40	40	24
	Softer	0	0	52	4	4	60	0
De	Louder	84	52	12	8	44	4	84
	Equal	16	8	20	72	20	8	8
	Softer	0	40	68	20	36	88	8
El	Louder	36	52	16	0	32	0	32
	Equal	48	48	76	96	56	36	68
	Softer	16	0	8	4	12	64	0
La	Louder	95	60	95	10	40	5	0
	Equal	0	0	0	70	0	0	5
	Softer	5	40	5	20	60	95	95

and 'softer.' Table IV, however, in which these percentages are recorded, shows that this does not occur. Of the four *O*s, *El* alone reported a large amount of 'equal' judgments, and, even in his case, the percentages of reports of 'louder' and 'softer' for several frequencies are significantly different. All the *O*s agreed in reporting tones of 360 and 355 cycles as 'louder' most of the time, 345 as 'equal,' and 335 as 'softer,' in comparison with the standard tone of 345 cycles and of equal voltage. This result clearly indicates that tones of 360, 355, and 335 cycles differ in sensory intensity from one of 345, even though the output energy across the phone is maintained constant.

TABLE V

Intensity as a Function of Voltage with Frequency Constant

O	Fixed comparison (cycles)	Report	Comparison stimuli (volts)					h	DL	I.U.	S.E.
			1.8	1.9	2.0	2.1	2.2				
Du	330	Louder	0	4	32	32	64	3.89	.14		
		Equal	20	16	44	44	20			.17	2.06
		Softer	80	80	24	24	16	3.72	.03		
	360	Louder	16	44	52	68	80	2.96	−.01		
		Equal	28	40	32	24	20			.22	1.88
		Softer	56	16	16	8	0	3.58	.21		
Ro	335	Louder	4	12	24	24	40	2.08	.30		
		Equal	12	28	20	32	24			.27	2.13
		Softer	84	60	56	44	36	3.93	−.04		
	355	Louder	32	20	72	68	84	2.92	−.04		
		Equal	44	56	16	20	12			.41	1.81
		Softer	24	24	12	12	4	1.65	.45		

To determine this point more conclusively intensitive limens for *Du* and *Ro* were determined with the comparison stimuli at different frequencies from the standard, and the effect of difference in frequency was noted (see Table V). For *Du* the comparison stimuli were fixed at frequencies 330 and 360 cycles, and for *Ro* at 335 and 355 cycles, with the standard always, of course, at 345 cycles and 2.0 volts. It is seen that tones of 360 and 355 cycles are much louder and tones of 330 and 335 are much softer than the standard of 345. These results, except perhaps those with 330 cycles, are in agreement with the conclusions drawn from the results given in Table IV.

Thus it appears that these results are in accord with the general rule that higher frequencies, within the lower frequency ranges, tend to be more intense.

THE EFFECT OF HUNGER ON VARIABILITY OF PERFORMANCE

By M. H. Elliott, Harvard University

The usual learning experiment with rats may be considered to deal with the changes in the initial random behavior which result from differential rewards; that is to say, certain of the responses are rewarded and others are punished or not rewarded. The distinguishing feature of the present experiment lies in the fact that the animals are always rewarded in the same manner regardless of the path taken. Under these conditions the question arises as to whether variations in the strength of hunger will be followed by changes in the variability of performance.

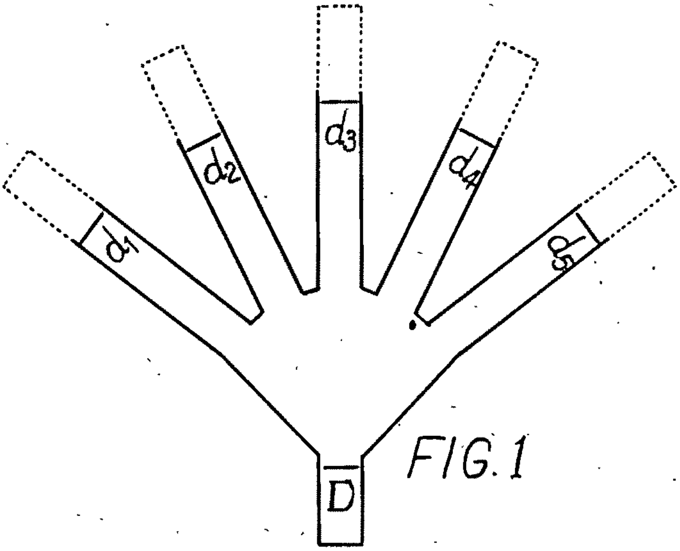

FIG. 1

Fig. 1. Floor-Plan of Apparatus

Apparatus. The floor-plan of the apparatus is shown in Fig. 1. There were 5 alleys leading from the large end of a wedge-shaped compartment. The rat was placed in the entrance-box. When the door (D) was opened the rat could enter the choice-compartment and from there proceed down any one of the 5 alleys. At the end of each alley was a small wire cage containing food. When the rat had entered one of these food-cages the appropriate door (d_1, d_2, etc.) was closed.

The sides of the apparatus were made of sheet metal and the top was covered with wire-mesh. The flooring was made of rubber composition. The doors were sheets

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of metal sliding in felt-lined vertical grooves. These doors were operated by strings passing over pulleys and were practically noiseless. Indirect lighting provided a dim and fairly uniform illumination. The apparatus was placed so that the entrance-box protruded through an opening in a large wall-board screen. *E* was stationed behind this screen and watched the animal through a narrow slit.

Special precautions were taken to prevent a rat from trailing the path of the previous rat. The flooring was thoroughly brushed with a damp brush after each trial and clean flooring was laid down after every four or five trials.

Daily procedure. Each rat was given two trials per day and was allowed to eat for 2½ min. at the end of each trial. A period of 15–20 min. intervened between the two trials and other rats were run during this time. After the two trials the rat was returned to the living-cage in another room and 45 min. later was given a measured amount of additional food to make up the daily ration.

Variations in hunger. The variations in hunger were determined by the amount of food given at the later feeding in the living-cages. Four different degrees of hunger were used. Three of these corresponded to daily rations of 4, 6, and 10 grm. per animal. The fourth and least degree of hunger was produced by allowing the animal to eat freely from 45 min. after the trials on one day until 45 min. before the trials on the next day.

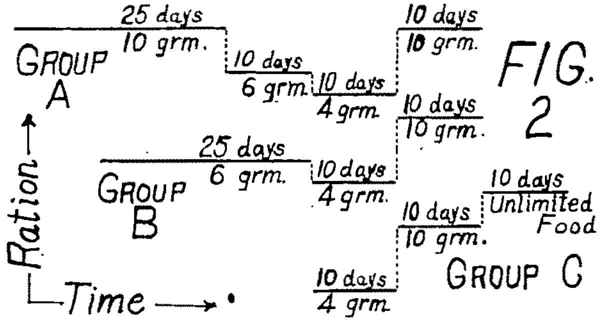

FIG. 2. RATIONING PLAN FOR GROUPS A, B, AND C

Experimental groups. The rats used in the experiment varied in age from 85 to 100 days at the beginning of training.

Fig. 2 shows the way in which the rations were varied in successive increments of time for the three groups, A (15 rats), B (14 rats), and C (20 rats). Each day represents two trials. It will be seen that Groups A and B were given similar treatment in that hunger was increased in successive periods until after 10 days on the 4-grm. ration and then decreased. Moreover, conditions for these two groups were identical for the last 30 days of training. Group C, on the other hand, started with maximal hunger, and then the hunger was decreased to a minimum in the last 10 days.

Statistical treatment. The data obtained consisted of records of the alleys chosen by each rat in the various trials. The alleys were arbitrarily numbered from 1 to 5. Thus Rat No. 1 of Group A gave the following distribution of choices during the initial period of training on 10 grm. per day:

Alley No.	1	2	3	4	5
Choices	2	2	11	14	21

Treating this array as an ordinary distribution one obtains a standard deviation of 1.088, which may be considered as one measure of the variability of this rat's performance during this period. In this way a variability score (σ) was obtained for each rat for each of the experimental conditions. These derived measures were then treated in the usual manner: means and standard deviations were computed and comparisons made, as shown in the tables. The appropriate formulae for use with a small number of cases were used. In obtaining the σ diff. the complete formula for correlated measures was used.

In order to approach the data from a slightly different point of view a statistical study was made of the relative preference for paths. This result was achieved by taking for each rat the path most often selected during the whole training period and then determining the percentages of choices of this path under the various experimental conditions. Thus it was found that over the whole period of training Rat No. 1 of Group A entered Alley 4 more often than any other. From the distribution given above we find that in the initial period of training on 10 grm. per day this rat selected Alley 4 28% of the time. In the following periods the percentages were respectively, 25, 70, and 80. From these percentages of choices of preferred path for each rat distributions were made for the different groups under the various experimental conditions. Means and standard deviations were computed and comparisons made as before.

TABLE I

Variability of Performance

Group		10 grm.	6 grm.	4 grm.	Terminal 10 grm.
A	Mean	1.459	1.283	.998	.848
	S.D.	.448	.560	.620	.623
B	Mean		1.452	1.081	1.189
	S.D.		.333	.735	.560

Although this second procedure does not give so direct or dependable a measure of variability of performance, it nevertheless provides an indication of the consistency of preference.

Results. Table I presents the means and standard deviations of the variability of performance for Groups A and B under the various conditions of hunger. It should be remembered that these means are means of distributions of standard deviations. Likewise, the standard deviations are standard deviations of distributions of standard deviations.

It will be seen that the variability (mean) becomes smaller with the increase in hunger. Contrary to expectations, however, the variability did not return to its former level when the hunger was decreased (terminal 10 grm.)

Table II presents for the various conditions of Groups A and B the differences in the mean variability of performance and also gives measures of the significance of these differences.

The data in Table II show that there is a fairly significant decrease in the variability of performance when the hunger is made sufficiently strong. This change in variability appears to have a certain permanence as it is not greatly affected by the later decrease in hunger.

Group C was intended to serve as a control to determine whether the change in variability could be ascribed to mere repetition. It will be remembered that this group started with maximal hunger and that the hunger was *decreased* in successive periods. The results for Group C are shown in Tables III and IV.

TABLE II

DIFFERENCE IN VARIABILITY OF PERFORMANCE

Group		10 vs. 6 grm.	10 vs. 4 grm.	10 vs. term. 10 grm.	4 vs. term. 10 grm.
A	Diff.	.176	.461	.611	.150
	D/σ diff.	1.354	3.013	3.748	1.250
		6 vs. 4 grm.	6 vs. term. 10 grm.	4 vs. term. 10 grm.	
B	Diff.	.371	.263	.108	
	D/σ diff.	2.876	2.121	.651	

It is apparent that the differences in Table IV can not safely be regarded as significant. There is little evidence of an effect on variability resulting from repetition. As a further check the variability of the combined Groups A and B on the 4-grm. ration was compared with that of Group C on the 4-grm. ration. This comparison gave a difference of 0.141 with the D/σ diff. of 0.671. Again there is indication that the amount of previous training had little effect on the variability of performance.

TABLE III

VARIABILITY OF PERFORMANCE

	4 gms.	10 gms.	'Unlimited' food
Mean	.903	.788	1.092
S.D.	.513	.721	.708

The data on the percentages of choices of path most preferred over the whole training period are summarized in Table V.

The differences given in Table VI appear to have approximately the same significance as those in Tables II and IV. The decrease in variability of performance shown in the earlier tables is paralleled by a tendency for each rat to select more often some particular path. This relative preference is little affected by differences in training prior to the period of maximal hunger. After the period of maximal hunger a decrease in hunger does not change the preference.

TABLE IV

DIFFERENCE IN VARIABILITY OF PERFORMANCE

	4 vs. 10 grm.	4 grm. vs. unlimited	10 grm. vs. unlimited
Diff.	.115	.189	.304
D/σ diff.	.248	.390	.704

Conclusions. When rats in the multiple-choice apparatus were sufficiently hungry, each rat tended to select some particular path on successive occasions in spite of the From these data some of the more important comparisons are given in Table VI. fact that all choices were equally rewarded. In the case of Group A it was found that many repetitions with insufficient hunger had little effect on the variability of performance. The variability of performance is inversely related to hunger but the change in variability of performance is not reversible. That is to say, after variability has been reduced under conditions of strong hunger, it is found that decreases of hunger have little effect. Furthermore, the amount of previous training does not greatly affect the variability under any particular degree of hunger.

TABLE V

PERCENTAGE OF CHOICE OF PREFERRED PATH

Group		10 grm.	6 grm.	4 grm.	10 grm.	Unlimited
A	M	51.3	54.7	66.8	70.0	
	S.D.	20.1	22.6	25.3	25.8	
B	M		50.9	70.0	68.6	
	S.D.		21.1	26.9	20.3	
A+B	M		52.8	68.6	69.2	
	S.D.		21.6	25.7	22.4	
C	M			74.3	81.0	71.5
	S.D.			26.5	20.1	28.8

Discussion. The relationships between hunger and variability of performance do not appear to have been explicitly treated in the literature. Experiments with activity-wheels indicate that there is a relationship between hunger and *amount* of activity. Moreover, various studies have shown that efficiency of learning in the maze is positively related to strength of hunger; but this relationship also must be due to the greater activity of the hungrier animals. Neither type of study then has been concerned with stability of performance. Stated roughly it can be said that the experiments with activity-wheels and mazes have been dealing with the number of actions per minute as related to hunger. On the other hand, the present experiment has dealt with the persistence of some particular sequence of actions as it is related to hunger.

TABLE VI

DIFFERENCES IN PERCENTAGE OF CHOICE

Comparison	Diff.	D/σ diff.
Group A+B, 6 vs. 4 grm.	15.8	2.424
Group A+B, 4 vs. terminal 10 grm.	0.6	.091
Group A, initial 10 vs. A+B, terminal 10 grm.	17.9	2.609
Group C, 4 vs. 10 grm.	6.7	.903
Group C, 4 grm. vs. unlimited food	2.8	.321
Group A+B, 4 grm. vs. Group C, 4 grm.	5.7	.730
Group C, 10 grm. vs. Group A, initial 10 grm.	29.7	4.329

The usual trial-and-error account of learning seems to imply that the state of motivation results in random and variable behavior. Presumably, there is also the implication that the stronger the state of motivation, the more variable will be the behavior. However reasonable such a relationship may appear, if it exists at all, it can exist only until the situation yields some reward, for the results of the present experiment indicate that after the reward there is an inverse relationship between strength of hunger and variability of performance. Moreover, it seems that this relationship is not reversible.

This irreversibility of the relationship, shown by the fact that, after the behavior had been affected by strong hunger, it was not changed by later decreases of hunger, brings the experiment into line with other learning experiments. Gilhousen[1] and Krechevsky and Honzik[2] have shown that repetition has the effect of fixating habits. The results of this experiment indicate the possibility that strong motivation is another fixating agent.

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[1] H. C. Gilhousen, An investigation of insight in rats, *Science*, 73, 1931, 711-712.

[2] I. Krechevsky and C. H. Honzik, Fixation in the rat, *Univ. Calif. Publ. Psychol.*, 6, 1932, 13-26.

THE RELATIONSHIP BETWEEN TRANSFER OF LEARNING AND AGE OF PREVIOUS ASSOCIATIONS

By STEUART H. BRITT, Washington University

The phenomenon of transfer of training, as treated by Carr,[1] refers to the utilization of past experiences in dealing with new situations. The effect of training in the earlier activity may be (1) positive, facilitating the formation of the subsequent habit; (2) negative, inhibiting the formation of the second habit; or (3) of no effect at all.

There may be several factors in the first learning situation which will be conducive to positive transfer and several other factors conducive to negative transfer. Whether the learning of the first problem aids, hinders, or has no influence upon the learning of the second problem is probably a matter of the balance between the positive and negative transfer elements.

The present investigation represents a study of the relationship between the transfer effect of one maze upon another and the age of the associations in the first problem. In another laboratory experiment, concerned with Jost's Law,[2] it was discovered that two groups of *Ss* differed greatly in the efficiency exhibited in mastering a second maze when, in one case, the first problem had been learned to a definite criterion immediately preceding the second, and, in the other case, the immediately preceding activity had been the relearning of the first maze to the same criterion after an interval of 48 days. A difference between the two age-groups in the ease with which the second maze was learned is what one would have expected. However, as a control, it was thought worth while to have a third group master the second maze problem without having had any previous maze experience. This was done in order to determine the exact amount of difference in transfer when the same level of efficiency has been reached on the first maze problem but when the associations in one group are older than in another group.

Conditions of experiment. The learning problems were two stylus mazes, I and II. Maze I was the original learning problem, a brass maze, approximately 14½ by 14¼ in., of the same pattern as that used by Carr in his experiment on visual guidance.[3] Maze II, the second problem, was made of fiber-board, approximately 12 by 12 in. The pattern of each maze is shown in Fig. 1.

The *Ss* were 90 students of Washington University, none of whom had had previous maze experience. They were divided in random order into 3 groups of 30 each. Men and women were approximately equally represented in each group.

One group of *Ss* learned the original problem, after a time interval of 48 days relearned the same problem, and immediately mastered the second problem. The second group learned the original problem, and immediately learned the second maze. These two experimental groups reached the same level of performance, *i.e.* 3 suc-

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* Accepted for publication August 1, 1932.

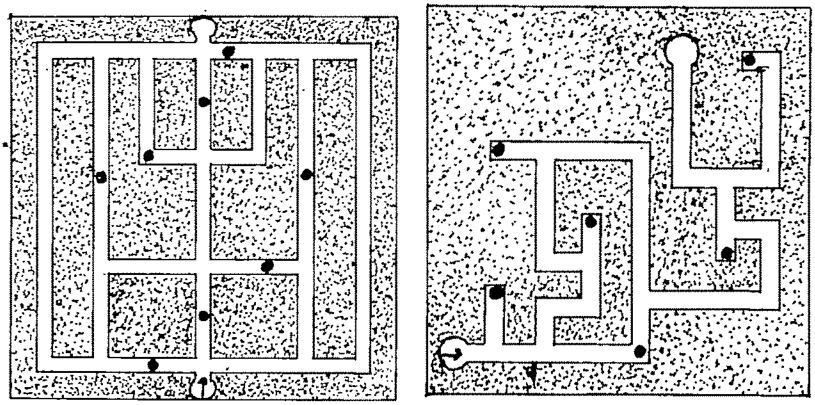
[1] H. A. Carr, *Psychology*, 1925, 99, 237 ff.

[2] S. H. Britt and M. E. Bunch, Jost's Law and retroactive inhibition, this JOURNAL, 46, 1934, (April).

[3] H. A. Carr, The influence of visual guidance in maze learning, *J. Exper. Psychol.*, 4, 1921, 399-417.

cessive trials without error, in the first problem immediately before learning the second problem; but they differed in the fact that Group 1 had learned the first maze 48 days before Group 2. Group 3, as a control, learned only the second maze. This control served as a standard with which to compare the learning scores on the second problem of the two transfer groups.

Three measures of learning efficiency were employed for each maze problem: the total number of trials, the total length of time in seconds (both of these records including the last three perfect trials), and the total number of errors. The particular kinds of errors were also recorded, designated as A error, B error, and C error. An "A" error consisted of retracing the stylus anywhere in the true pathway; a "B" error consisted of entering a blind alley or retracing in a blind alley after having started out; and a "C" error consisted of touching the end, a stop, in a blind alley.


Maze I

Maze II

FIG. 1. SHOWING PATTERNS OF MAZES

Maze I, original problem, made of brass: Maze II, secondary problem, made of fiber-board.

Results. The experimental results are presented under three separate headings: (1) the amount of transfer in the older age-group, Group 1; (2) the amount of transfer in the younger age-group, Group 2; and (3) the relative amounts of transfer in the older and younger age-groups, Groups 1 and 2.

(1) The amount of transfer in the older age-group, Group 1. The first results to be considered are those where Maze II was introduced after the original problem, Maze I, had been brought up to the level of complete mastery 48 days after original learning. This means that the older age-group, Group 1, is to be compared with the control group, Group 3.

According to every type of measurement, Group 1 was far superior to Group 3, and this shows a great amount of positive transfer for Group 1 to the second maze problem. Group 1 averaged 632.83 sec. to learn Maze II, whereas Group 3 averaged 1,240.47 sec., which means that Group 1 required less time, on the average, than Group 3 by 607.64 sec. Similarly, Group 1 required, on the average, only 21.63 trials, while Group 3 took 37.03 trials, a difference in favor of Group 1 of

15.40 trials. As to total errors, the average score for Group 1 was 203.63, but for Group 3 was 533.90, an average-difference of 330.27 total errors. There was the same relationship between the two groups for the three types of errors: Group 1 was superior to Group 3, in averages, by 104.64 A errors, by 128.33 B errors, and by 97.30 C errors. With every type of measurement, the ratio of each difference to the sigma of that difference indicated that the chances were 99.9 in 100 that the difference between the two groups was a true one.

(2) The amount of transfer in the younger age-group, Group 2. The second set of results are those obtained under the conditions in which Maze II was given immediately after the complete mastery of the original problem, Maze I. Hence, the younger age-group, Group 2, is to be compared with the control group, Group 3.

In general, Group 2 was slightly superior to Group 3, indicating some positive transfer for Group 2 to the second problem. The differences between the two groups as to time scores were slight: an average of 1,159.10 sec. for Group 2, and of 1,240.47 sec. for Group 3, which means that Group 2 required less time, on the average, than Group 3 by 81.37 sec. On the basis of trials, Group 2 was inferior: 39.20 trials, as compared with 37.03 for Group 3, a difference in favor of the control group of 2.17 trials. However, with regard to errors, Group 2 was better than Group 3 by every type of measurement. The average total-error score for Group 2 was 478.46, and 533.90 for Group 3, an average-difference of 55.44 total errors. Group 2 also made a better average score than Group 3 by 10.14 A errors, by 29.83 B errors, and by 16.53 C errors. The following figures show the chances in 100 that, if the experiment were repeated, the direction of the difference would be confirmed: over 65 for time; over 62 for trials; over 73 for total errors; 62 for A errors; over 82 for B errors; over 74 for C errors.

(3) The relative amounts of transfer in the older and younger age-groups, Groups 1 and 2. It has been shown that there was transfer to Maze II in the case of both the older age-group, Group 1, and the younger age-group, Group 2. The relative amounts of transfer in the older and younger age-groups are obtained by a comparison of the differences between Groups 1 and 3 (showing transfer for the older age-group) with those between Groups 2 and 3 (showing transfer for the younger age-group). These data have been presented in the preceding sections. The results of the comparison are given graphically in Fig. 2. The differences between each of the transfer groups and the control group are presented as a percentage decrease or increase with respect to the learning scores of the control group. For each pair of blocks in Fig. 2, the first in each case (lettered O) represents the percentage of transfer in the older age-group, *i.e.* the percentage of transfer of Group 1 as compared with the control, Group 3; while the second block in each case (lettered Y) represents the percentage of transfer in the younger age-group, *i.e.* the percentage of transfer of Group 2 as compared with the control, Group 3.

According to every type of measurement employed, there was a far greater amount of positive transfer (superiority to the control group) for the older age-group than for the younger. The percentage of transfer for Group 1 was 49.0% in time scores, but the percentage of transfer for Group 2 in time scores was only 6.6%. On the basis of trials, the percentage of transfer for Group 1 was 41.6%; on the other hand, the control group was somewhat superior to Group 2 in trials, by 5.9%. The other percentages of transfer were as follows: in total errors, 61.9% for Group 1, 10.4% for Group 2—in A errors, 55.1% for Group 1, 5.3% for Group 2—in B errors, 64.9% for Group 1, 15.1% for Group 2—in C errors, 66.6% for Group 1, 11.3% for Group 2.

A second method of determining the relative amounts of transfer in the two age-groups is to compare the raw learning scores of Groups 1 and 2. On this basis, Group 1 was superior to Group 2 in learning Maze II by the following percentages: 83.2% in time; 81.2% in trials; 135.0% in total errors; and 110.6%, 141.7%, and 168.0% in A, B, and C errors respectively. In each case the chances were 99.9 in 100 that there was a true difference between the two groups.

Conclusions. This experiment concerns the relationship between the age of the associations for a learning problem and the amount of positive transfer to a second learning problem. The results show that if the associations for a learning problem are of the same strength but are of unequal age, the older association is more subject to positive transfer to a second problem than is the younger association.

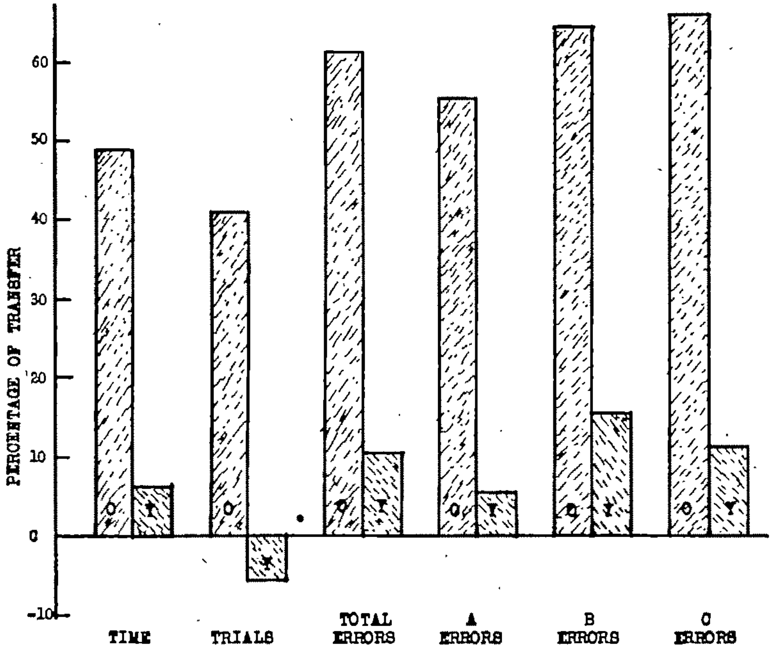


FIG. 2. PERCENTAGE OF TRANSFER FOR GROUP 1 AS COMPARED WITH GROUP 2
The base line marked C represents the learning scores of the control group, Group 3. These scores are used as a standard with which to compare the percentage of transfer of the experimental groups, Groups 1 and 2. Blocks lettered "O" (older) indicate the percentage of transfer of Group 1 as compared with Group 3. Blocks lettered "Y" (younger) indicate the percentage of transfer of Group 2 as compared with Group 3.

From a theoretical standpoint, this conclusion does not imply that the amount of positive transfer is caused by the age of the association. It is simply a statement of the relationship between age differences and transfer. The greater amount of positive transfer for the older association may be due to a large number of factors which necessarily vary with the age of the association, such factors as distribution of effort, additional practice, greater familiarity, and confidence.

EQUIVALENCE OF BRIGHTNESSES ACROSS MODALITIES[*]

By NATHAN E. COHEN, Harvard University

Von Hornbostel,[1] in a recent article on olfactory brightness, raised the question as to whether the brightnesses of the various modalities are identical or merely analogous. In support of the alternative of identity he described an experiment involving, first, the determination by direct comparison of a tone and a gray, each equal in brightness to a given odor, and then, the determination of a gray equal in brightness to the tone. He found the grays determined by these two procedures to be of practically the same stimulus value.[2] These results clearly suggest that, at least in the case of tones, odors, and grays, brightness is a common attribute in the following sense. Supposing *b* to represent a point on the visual scale of brightness and *b′* the point on the auditory or olfactory scale, a point which is called identical in comparison, then the replacement of *b* by *b′* should cause no change in the continuity of the visual brightness scale and vice versa.

The importance of Von Hornbostel's results systematically made it desirable to verify them to make sure that they were not due to the operation of some experimental artifact. The present investigation was carried out with stimuli similar to those used by Von Hornbostel but the results obtained were quite different. It will help clarity to distinguish Parts I, II, and III.

Part I. Part I of the investigation consisted in obtaining a series of comparative judgments between an odor and a range of tones. The constant olfactory stimulus was 5 cc. of benzol, and a Stern variator with a range of 150-300 cycles was used as the variable. *O* was seated 10 ft. from the variator with his back to it. The bottle containing the odor was placed on a stand in front of him. The following instructions were given him: "At the signal 'ready' raise the stopper from the bottle and sniff the odor. After a brief interval a tone will be sounded. Immediately judge to yourself as to whether the tone is greater, equal, or less in brightness than the odor. State your judgment to the *E*." *E* started at the upper end of

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[*] Accepted for publication June 17, 1932.

[1] E. M. von Hornbostel, Über Geruchshelligkeit, *Arch. f. d. ges. Physiol.*, 227, 1931, 517-538.

[2] "Vergleicht man etwa einen bestimmten Ton mittlerer Lage, z.B. 400 Hertz, mit Schwarz-Weiss-Mischungen auf dem Farbenkreisel, so erscheint ein schwärzliches Grau viel zu dunkel, ein weissliches Grau viel zu hell, und indem man sich von beiden Seiten her annähert, gelangt man schliesslich zu einem mittleren Grau, das zu dem Ton am besten zu passen scheint. Ebenso kann zu einem Geruch das gleichhelle Grau oder der gleichhelle Ton eingestellt werden. Sucht man dann zu dem so gefundenen Ton das zu ihm passende Grau, so erhält man sehr annähernd denselben Kreiselwert wie direkt vom Geruch aus, z.B.: Benzol = 40° Weiss; Benzol = 220 Hertz; 220 Hertz = 41° Weiss. Durch solche Äquivalenzbestimmungen, im Dreieck erscheint die Annahme begrundet, dass Helligkeit nicht nur eine analoge, sondern eine identische Seite der Phänomene der verschiedenen Sinnesgebiete ist. Dann wird aber auch in den zugrunde liegenden Nervenvorgängen ein von der Sinnesmodalität unabhangiger Parameter für die Helligkeit vorauszusetzen sein." *Op. cit.*, 519.

the tonal range and worked down to find, first, a higher, and then lower, level of approximate equivalence to the brightness of the constant odor. This range of tones thus obtained was divided into equal steps and the tones were then presented in a haphazard order in the method of constant stimuli. Urban's tables were used throughout the experiment in determining the point of subjective equality (P.S.E.).

The results obtained from the two *Os*, *WA* and *SP*, used throughout the experiment, were as follows:

WA in a total series of 125 judgments gave a P.S.E. of 159 cycles.
SP in a total series of 200 judgments gave a P.S.E. of 267 cycles.

Part II. Part II, carried on a week later, consisted in obtaining a series of comparative judgments between an odor and a range of grays. The constant was again 5 cc. of benzol, and the grays produced by a color-wheel were used as the variable. *O* was seated 5 ft. from the color-wheel and a screen was used to prevent him from noting the changes made. Lighting was artificial and constant. The bottle containing the odor was placed on a stand alongside *O*. The following instructions were given: "At the signal 'ready' raise the stopper of the bottle and sniff the odor. After a brief interval a disk will be presented on the color-wheel. Immediately judge to yourself as to whether the disk is greater, equal, or less in brightness than the odor. State your judgment to *E*." *SP* was not started at the top of the range, but at 90° white and worked down from this point. *WA* was started at 240° white and worked down until the higher and lower levels were obtained. Upon the obtaining of the range the treatment was the same as in Part I.

The results obtained from the two *Os* were as follows:

WA in a total series of 125 judgments gave a P.S.E. of 83° white.
SP in a total series of 150 judgments gave a P.S.E. of 85° white.

Part III. Part III, carried on a week later, consisted in obtaining a series of comparative judgments between a gray and a range of tones. The P.S.E. obtained for each *O* in Part II was used as the constant (*WA*, 83° white; *SP*, 85° white) and for preliminary exploration tones of a Stern variator with a range of 500-1000 cycles were used as the variable. The conditions were similar to those in Parts I and II. The following instructions were given: "At the signal 'ready' a disk will be presented on the color-wheel. After a brief interval a tone will be sounded. Immediately judge to yourself as to whether the tone is greater, equal, or less in brightness than the disk. State your judgment to *E*." *SP* started at 900 cycles and worked down until a higher and lower level of approximate equivalence were obtained. *WA* started at 920 cycles and worked down, but a lower level could not be found on the range of the variator used. Another variator with a range of 200-400 cycles was substituted and the same procedure followed.

The results obtained were as follows:

WA in a total series of 125 judgments gave a P.S.E. of 300 cycles.
SP in a total series of 150 judgments gave a P.S.E. of 804 cycles.

The results of the three determinations of equivalents may be summarized as follows:

PITCH	(1)	ODOR	(2)	GRAY	(3)	PITCH
159 cycles	←	benzol	→	83° white	→	300 cycles
267 cycles	←	benzol	→	85° white	→	804 cycles

The arrows point from the standard of brightness toward the variable that was subjectively equated to the standard. The numbers in parentheses show the order of the experiments. Benzol was first equated to a pitch and then to a gray, which was in turn equated to a pitch. Since the arrows indicate equivalences, the frequencies at the two ends of each line should be the same. There is, however, about an octave of difference in the first case, and an octave-plus-a-fifth of difference in the second case.

These results indicate too great a discrepancy to render the hypothesis of identity plausible. The question therefore remains as to how the *Os* make their comparative judgments. A clue is provided by the recent work upon 'absolute' judgments[3] when considered with the indubitable fact of transposition.[4] Previous stimulation may have been sufficient for the establishment of the attitude necessary for 'absolute' judgments for each of the three continua of brightness. It would not be unreasonable then to suppose that cross-modality comparison should be based (physiologically, if not introspectively) upon relative positions within different 'absolute' scales. According to this view equation with respect to brightness of two experiences of different modalities would involve nothing more than the identity of relative positions upon two wholly independent scales.

This hypothesis is adequate not only to explain the general results of the experiment, but also the difference in results in the case of the two *Os*. Clearly the absolute series will depend in varying degree upon past experience and present range of stimuli.[5] Thus we may expect two different types of judgment, as past experience preponderates over present range of stimulation or the converse. If we assume that for *WA* the importance of past experience is greater than for *SP*, we can understand that for the former the results will be fairly constant, whereas, for the latter a shift to most any position along the scale may be obtained.

This hypothesis is further confirmed by the following observations: Three *Os* were given a tone of 220 cycles and asked whether it was high, low, or medium in brightness. Two of the *Os* reported it as medium, the third as lying between low and medium. The same type of observation was then made on the scale of grays. Eight *Os* were asked to judge whether 85° white is 'high,' 'low,' or 'medium' in brightness. Six reported it as medium, one as high, and the other as low. Thus we see that, if a medium tone or gray is given alone, it generally tends to be reported as medium, that is to say, the absolute judgment tends to be determined by past experience. Another set of observations consisted of presenting to a group of *Os* the upper 18 papers of a series of 50 grays. The *Os* were asked to select the gray that was equal in brightness to a given tone of 220 cycles. The papers selected ranged between the second and the fifteenth. Three weeks later the same *Os* were asked to perform a similar task: the tone was again 220 cycles but the grays were the lower 18 of the 50 papers. Nevertheless, all the *Os* made a selection, and the choices ranged between the thirty-eighth and forty-eighth papers. Thus when a gray is presented as part of a series, its 'absolute' brightness for all *Os* appears to be mainly a function of the series.

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[3] E. G. Wever and K. E. Zener, The method of absolute judgment in psychophysics, *Psychol. Rev.*, 35, 1928, 466-493.

[4] W. Köhler, *Gestalt Psychology*, 1929, 214.

[5] Wever and Zener, *op. cit.*

MINOR STUDIES FROM THE WELLESLEY COLLEGE PSYCHOLOGICAL LABORATORY

V. The Recognition of Relatively Simple Sensory Experiences

By Edith Brandt Mallory

The problem for which this series of experiments was originally undertaken was to compare the relative recognition-value of concrete materials perceived through different senses. Since complexity was known to be a factor which assists recognition-memory, the material was chosen so that the complexity, both of the experiences themselves and of their associations, would be as nearly as possible equalized, by elimination. Results of tests with these simple units proved so different from what might logically be expected from everyday experience that it seemed desirable to expand the scope of the experiment to discover what would happen with the use of slightly more complex units, and also to investigate the relative *associability* of items in several different sense fields.

Historical. In 1909 Gamble reported a series of Wellesley College memory studies in which smells and colors, as well as nonsense-syllables, were used.[1] The standard test procedure employed was that of the reconstruction method, which was applicable to all three kinds of materials. Her conclusions concern chiefly the differences between the concrete and verbal types, the former being found, on the whole, the easier to learn in short series, and the latter being the easier to learn in long series. A comparison of smells and colors, as materials representative of different senses, does, however, appear in that "neither the numerical results nor the introspective observations reveal any essential difference in memorizing the three kinds of series." This conclusion, it will be noted, concerns the qualitative nature of the processes, and not the relative *ease* of remembering material from different senses. The latter comparison was, of course, not possible in this case, since the various types of material employed were admittedly unequal in respect of the important factors of complexity and interest.

In 1915 Woods made use of concrete material of various sorts, including olfactory, tactual, auditory and visual, as well as verbal items.[2] In this study, however, the aim was not the differentiation of the process of recognition as it occurs in the several types of sensation, but rather an inclusive definition of the experience of familiarity as it is found in all of these various situations.

The direct comparison of memory for the different senses seems to have been

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* Accepted for publication September 19, 1932. The experimental work of this study was conducted by Mary Bressler, Mary Butler, Regene Pollock, Rosamond Doering, and Alice Walker.

[1] E. A. McC. Gamble, A study in memorizing various materials by the reconstruction method, *Psychol. Monog.*, 10, 1909, (no. 43), 1-211.

[2] E. L. Woods, An experimental analysis of the process of recognizing, this JOURNAL, 26, 1915, 313-387.

generally ignored, presumably on the ground that really comparable material could not be devised for the different cases; and it must be confessed that the selection of suitable material *is* the paramount difficulty of the problem. The writer's attempts to meet this need are given in detail in a later section.

Gordon studied the influence of complexity on memory by comparing (1) scores for syllables with plain backgrounds with scores for syllables with colored ones, and (2) scores for series of syllables shown all successively in the same place with scores for series in which each item appeared in a different spot.[3] She found that the more complicated stimuli were as a rule the better remembered.

In Peterson's experiments other kinds of variation were used.[4] When memory for words and sentences was compared, the more complex meaning was found to have an advantage in recognition. Series of nonsense syllables varying (1) in size, (2) in color, and (3) in style of type showed marked gains over the plain control series. The influence of size, color, form, and their combinations on memory for plane figures was also noted. For the most part series with combinations of factors were remembered better than simple ones, but since there were only 3 *O*s and individual differences were marked, really clear-cut conclusions were hardly possible.

A general indication of the importance of complexity in recognition-memory appears incidentally in a study by Achilles.[5] Of the three types of materials used—proverbs, words, and nonsense syllables—the most complex kind (proverbs) is found easiest to recognize, and the least complex kind (nonsense-syllables) is found to be the most difficult.

General plan of the experiments. For the two fundamental problems investigated in these studies—(1) the relative recognition-value of simple stimuli in different senses, and (2) the recognition and association values of various combinations of these stimuli—the first step was to devise material which would be uniformly simple, and comparable in all respects except the actual sense avenue through which the *O* was to perceive it. Not only did logical associations need to be equalized or excluded, but other verbal and even merely 'vocalizable' associations as well, since one cannot compare items as representative of their individual sense fields alone if, as in the case of nonsense-syllables, they can simultaneously be imaged in visual, auditory and vocimotor forms, or if they can be adequately differentiated by verbal descriptions which may be imaged in these ways. Such associations were regarded as undesirable because they made it difficult to control the factor of complexity, as well as on account of the fact that they involved sense fields other than the one supposedly represented.

The solution chosen was the use of items which differed from others in a particular series, not by the possession of different qualities, but simply by exhibiting quantitatively different degrees of some one quality. The qualities selected were all of the kind that could be varied along a linear scale: thus items of the visual series differed among themselves only in light-intensity; those of the auditory series, in pitch; those of the tactual series, in roughness; in the thermal series, in grades of warmth and cold; and those in the tests of kinesthetic memory, in extent of passive hand-movement. With such series the items were too similar to make ordinary associations with concrete memory images useful. If an *O* tried to remember one stimulus as 'like a certain gray dress,' for instance, the other stimuli in the series were also so much like that dress that the auxiliary association did not really distinguish the one item from the other. Neither was it possible to differentiate adequately with verbal descriptions. The terms 'more,' 'less,' 'very much,' 'slightly,' etc., could of course be applied, but these were not effectively definitive, and, whatever their value, would be *equally* possible for *all* of the qualities in *all* of the senses tested.

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[3] Kate Gordon, Meaning in memory and attention, *Psychol. Rev.*, 10, 1903, 267-283.

[4] H. A. Peterson, On the influences of complexity and dissimilarity on memory, *Psychol. Monog.*, 12, 1910, (no. 49), 1-86.

[5] E. M. Achilles, Experimental studies in recall and recognition, *Arch. Psychol.*, 6, 1920, (no. 44), 1-80.

While, as has been noted above, the individual items could not be distinguished easily by *description,* great care was taken to make sure that the difference between any two items of a series was unquestionably greater than the actual 'difference limen,' *i.e.* the difference was always perceptible when the stimuli were presented either simultaneously or in immediate temporal succession. With the exception of a special case which has been noted, errors may therefore be attributed to the ineffectiveness of memory, and not to inability to make the necessary sensory discrimination.

A number of test methods were seriously considered before the selected ones were adopted. Obviously the usual types of recall tests could not be used uniformly for all the senses studied. Reconstruction would of course be applicable in every case but was ruled out because this procedure does not necessitate memory for the actual sense experience, but merely the retention of the *order* of a series; and this, although perceived through a single particular sense, is apt to be remembered by means of spatial or other patterns, composed of images from other sense fields. These considerations led to the adoption of recognition methods of memory testing. In two of the experiments a simple form was used, a form in which items seen in a previous series are to be recognized and identified when they are again presented together with a number of other items. In one experiment a somewhat more elaborate form was employed in order to test association as well as bare familiarity. Detailed descriptions of both methods are given in later sections of the paper.

The investigation of individual differences, in the abilities tested, would make an interesting sequel to these experiments, but does not come within the scope of this study.

PART I. A COMPARISON OF RECOGNITION-MEMORY IN DIFFERENT SENSE FIELDS

(Conducted by Mary Bressler)

The purpose of Part I was to compare recognition-memory for materials perceived through four different avenues of sense: vision, audition, touch and the thermal senses.

General procedure. In each of the four parts of this experiment a series of 5 items was first presented, after which came a second series composed of the 5 original items mixed in chance order with 5 others not previously given. As each item in the second series was presented, *O* reported whether she believed it had or had not occurred in the earlier series. Each stimulus lasted for 8 sec., and 20-sec. intervals elapsed between them. In every case *O*'s report was recorded when given, but full introspections were not rendered until the conclusion of the test.

Materials. In the tests of visual memory the material included ten 3 x 3 in. cardboard squares covered with gray papers of 10 different shades, a watch with second hand for timing, and an exposure apparatus. The latter consisted of a screen with a small square window, behind which there was a carrier for the cards. A spring attachment on this carrier kept the card out of range of the window between presentations, but light manual pressure brought the card readily and quickly into view.

A pitch pipe was used to produce the auditory stimuli. The first series was composed of the notes a, a♯, c, c♯, and e; and these were repeated in the test series combined in chance order with b, d, f♯, g and g♯.

For tactual stimuli, 10 samples of cloth varying in roughness from the smoothness of satin to the coarse texture of burlap were chosen. These were tacked to pieces of wood in a way to insure even weight and hardness to the surfaces.

Supplies of ice and boiling water, as well as of plain tap-water, were provided in the preparation of thermal stimuli. By the combination of these, water was mixed in cups to the temperatures: 10°, 15°, 20°, 30°, and 40°C., for one series, and to 5°, 25°, 35°, 45°, and 50°C., in addition to the above for the second series. A standard cup of water at 26°C. was also maintained (see below). These temperatures were checked and tested with a thermometer at the beginning of each stimulation.

Directions to observers. The following directions, in type-written form, were handed to the *O*s before each part of the experiment.

> "You will have presented to you a series of stimuli. Each stimulus will last 8 sec., and there will be 20 sec. between presentations.
>
> "Immediately following the first series you will have the same stimuli presented to you, mixed with new 'confusion' stimuli, and you are to recognize as many as possible of those that were in the first series. After each stimulus in this second series, say 'yes' or 'no' to show that you do or do not recognize it as one of the earlier series. You may add 'doubtful' if you are not sure of your decision."

The *O*s were instructed which type of stimuli, whether visual, auditory, tactual, or thermal, was to be given, in each case.

Methods adopted to avoid error. Care was taken that the *O*s should receive no cues from seeing the *E* prepare or adjust the material. During the tests of visual and of auditory memory, all necessary manipulation of gray cards and of the pitch pipe was concealed from the *O*s behind a cardboard screen. In those parts of the experiment involving tactual and thermal stimuli the *O* was blind-folded.

In order that the *O*s should be passive in their perception of tactual and thermal stimuli as well as in vision and audition, special technique was employed in the two former cases. The *O* did not actively rub the cloth, but simply laid her hand on the table, palm up, with middle and index fingers extended toward *E*, while the latter drew the cloth-covered blocks of wood steadily and evenly across the tips of *O*'s outstretched fingers. In the case of thermal stimulation, *O*'s fingers were guided to the cup and dipped into the water (always to the same depth) by *E*.

Under the given conditions of the experiment *sensory adaptation* offered no particular difficulty in the case of vision and audition, but in the other two tests some care had to be exercised in this connection. The *O*s were asked to report any numbness or confusing sensations which may have occurred in their work with the different grades of roughness and were told to rub the fingertips on the palm of the hand in the intervals. (Very little difficulty or confusion from this source was reported.) Adaptation to temperature proved to be a much more serious problem. Before every regular item in the thermal series *O*'s fingers were placed in a cup of water of standard (26°C.) temperature, and then dried. In this manner it was assured that the skin would be always at the same temperature when the stimulation was given, so that the stimuli when repeated in the second series would feel exactly as they did in the first, because of the same relative sensory difference as well as because of the same absolute degree of heat.

Observers. Of the 20 *O*s who took part in this experiment 17 were students, and three were members of the college staff in psychology.

Scoring. In any test where the answer can be only one of two choices, it is possible for a half of the perfect score (in this case 5) to be made by sheer chance. The score for each test was, therefore, the number of items in the test series reported correctly, minus 5. The score for each type of sensory material was determined by averaging the scores of all 20 *O*s for each class of test.

Results. The average recognition score for grays was 0.60, P.E. = 1.27; for pitch, 0.75, P.E. = 1.1; for roughness, 1.45, P.E. = 1.1; and for temperature, 1.75, P.E. = 0.84.

The striking fact here is that although all the items were easily susceptible of immediate sensory discrimination, and there were only 5 items to be remembered, the scores are all so low as to be possible by the operation of mere chance.

The slight differences which occur show higher scores for tactual and thermal memory than for visual and auditory memory, an outcome which appears at first glance extraordinary, since it is a commonplace of experience that we use and rely on memory in the two latter fields rather than in the two former ones. Reflection suggests that it may be the wealth of associations which are usually combined in our auditory and visual experiences which make them superior in everyday life, but this does not in any way account for their making a *lower* score than tactual and thermal stimulation in the laboratory situation. Suspecting that the answer might lie in some unanticipated peculiarity either in materials or in procedure, *E* reëxamined these with care. A study of the *O*s' introspections brought to light the fact that the *texture of the fabrics* had afforded composite experiences of softness, irregularity, clingingness, etc., as well as roughness, and occasionally even the identifying name of the fabric. Obviously then, the tactual stimuli had not been 'simple,' as intended, but really possessed very complex qualities; and thus it seemed more probable that the superiority in memory-value was due to complexity rather than to the sense modality. When the various *procedures* were compared it was found that the experiment with temperature differed in one respect from all the rest. In order to rule out complications arising from sensory adaptation, a standard stimulus of 26°C. was given before each regular stimulus. In so doing, however, *E* had inadvertently presented a much more complex situation in this test than in the others. Two related items, instead of one, had really been presented in each case. Evidently the thermal stimuli, as well as the tactual ones, were too complex to be judged on even terms with those of vision or audition, since in these instances the factor of complexity (to be studied in the following experiment) was in operation.

Conclusions. (1) Recognition memory for material of the very simple type used in this experiment is exceedingly difficult. *(2)* The apparent advantage of tactual and thermal material is probably due to the fact that these were more complex than the other two types.

PART II. THE RECOGNITION VALUE OF COMPLEXITY IN MATERIAL AS PRODUCED BY THE COMBINATION OF SEPARATE STIMULI

(Conducted by Mary Butler and Regene Pollock)

Part II was devised to discover the effect of complexity of material on recognition-memory.

General procedure. Material from three senses was used—vision, audition, and touch. The materials were gray papers, tones of different pitches, and grades of roughness as found in a series of sandpapers. Tests were given for various combinations of these materials, *i.e.* for series of (1) single stimuli in each sense field, (2) pairs of stimuli in each sense field, (3) pairs made up of combinations of material from two sense fields, and (4) trios combined from all three senses. The simple recognition test, described in the preceding experiment, was used. Stimuli lasted for 2-sec., and 3-sec. intervals elapsed between them.

Materials. For the tests of visual material, cardboard squares of varying shades of gray were used. These were made by dyeing white cardboard with graded solutions of black ink. They were chosen as approximately equidistant one from another along the range from very light to very dark gray and were discriminable at a distance of 30 ft. A purely subjective criterion (agreement of 3 *O*s) was used in selecting these shades, but subsequent testing by means of a Nicol prism polariscope showed that each gray had between 80% and 85% of the intensity of the next brighter stimulus. For audition, a pitch pipe was used as in the preceding experiperiment. Ten pieces of sandpaper of discriminable grades of roughness were used as touch stimuli. These were glued to blocks of wood to insure equal weight and hardness of surface.

Directions to observers. Typewritten copies of the instructions were handed to *O*, and at the same time read aloud by *E*. Sample directions of each general type are given below.

(a) "I am going to present to you a series of 5 grays differing in degrees of brightness. I want you to try your best to remember these, so that you will be able to recognize each one when you see it again. After I have shown you the first series, I will present a second series composed of 10 grays, which will include the 5 that you have just seen, as well as others. Try your best to identify those first 5 grays."

(b) "I am going to give you a series composed of 5 somewhat complex situations, each affecting two senses; namely, touch and hearing. Each will be composed of a certain degree of roughness and a certain degree of pitch. After I have shown you the first series," etc.

(c) "Now I am going to give you another series to remember. This time both stimuli of each pair will be touch sensations, of particular degrees of roughness," etc.

(d) "Now I am going to give you a series of experiences in which three senses are stimulated," etc.

As the second test series was shown *O* responded to each item or combination of items with 'Yes' or 'No,' according as she did or did not recognize it as one of the first series.

Methods adopted to avoid error. Care was taken to be sure that all the selected grades or degrees of each quality used were discriminable one from the other when experienced simultaneously or in immediate succession.

The visual exposure apparatus was covered with green so that no relationships could be readily formed between it and the grays.

In order to prevent any visual aid in the tactual part of the experiment, *O* was made to extend her hand through an opening in a cardboard screen, and the application of the sandpaper was thus entirely concealed from her sight.

The pitch pipe was also used behind a screen, out of sight of *O*.

To avoid giving any particular material the advantage of practice at the end of the experimental period, or of initial zeal at its beginning, one *O* started with test No. 1, the second *O* with test No. 2, and so on. As they continued through the entire series, such rotation equated these factors for all the kinds of material.

Observers. The *O*s of this experiment were 37 members of Wellesley College, most of them students. The first seven sets of tests were regarded as a preliminary practice experiment, in the course of which the *E*s improved and mastered their technique. The results given below are derived from the scores of the last 30 *O*s.

Scoring. In this study the tests were scored by the 'right-minus-wrong' formula to eliminate the chance of scoring by mere guessing; that is to say, the number of wrong reports was subtracted from the number of right reports in each test. The scores given as representative of each type of material are averages of the tests of 30 *O*s.

TABLE I

SHOWING SCORES FOR DIFFERENT COMBINATIONS OF MATERIALS

<table>
<tr><th rowspan="2">Average Scores for</th><th rowspan="2">Vision</th><th rowspan="2">Audition</th><th rowspan="2">Touch</th><th colspan="2">Scores for different degrees of complexity</th></tr>
<tr><th>Average</th><th>P.E.</th></tr>
<tr><td>Simple items</td><td>2.86</td><td>1.63</td><td>2.73</td><td>2.24</td><td>.84</td></tr>
<tr><td>Pairs in single sense</td><td>4.53</td><td>3.40</td><td>2.60</td><td>3.49</td><td>.95</td></tr>
<tr><td>Pairs in unlike senses</td><td colspan="2">3.66</td><td></td><td></td><td></td></tr>
<tr><td></td><td></td><td colspan="2">3.86</td><td></td><td></td></tr>
<tr><td></td><td colspan="3">4.86</td><td>4.29</td><td>1.08</td></tr>
<tr><td>All tests in which a particular kind of material occurred</td><td>4.3</td><td>3.66</td><td>4.03</td><td></td><td></td></tr>
<tr><td>Trios of all three senses combined:</td><td></td><td></td><td>5.68</td><td>5.68</td><td>.79</td></tr>
</table>

Results. Table I shows the average scores for tests involving single items in each sense field, pairs of stimuli from the same sense, pairs from dissimilar senses, and trios.

Scores for the single items (2.86 for vision, 1.63 for audition, and 2.73 for touch) show again that touch in its simple form is not harder to remember than items from the other two senses. Audition appears exceedingly low. This result, which seems strange in view of our extensive use of auditory memory for the complex sorts of stimuli usual in everyday life, is corroborated by the acknowledged rareness of the occurrence of 'absolute pitch' where memory for single tones is required.

Scores for pairs show a different tendency. The visual pairs gave an average score of 4.53, auditory of 3.4, and touch of 2.6. Whereas in the first two senses pairs were almost twice as easy to remember as the single items, touch showed no advantage in scores for the pair as compared with the single unit. Either two touches are not easily associable one with another, or sensory adaptation interferes with discrimination when several sensations of roughness occur in succession. The following scores for dissimilar pairs, however, seem to show that touch is of value when associated with material from another sense. These data will support the view that sensations of roughness are associated as readily as any other sensation if adequately perceived, but, whereas they are adequately perceived one at a time, sensory adaptation confuses the experience if stimuli occur in too close succession. Vision and audition combined received a score of 3.66; audition and touch, 3.86; vision and touch, 4.86; and the trio (vision, touch, and audition), 5.68.

When averages were taken for all of the tests in which vision was involved, vision had a score of 4.30, audition a score of 3.66, and touch a score of 4.03. It would appear that touch is of as much assistance as any other sense as a supplementary factor, although two touch stimuli do not make a combination that is easily remembered. The striking influence of complexity is shown in the following comparisons. When scores for all the single items are averaged, their score is 2.24, P.E. = 0.84; the average for all pairs is 3.89, P.E. = 1.02; and the average for the trios is 5.68, P.E. = 1.26.

Conclusions. (1) In general, the results of this experiment confirm those of the first one in that variations in memory-value of different sense materials are slight. Audition, however, is poorer than vision or touch when represented by simple stimuli and roughness is lower than the others when represented by complex stimuli. *(2)* Touch stands out from the other two senses studied in that, whereas it assists memory when combined with other senses, two touch stimuli appear to be no easier to remember than one touch stimulus. Sensory adaptation may be responsible for this apparent discrepancy. *(3)* Materials from different sense fields can be combined, and such combinations are as easy or easier to remember than combinations of stimuli from the same sense field. *(4)* Real differences show themselves between the memory-value scores for simple and complex material; the more complex the stimulus the higher the score.

PART III. THE ASSOCIABILITY OF STIMULI FROM DIFFERENT SENSE FIELDS

(Conducted by Rosamond Doering and Alice Walker)

The problem investigated in Part III was the associability of stimuli perceived through three different sense avenues; namely, touch, vision, and kinesthesis. Since in the preceding experiment simple elements showed approximately the same memory values, regardless of the senses represented, and since the more complex combinations were remembered better than the simpler ones, it was surmised that the advantage usually accredited to visual (and auditory) memory in everyday experience might be due to such complexity. There arose therefore the question whether percepts received through certain senses were more complex merely because these sensory systems admitted a greater wealth of data, or because experiences received through such senses were especially easy to associate and therefore to organize.

General procedure. The associability of different materials was tested by the following method. There was first presented a series of 8 items, grouped in 4 pairs. *O* was then given the first member of some one of these pairs, and asked to select, out of three stimuli offered, the second member of that pair. Memory for the other three associated pairs was similarly tested. The technique might be described as a modification of the method of right associates, in which a multiple-choice recognition test is substituted for the usual recall test. There were 9 tests in each complete cycle, arranged to test the associations of (1) grays with grays, (2) length of passive hand-movement (which, for the sake of brevity will be called "distances") with other distances, (3) grades of tactual roughness with other grades of roughness, (4) gray with roughness, (5) roughness with gray, (6) gray with distance, (7) distance with gray, (8) roughness with distance, and (9) distance with roughness. A 2-sec. exposure was allowed for each stimulus, a 3-sec. interval was allowed between the pairs, and a 5-sec. interval elapsed between the first presentation and the test series.

Materials. The visual items were 2-in. squares of Hering gray papers, mounted on cardboard. They were presented in the exposure apparatus described in Part I.

For tactual stimuli there were used a series of 10 grades of sandpaper varying serially from very rough to very smooth. These were mounted on blocks of wood to equalize their hardness and weight.

In the tests involving passive arm-movements there was used a special apparatus designed by Professor M. J. Zigler. On a heavy wooden base is a well-oiled metal groove with a millimeter scale. The carrier which slides along this groove has two knobs or handles so that with one it can be manipulated by *E* while *O*'s hand, grasping the other handle, is moved the required distance. This apparatus was placed so that all motion was away from and parallel to the median plane of *O*, and in line with her shoulder.

A large table screen with curtained aperture to admit *O*'s arm concealed from her sight all procedure of the touch and 'distance' tests.

An exposure apparatus for grays was placed at the side of the screen, where it could conveniently be observed. A metronome was used for timing.

Directions to observers. When *O* was seated at the table in front of the screen *E* announced:

> "In the course of this experiment you will have stimuli of various sorts, presented in pairs. They will include certain shades of gray, certain degrees of roughness, and certain distances that your hand will be moved. I will give you first a pair made up of two shades of gray [or whatever material was scheduled for the beginning of the test], then other pairs of grays until I have given you four pairs. I want you to associate the two grays in each pair together in such a way that when I later give you the first member of the pairs you will be able to pick out, from a group of three, the other gray which has been presented. Is this clear?"

Questions were answered and preliminary trials were made until it was certain that *O* understood exactly what was required. Similar forms of directions were used for the other materials and for their various combinations. None of the grays were repeated in the course of any one test.

Methods adopted to avoid error. A certain number of tests were given in which the four initial members of the pairs were shown in the same order in which they had originally been given, but it was discovered that under these conditions the *O*s were not really associating the two members of each pair, but rather remembering the pattern of the entire series of eight and trying to choose the stimulus that they recalled as "number 2" or "number 6." A change was therefore made in the technique, and the initial members of the pairs were presented in the test series in an order different from that in which they had at first appeared. Only the scores from the revised form of test are reported; the others were discarded as irrelevant to the problem in hand.

Observers. Six *O*s, five undergraduates and one member of the laboratory staff, took part in this experiment. All were trained, and accustomed to serving in psychological experiments.

Scoring. The score for each test was the number of pairs correctly associated. The highest possible score would thus be 4, and a score of approximately 1.3 would be expected from the operation of sheer chance. Each *O* took from two to eight cycles of tests, so that the final score reported for each of the nine combinations of materials is an average of the scores of 40 tests.

Results. Table II gives the averages found for the various combinations of materials.

For pairs of items from the same sense field, grays had an average of 2.62; roughness, 2.23; and distance, 2.91; the average for all like pairs was 2.65. Roughness pairs are below the others. For unlike pairs the averages were 2.31 for gray and roughness; 2.77 for roughness and gray; 2.87 for gray and distance; 2.77 for distance and gray; 2.99 for roughness and distance; and 2.32 for distance and roughness. An average of 2.68 for all unlike pairs was found.

TABLE II

Showing Scores of Results of Tests of Association Between Different Kinds of Materials

(The materials listed in the first column are those given as first members of pairs; those in the row across the top were given as second members of the pairs.)

	Gray	Roughness	Distance	Av.
Gray	2.6	2.3	2.8	2.6
Roughness	2.7	2.2	2.9	2.6
Distance	2.7	2.3	2.9	2.6
Av.	2.7	2.3	2.9	

The similarity of these figures suggests very strongly that pairs of simple stimuli can be associated with about equal facility, regardless of which of the three senses they involve. Slight differences were found between the senses when averages were taken for all the pairs containing each type. All the pairs containing gray gave an average of 2.67; the average of all those containing roughness gave 2.53; and the average for all combinations containing distance gave 2.78. When averages were scheduled for all the tests in which each type of material was used as the first member of the pairs, *i.e.* as 'cues,' scores for the three types were practically identical: viz. gray, 2.60; roughness, 2.67; and distance, 2.67. When these materials were used as the second members of the pairs, that is, the ones that had to be selected as familiar from groups of three, a little difference was found. The score for gray was 2.72; roughness, 2.29; and distance 2.92. Here, as in Part II, perception was probably at fault in the case of roughness.

Conclusions. (1) Simple experiences perceived through the three senses studied—vision, touch and kinesthesis—appear to be equally easy to associate. *(2)* It is as easy to associate two items from different senses as it is to associate two items from the same senses. *(3)* The apparent slight inferiority of memory for the connection of two items both in the field of touch may be due to difficulties attending their perception rather than their association.

GENERAL DISCUSSION

Since the results of the foregoing studies do not demonstrate those differences in memory-value that are usually ascribed to the various senses (and which are indeed very regularly found in introspective reports of memory-imagery), there arises the question: What is the difference between these experimental conditions and those of everyday life? In the latter practical situation the experiences ordinarily differ from these laboratory stimuli in their more varied range of interest, complexity, and organization. In the opinion of the writer the apparent advantage of memory for some senses is actually due to these factors, to which both the wider range of discriminable qualities and better opportunities for relationship (such as the spatial connections of vision and the temporal aspect of audition) especially contribute. Hollingworth has made interesting use of these points in his discussion of the value and relationships of the senses.[6]

In view of the fact that the differences (in pitch, light, intensity, roughness, or temperature) between the items in any given series were all greater than the *Os*' limen for immediate sensory discrimination, and that only five items were presented in each original series, the recognition scores, in some cases only slightly above what might be expected by pure chance, seem astonishingly low. The obvious interpretation would appear to be that the extremely low scores are due to the extreme simplicity of the stimuli. If this be true, it suggests that complexity may be thought of not merely as an 'aid' in recognizing, but as fundamentally necessary to the process. Completely simple and unrelated items cannot, presumably, ever be given, since temporal and spatial as well as other self-relating associations are bound to enter in,[7] but as the connections are reduced the possible memory value seems to become slighter and slighter. If one may argue from the analogy of the variable approaching a limit, one might surmise that a completely unrelated homogeneous stimulus must be completely impossible to remember.

SUMMARY AND CONCLUSIONS

It was noted that, in general, an individual remembers experiences received through certain senses more frequently and more accurately than he remembers those perceived through certain others. There arose, therefore, the question whether this difference is to be attributed to the fact that it is actually easier to remember material for one sense than it is to remember equivalent material from another, or simply because in the former case there is commonly some special characteristic such as variety, complexity, attention-value, etc., which gives it superior memory-value.

In order to make the material as nearly as possible equivalent for the several senses, the factors listed above were ruled out by selecting in each sense material that varied only in the quantitative degree to which it possessed some single quality—*e.g.* roughness in touch, and reflected light-intensity in vision.

These very simple stimuli and also slightly more complex ones were used in recognition tests of the 'identification' type. Tests to measure the ease of associating the simple sensory items were also given.

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[6] H. L. Hollingworth, *Psychology: Its Facts and Principles*, 1928, 459.

[7] Cf. E. G. Boring's description of consciousness as relational, which has appeared since the acceptance of this article for publication. *The Physical Dimensions of Consciousness*, 1933, 222-229.

Results of these studies have led to the following conclusions:

(*1*) None of the senses studied shows any marked superiority over the others in the recognition-value of its simple elements.

(*2*) Simple items in vision, kinesthesis and touch appear equally easy to associate, although difficulties of sensory discrimination give an apparent disadvantage to the association of one touch stimulus with another.

(*3*) Complexity is of great importance as an aid to recognition value. It appears possible that the differences in memory-value customarily attributed to the several senses may be due largely to variations in this factor.

(*4*) It is possible to devise materials representative of several senses, that are comparable to each other in respect to the factor of complexity, by using series in which the items vary only by the possession of different quantitative degrees of some single quality.

APPARATUS

THE TACHISTOSCOPE AS A MEASURE OF READING PERCEPTION

By Francis P. Robinson, University of Iowa

Both tachistoscopic and reading perception are characterized by a limit to the amount of material that is perceived. Because of this, one has been assumed to be a measure of the other. Thus, Tinker,[1] noting that several investigators have found that perception-span correlates fairly well (0.50–0.70) with the ability to score on reading tests, concludes that "the span of visual apprehension for certain types of material is an important determinant of reading ability." Brooks interprets the tachistoscopic span as being the physiological limit determining the upper levels of reading-span development.[2] The purpose of this study is to evaluate the tachistoscope as a measure of this aspect of reading perception.

The experimental procedure for determining the relationship of these two variables was as follows. The eye-movements of 51 college students were photographed as they read a given selection. Their average width of fixation was found in terms of words by dividing the number of fixations made into the total number of words read. Following accepted scoring procedure, the first line was omitted from these computations. Their perception-span was found by taking the average number of words perceived in 30 tachistoscopic presentations of unrelated sentences. Tinker's method of scoring one point for each word right and one-half point for words in the wrong order was used.[3] The nature of this tachistoscope will be described later.

In order to allow the most reliable determination of these spans, the following controls were set up for this experiment. A very simple story, that of Aladdin, was used so as to eliminate difficulties in vocabulary and comprehension. Since this reduced regressions to a minimum, it facilitated the computation of a valid width of fixation. It also allowed the determination of the maximal perception-span unaffected by long or hard words. Several trials were given before each part of the experiment to adjust the student to the experimental situation. The stimuli for each experiment for any one individual were selected from different parts of the story, but by rotating these parts with each new case, the same stimuli were used by the group in all situations. It had originally been intended to restrict the computation of the average width of fixation to the same lines that were used in the tachistoscopic test, but when this was found to correlate 0.95 with the average reading-span for the whole selection, the latter was used because of its greater reliability.

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[1] M. A. Tinker, Visual apprehension and perception in reading, *Psychol. Bull.*, 26, 1929, 223-240.

[2] F. D. Brooks, *The Applied Psychology of Reading*, 1926, 37.

[3] M. A. Tinker, The effect of color on visual apprehension and perception, *Genet. Psychol. Monog.*, 11, 1932, 61-136.

The group's average width of reading fixation was 1.64 words, S.D. 0.43, while their average perception-span was 3.39 words, S.D. 0.76. It will be noted that according to Brook's contention, the width of fixation is less than one-half of what it might be, but studies of superior readers[4] indicate that their reading-span never does approximate such a value and studies in training indicate that they cannot be made to do so.[5]

The point of interest here is, however, the actual relationship between these two variables. The correlation found was 0.52±.07 which is substantiated by the correlation of 0.46±.06 found by Litterer.[6] The higher correlation in this experiment, if there is a true difference, may be attributed to the use of identical materials and similar procedures in the two situations which Litterer did not have, or possibly to a slightly greater range of reading ability. It might also be noted here that as an aspect of an earlier study[7] the writer correlated these two variables using a small group of a very restricted range of reading ability. The correlation found was very small, but the discrepancy is explainable in terms of the range of reading ability measured. In fact that experiment led to this study.

These results indicate that perception-span as measured by the usual tachistoscopic procedure is somewhat related to reading span but that such a perception-span probably should not be used for predicting what the reading span is, or what it should be.

In order to determine if the use of a tachistoscope which was more like the reading situation would raise this correlation, the tachistoscopic procedure was changed in the following manner. Since, in reading, the theme of the story indicates something of what is to come, in this tachistoscopic experiment the sentences were presented in their meaningful order. To present these sentences one at a time would not, however, duplicate the reading situation because the long waits between exposures would allow the meaning to drop out and the psychological set would not be like that in reading. So in this study a tachistoscope was built which allowed *O* to read a paragraph down to the last line which was then exposed tachistoscopically for 100 σ by his pressing a key (see Fig. 1). It will be noted that the exposure of the sentence after the return sweep—which is the most pronounced break in the time sequence of reading—introduced as little distraction as possible into the reading. The students were given several trials to introduce them to this coordination and they had already used the apparatus for exposing single sentences in the other experiment on perception.

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[4] R. Y. Walker, The eye-movements of good readers, *Psychol. Monog.*, 44, 1933, (no. 201), 95-117.

[5] C. C. Ring and M. Bentley, The effect of training upon the rate of adult reading, this JOURNAL, 42, 1930, 429-430.

F. P. Robinson, The rôle of eye-movements in reading with an evaluation of techniques for their improvement, *Univ. of Iowa Studies: Series on Aims and Progress of Research*, No. 39, 1933, 1-52.

[6] O. F. Litterer, An experimental analysis of reading performance, *J. Exper. Educ.*, 1, 1932, 28-33.

[7] F. P. Robinson, *op. cit.*, 35-37.

This apparatus is like the reading situation in several other respects. The material appears as a book-like surface-plane at an efficient angle for reading; the visibility of the moving part is minimized by painting it the color of the paper and screening it as much as possible; the lighting comes from an ordinary reading lamp in a conventional position for reading; and the *O* is unrestricted as to his sitting position.

The controls used to make the two tachistoscopic and eye-movement situations comparable, besides those already described, were as follows. Not only was all of the same story used in each situation through rotation, but in the two tachistoscopic tests the same sentences were used. Since the last line of each paragraph had been written to stand grammatically alone as a sentence, these were taken and placed in an unrelated order for the first tachistoscopic experiment. Thus any difference found in the spans for the two tachistoscopic situations can be attributed to the meaning added by the previous story.

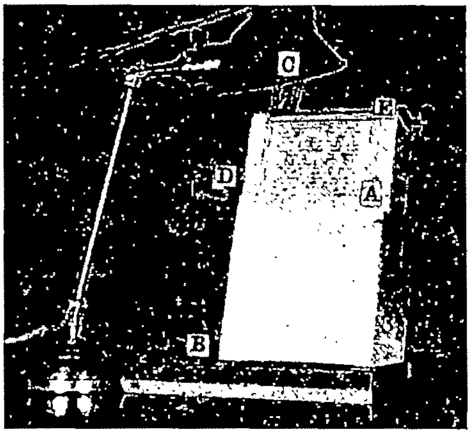

FIG. 1. A TACHISTOSCOPE FOR THE STUDY OF READING

It will be noted that this exposure device allows the reading of the printed material (as much as 20 lines) down to the shield (A) which covers one line. When *O* reaches the end of the last visible line and jumps back to where the first of the next line will begin, he presses the key (B) which causes the covered line to be exposed for 100 σ. The pressing of this key releases an unbalanced rocker-arm switch which has contacts on either end. During part of the swing of the falling arm, both contacts are out of their mercury cups which breaks the circuit to the magnets that hold the exposure arm up. The distance of the fall of this rocker-arm determines the interval of exposure. Thus the time interval can be changed by raising or lowering the mercury cups. The P.E. of such a timing device is about 4 σ. The reading lamp (C) illuminated the material and the mirror (D) is used for checking the location of the reading material as it is rolled up on roller (E) after each exposure. To obtain the usual tachistoscopic set-up, single sentences were exposed behind this arm without any preceding reading matter.

The group's average perception-span for this modified tachistoscope was 3.97 words S.D. 0.81, for 30 presentations. This is 17% greater than their span on the conventional tachistoscope and represents a reliable difference (10.51 times the the S.E. diff.) The intercorrelation of these two perception-spans is 0.88 (0.91 attenuated). Two points of interest are indicated by these results. First, the basic perception span, as it has been related to reading, is larger than it is usually described. Thus the average reading span of this group represents only 41% of their perception-span. Secondly, it might be noted parenthetically that since the added reading material in this tachistoscopic situation has resulted in an increased span, the difference found might be taken as an objective measure of the meaning of the previous story. That is, it would be a measure of the meaning as it contributes to the idea of the exposed sentence.

The point of particular interest here, however, is the relationship of this perception-span to the average width of reading fixation. The correlation was 0.66±.05. This is 0.14 higher than the correlation between the usual tachistoscopic and reading spans and represents a reliable difference (4.07 times the P.E. diff.). Thus the alteration of the tachistoscopic procedure into one more closely approximating the reading situation produces a better measure of reading perception than the usual tachistoscopic procedure, but even yet it does not produce a good measure of what the reading span is or even what it should be. Furthermore, the interrelationship of these spans is not sufficient to explain the correlations found between perception-span and reading ability. Other factors, such an intelligence and motivation, are probably also important.

The conclusions of this study are as follows. First, the usual tachistoscopic test is not a very good measure of the average span used in reading ($r = 0.52$) nor probably of the limits of its development. Secondly, the technique developed in this study for measuring perception-span after having read a paragraph of meaningful material is a reliably better measure of the reading span (4.07 times the P.E. diff.), but it still is not a very good predictor of eye-movement behavior ($r = 0.66$). Since most other measures, however, do not correlate very highly with reading ability, these results indicate that the technique described here has some value for the analysis of reading ability. The feasibility of using this approach to analyze the accuracy of reading perception has yet to be evaluated.

AN IMPROVED RESETTING CHRONOSCOPE

By SAMUEL RENSHAW and I. L. HAMPTON, Ohio State University

The first magnetic resetting chronoscope employing a new type of clutch was designed and built in 1929 and was shown by us at the Cornell meeting of the American Psychological Association in September, 1932. A description of the first model of the non-resetting instrument was printed in this JOURNAL.[1] Certain improvements, including the improved reset mechanism and its circuit, seem to warrant this brief description.

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[1] A combined chronoscope and interval-timer, *op. cit.*, 43, 1931, 637 f.

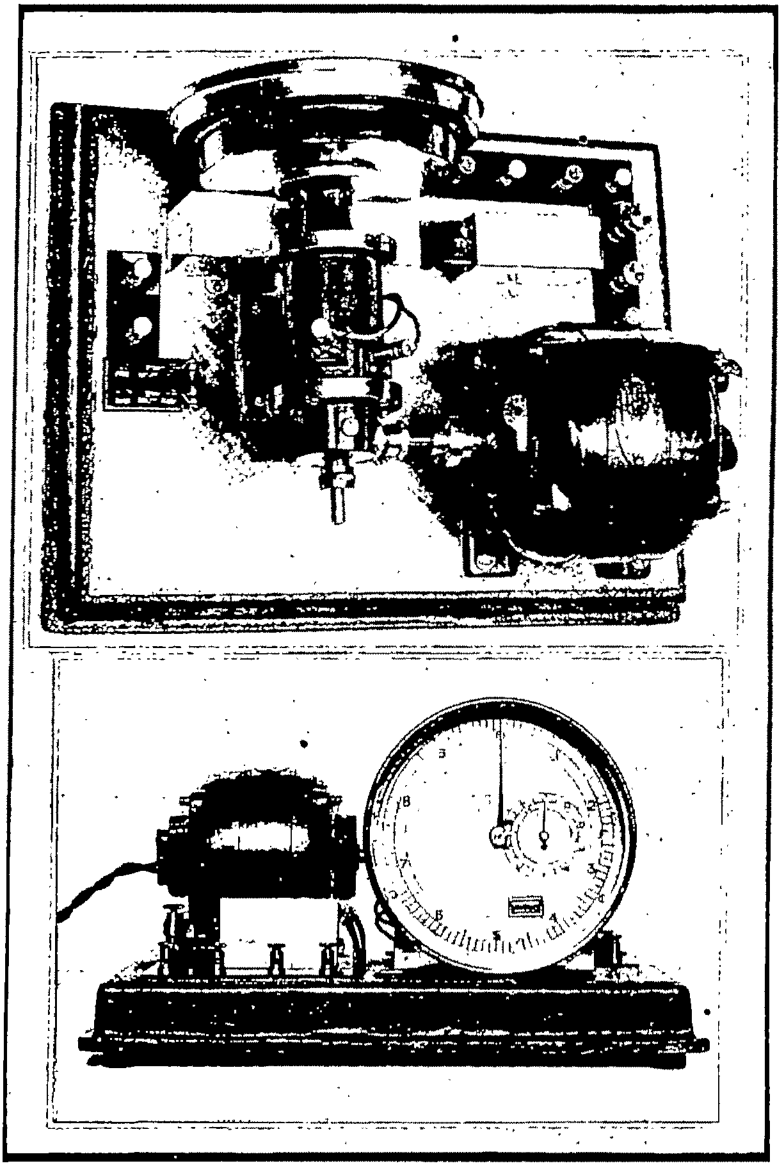

Fig. 2. An Improved Resetting Chronoscope

This instrument possesses several features which are improvements on most existing chronoscopes, and which increase its usefulness in many experiments where accurate timing is desired.

The dial is large and easily read. Seconds, hundredths-seconds, or sigmas may be read off directly, as one pleases. Times from sigmas to hours can be measured with great accuracy. The dial is enclosed in a dust-tight housing with a beveled plate glass cover. The non-resetting Veeder counter provides for the immediate recording of times greater than one second. Pressure on the reset button, located just below the dial to the left, resets to 0. Resetting can be done also by remote control by the use of a pair of binding posts in parallel with the reset push button. Cumulative timing useful in certain kinds of experiments is accomplished by a single make-break key.

The clutch engages at any point in its circumference. There are no teeth to mesh. The holding power of the clutch is several hundred times the actual demand for moving the index hands. From this detail we derive a high degree of constancy. One hundred consecutive timings of a contact-pendulum falling through 20° of arc gave the following distribution:

Time (sec.)	n
.169	1
.168	8
.167	32
.166	41
.165	15
.164	3
Total	100
Mean	.1663
S.D.	.00095

The instrument has few moving parts and cannot be tinkered into a new set of error constants. When it is once calibrated, the only variable is the constancy of the cycles of the 110-volt A.C. current supplied to the synchronous motor. In most modern power plants this is held to a very low range of variation. The accuracy of timing with the instrument depends upon the equalization of the starting and stopping lags. The former is determined by the time required to build up the magnetic field in the clutch coil and the latter is determined by the energy stored in the music wire spring of the rotating member of the clutch. Since this moves only 0.0025 in. in engaging it is possible to balance these lags very closely and any discrepancy can be detected by calibration of the constant error, which then can be added or subtracted to the readings. All construction of moving parts is to precision standards. Occasional oiling is the only attention needed. The whole mechanism operates with very little noise, the A.C. hum of the motor being the largest contributor. The resetting through an external key makes possible mounting the entire instrument in a sound proof box with a glass window in those experiments in which complete silence is required.

The interval timer can be attached as a separate unit to the main drive shaft which is seen extended from the reducer unit, at the rear of the clutch. A chuck mounted on this shaft, carrying a commutator disk with suitable contacts, will give a convenient and readily accessible source of seconds or fractions of one second. A simple stepping unit of the star wheel type can be made to provide for the control of much larger temporal intervals. A light-chopping disk can also be mounted on the main shaft and a photocell circuit used in those experiments in which such apparatus is desired.

The cost of the unit complete is no greater than the most widely used similar instrument, and its superiority has been clearly demonstrated in our laboratory.

A 6-volt D.C. current operates the clutch and make-break circuits. Two binding posts, shown at the right of the dial, are in series with the clutch coil. Thus in a reaction-experiment the stimulus can be delivered simultaneously with the engaging of the clutch. A holding relay is energized by the *E*'s key so that the *E*'s reaction time is entirely eliminated. The resetting is done by the clutch itself. When the reset key is depressed the clutch coil is energized through a special holding relay which returns the index hands to the vicinity of the zero point. Here the circuit is mechanically broken and a magnet is energized which forces its armature pawl into the hilum of a cordate cam which completes the return of the index hands to the exact zero position. Resetting occupies on the average about 0.5 second. The essential units are mounted on an iron base casting 10 x 13 in., resting on rubber feet. This casting is faced with a plate of polished non-rusting, non-corroding Enduro iron.

A SWITCH FOR THE PRESENTATION OF ELECTRICALLY GENERATED TONAL STIMULI

By S. S. Stevens, Harvard University

In the presentation of electrically generated tonal stimuli it is frequently of great importance that the click which accompanies an instantaneous 'make' or 'break' of the circuit be eliminated. This click can be suppressed if the potential across the receiver is changed gradually and not abruptly both at the beginning and at the end of the presentation.

The device shown in Fig. 3 provides a convenient means of presenting 'clickless' tones. It consists of a two-pole, double-throw switch to the terminals of which are fastened brass extensions. These extensions make contact with the blades of the arm of the switch and maintain that contact throughout all but a small sector of the arc through which the arm swings. A string runs from the arm over a small pulley, situated directly above the 'off' position of the switch, to a pin fastened in the edge of a gear of 2½-in. diam. This gear drives a small gear, 1-in. diam., fixed on a shaft, on the other end of which is mounted a small wire-wound, variable resistance. This resistance is permanently connected across the receiver so that when its value is minimal the current is shunted through it and does not go through the receiver. Opposite the pin, in the larger gear to which the string is fastened, is another pin to which are attached enough rubber bands to pull the resistance back to its 'off' position whenever the tension on the string is relaxed.

The operation is simple. When the arm of the switch is in the vertical position, between the two pairs of brass extensions, the circuit is open and the resistance is low. As the arm moves to the right or left (depending on which tone is to be presented first), it makes contact with the brass extensions and closes the circuit for one of the tones. Since the resistance is still low, however, the receiver is short-circuited, and is not activated. Then, as the arm moves farther, the string begins to turn the gears and the variable resistance, introducing resistance into the shunt across the receiver, and thus increasing the amount of current through the receiver. The tone is terminated by the reverse process; and the second tone can be turned on and off in a similar manner by the other half of the switch.

This switching device has been used in the output circuit of an oscillator in an experiment in which two tones were presented alternately at the rate of about 40 per min. At this rate of operation the variable resistance requires about 0.2 sec. to make a complete excursion from the 'off' to the 'on' position. Under these conditions the click is effectively eliminated.

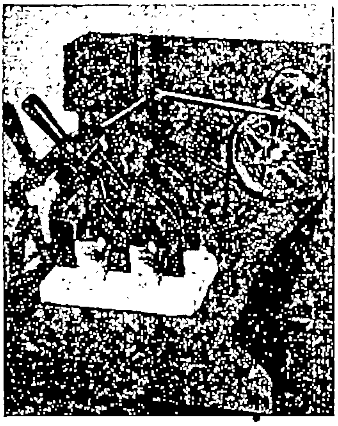

FIG. 3. SWITCH FOR THE PRESENTATION OF ELECTRICALLY GENERATED TONAL STIMULI

The magnitude of the variable resistance used should be determined by the characteristics of the circuit and of the receiver or loud speaker. In the experiment mentioned a resistance of 5000 ohms was used in connection with a Western Electric earphone and a resistance of 2000 ohms with a pair of Jenson loud speakers. The effectiveness of the device could be somewhat improved if a variable resistance wound on a logarithmic form were used. With this type of winding the resistance is increased slowly at the beginning, but rapidly towards the end of the excursion of the switch.

A MICRO-RECORDER ADAPTER FOR USE WITH KYMOGRAPHS AND POLYGRAPHS

By Edward W. Geldreich and Theodore W. Wood,
University of Cincinnati

Investigators who have had experience with various recording devices, viz., tambours and other markers, have found difficulty in causing the styluses of such instruments to make proper contact with the paper of kymographs and polygraphs.

Recording instruments have been designed to eliminate a part of this difficulty. Such designs usually operate with a cog gear that provides a horizontal or parallel displacement or approach of the recording device to the plane of the paper on the kymograph or polygraph, but they do not provide a perpendicular displacement or approach of the stylus to the plane of the paper. Obviously, in actual operation, this limitation involves first the lining up all the recording instruments parallel to the axis of the drum of the kymograph or polygraph, and then the moving of each instrument to the drum, when it is often discovered that the whole ringstand must be set over to secure the optimum angle for contact of the stylus and operation. There is also the necessity for frequent and delicate adjustment while records are being made.

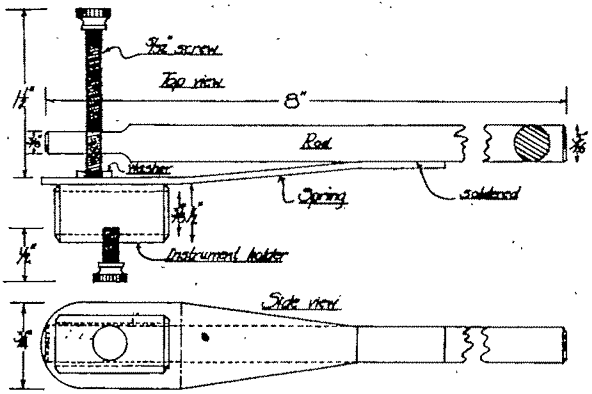

Fig. 4. Diagram of the Micro-Recorder Adapter

An instrument designed by the authors eliminates this difficulty by causing the stylus of any recording device to approach the recording paper slowly and to make fine contact with it. The essentials and dimensions of this device are shown in Fig. 4. One feature of the construction must be carefully observed: the hole through the brass block, which is to hold the recording instrument, should be drilled straight and large enough to accommodate any recording instrument commonly used in conjunction with kymographic and polygraphic work. We have found that a ⅜-in. hole carries most of the instruments used in psychological and physiological research.

AN ADJUSTABLE MAZE FOR VISUAL AND NON-VISUAL LEARNING

By SHIRLEY MORRISON and T. W. COOK, Acadia University

The maze shown in top and edge view in Fig. 5 is an adaptation of the Shepard Adjustable Maze for visual as well as non-visual learning with human subjects. The top of the maze consists of 33 rows of wooden blocks separated laterally by ⅛ in. These blocks are 1 in. square on the top and ¾ in. deep, and each is raised from the base by a ½ x ½ x ⅜ in. block glued to the center of its lower surface.

A 1¾-in. no. 6 round-headed screw through the center of upper and lower block holds both in position on the base. Each lower block is also glued to the base. Beneath each of the four sides of every cube a ⅛-in. hole is bored in the base. As is shown in the edge view, each of these holes just clears the middle of one side of a supporting block. Stops can thus be inserted or removed by turning the maze over and raising boards hinged to the bottom.

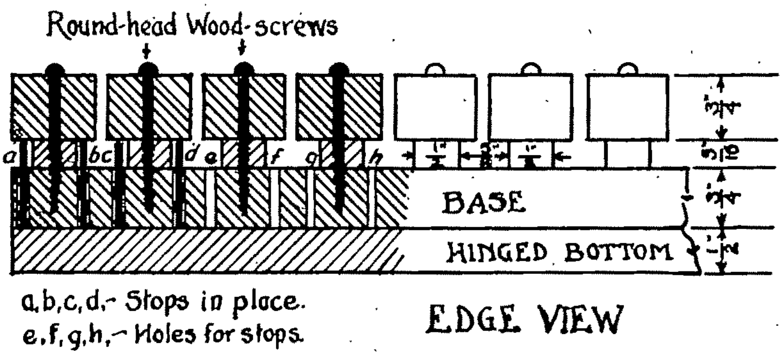

FIG. 5. ADJUSTABLE MAZE FOR VISUAL AND NON-VISUAL LEARNING

The maze has the advantage of the Shephard type: it permits the construction of a large number of comparable patterns. In addition, since the stops are concealed and all paths seem open when viewed from above, the present maze can be used for visual as well as non-visual learning.

Our model is of hardwood and the cost (except in labor) is trifling. Brass would of course be preferable if expense is not a major consideration.

NOTES AND DISCUSSIONS

THE PRONUNCIATION OF *Liminal*

In the galley proofs of the forthcoming *Dictionary of Psychology and Cognate Sciences* Professor H. C. Warren, the general editor, says that the word "liminal" should be pronounced "lĭm-." As the pronunciation recommended was strange to me, I queried it, only to have a rule of pronounciation quoted to me that "the long vowel in the noun is, in accordance with Latin etymology, shortened in the adjectival form."

The rule, if it is a rule, finds ready illustration (*e.g.* stamen-staminal; semen-seminal; nomen-nominal; crime-criminal; line-lineal; type-typical; bible-biblical; femur-femoral). But instances to the contrary—*i.e.* where the vowel in the noun remains long in the adjective—occur also with great frequency (*e.g.* cerumen-ceruminal; lumen-luminal; anus-anal; base-basal; locus-local; cement-cemental; Ceres-cereal; delusion-delusional; feud-feudal; fetus-fetal; flora-floral; lay-laical; labium-labial; legion-legionary; leo-leonine; lichen-lichenal; and the adjectival form of words ending in '-tion,' viz., notion-notional; sensation-sensational, etc.).

It must, furthermore, be pointed out that Latin etymology offers no sanction for the rule mentioned above. The long 'i' in 'līmen' and 'vīta,' for example, ***remains long*** in the adjectives 'līminālis' and 'vītālis.' But, as Jespersen says, "in Latin words it is not the classical quantity that is decisive, but the English school-pronunciation which followed its own rules."[1] The rule followed in this case is the so-called 'three-syllable rule' that "when a stressed syllable is followed by two (or more) weak ones, there is a strong tendency to shorten it."[2]

Rules, since they are known by their exceptions, and examples, since they are merely specific instances of usage, offer an insecure basis upon which to determine the correct pronunciation of any given term. The final arbiter in questions of this kind is authoritative usage. "Limen" and "liminal" are psychophysical terms. The psychologists, and in particular the psychophysicists, have, therefore, a right to be heard.

In order to discover the pronunciation actually used by competent men, I sent postal reply cards to 64 psychologists: to the past presidents of the American Psychological Association, to the psychophysicists of this country,[3] and to the heads of our foremost departments. I asked them to designate upon the reply cards, which carried the alternative pronunciations diacritically marked, the pronunciation that they used. Without having recourse to a 'follow-up,' I received 61 replies. Two of this number, however, count as blanks: one because my correspondent did not know which, if either, was correct; and the other because both pronunciations were used indiscriminately. Of the 59 positive replies,[4] 32 indicated the usage of "lim-," and 27 of "li-."

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[1] O. Jespersen, *A Modern English Grammar,* Pt. 1, 1909, 131.

[2] *Op. cit.,* 122. Cf. also the section on quantity in Romance (Latin and French) words (pp. 130-145), particularly pp. 139 f., where certain exceptions "in some modern (chiefly learned) words" are cited.

[3] I counted as psychophysicists all those who had published articles on that subject or were at present teaching courses in it.

An analysis of the replies, given in the accompanying tables, reveals striking differences among the three groups circularized. As there is considerable overlapping among the groups—many past presidents, for example, are heads of departments and some are psychophysicists—a dichotomy has been made for every group. Table I shows the usage of presidents and not-presidents. About two-thirds of the presidents (18 to 8) shortened the vowel in the adjectival form; whereas the majority of those not having held this office (19 to 14) used the long vowel.

TABLE I

SHOWING THE PRONUNCIATION USED BY PAST PRESIDENTS AND BY ALL OTHERS INTERROGATED

	"li-"	"lim-"
Past presidents	8	18
Not-presidents	19	14
Total	27	32

TABLE II

SHOWING THE PRONUNCIATION USED BY PSYCHOPHYSICISTS AND BY ALL OTHERS

	"li-"	"lim-"
Psychophysicists	21	6
Non-psychophysicists	6	26
Total	27	32

TABLE III

SHOWING THE PRONUNCIATION USED BY HEADS OF DEPARTMENTS AND BY ALL OTHERS

	"li-"	"lim-"
Heads of departments	13	16
Not-heads	14	16
Total	27	32

The results of the poll of the presidents stand strikingly in opposition to those obtained from the psychophysicists (Table II), for the great majority of the latter (21 to 6) use "li-." The non-psychophysicists use the alternative pronunciation, "lim-" in even greater proportion, (26 to 6). The division here is so marked that pronunciation may be taken as a rough indicator of interest in psychophysics. Indeed, one of my correspondents, a past president, a psychophysicist, and a head of a department, predicted this result. He wrote, " 'li-' is usual in psychophysics, 'lim-' more usual in psychical research and in the psychopathology of 1910 *et ante*."

The replies of heads of departments and not-heads are shown in Table III. Usage in both of these groups is about evenly divided (16 to 13, 16 to 14), but in favor of "lim-." This division shows us, first, that eminence is about equally divided between the alternatives, and secondly, that eminence is as variable as the lack of it.

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[4] One of the correspondents, a past president, wrote that he used "lim-" but preferred "li-." Since we are interested in usage, his reply was tallied as "lim-."

In order to discover whether pronunciation was geographically conditioned, a regional classification was made of the replies. As shown in Table IV, the proportion of "li-" to "lim-" is 8 to 6 in New England, 8 to 9 in the East, 2 to 4 in the South, 8 to 7 in the Mid-West, and 1 to 6 in the Far West. With the exception of the Far West, which shows a decided preference for "lim-," the returns are about equally divided. These results are not, however, fair indicators of geographical influence, for pronunciation in every case was doubtlessly established long before the correspondents attained their present positions. Since we are dealing with a technical term, one not learned in childhood but only in professional study, a better indicator would be the place of study. The replies were accordingly classified (1) upon the basis of the place of the first degree, and (2), since the term may not have been met sufficiently often in undergraduate study to condition pronunciation, upon the basis of the place of the doctorate. The results, given also in Table IV, show but slight differences. There is no indication of sectional preference.

TABLE IV

SHOWING THE PRONUNCIATION USED IN THE VARIOUS SECTIONS OF THE COUNTRY

		New Eng.	East	South	Mid-West	Far West
Present	"li-"	8	8	2	8	1
	"lim-"	6	9	4	7	6
A.B.	"li-"	7	10	1	3	6
	"lim-"	6	11	2	8	5
Ph.D.	"li-" (2)*	6	15	1	3	0
	"lim-" (4)*	8	10	3	7	0

* The numbers in parentheses indicate degrees from Germany.

TABLE V

SHOWING THE PRONUNCIATION USED BY THOSE OBTAINING THE DOCTORATE IN THE SEVERAL DECADES

	"li-"	"lim-"
Before 1901	3	12
1901–1910	5	12
1911–1920	11	7
Since 1921	8	1
Total	27	32

Since the past presidents, the majority of whom received their professional training before the turn of the century, show a bias in favor of "lim-," it may well be that this pronunciation is the older, and that "li-," the pronunciation of the psychophysicists, has only gradually come into usage with the advance of psychophysics in this country. In order to test this conjecture, the replies were classified upon the basis of date of the doctorate (Table V). Of those receiving their professional training before 1901, the numbers using "li-" and "lim-" are respectively 3 and 12; between 1901-1910, 5 and 12; between 1911-1920, 11 and 8; and since 1921, 8 and 1. Though this analysis gives added warrant to our conjecture, it shows that there is no unanimity at any period and further that pronunciation is listing decidedly in the direction of "li-."

In view of these results a lexicographer is certainly not justified in being dogmatic. If he records usage he can do no less than to give both pronunciations of "liminal." If he wishes to standardize usage he cannot, without defying the present trend, declare for "lĭm-" and against "lī-."

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ARE TONES SPATIAL?

The behavior of congenitally blind *O*s in experiments involving judgments of the extension or spatiality of those experiences which originate through hearing is important to the understanding of the nature of certain auditory phenomena. The question of the relation of tonal volume to visual space can be readily resolved if it can be shown that an *O* having no experience of space is yet able to make judgments of the phenomenal volume of tones.

In the course of an experiment on tonal volume the services of Miss S. V. Fladeland, a congenitally blind instructor at the Perkins Institute for the Blind, were enlisted for the purpose of determining what effect the absence of visual imagery would have upon an *O*'s ability to equate for volume two tones of different pitch by changing the intensity of one of the tones. This *O* was familiar with the concepts *pitch* and *intensity* but had never heard of *volume* as a phenomenal dimension of tones. Consequently it was necessary to train her to recognize volume. In order to do this, she was presented with a high tone (3000 cycles) and asked to notice its 'size.' Then she was given a low tone (200 cycles). She came quickly to understand what was meant by *volume*, and described the low tone as being "larger" than the high tone. The dependence of volume upon the intensity of the tones was made clear to her in an analogous manner, and she found that she could very easily make two tones equal in volume by changing the intensity of one.

The quantitative results obtained by the process of equating two tones in volume agreed closely with those obtained from normal *O*s.[1] After a series of 32 judgments it had become possible for this *O* to abstract the 'size' of the tones from their pitch and loudness without the aid of concrete imagery. Her report subsequent to the experiment was as follows.

"I used an arbitrary basis at first to establish my sense of volume. I compared the tone to a tube, or jet, through which steam passes to produce a sound, and by using my hand as a cup to fit the escaping steam I was able to determine its size. All of the tones used in the experiment would be close to the size of a not-too-expanded teacup. The high tones, however, were not only smaller, but they seemed to rise in space."

To the question, "Where were the tones?", she replied that they were on the right arm of the chair, in a position such that she could cup her hand around them. In order to cup her hand around the high tones, she had to raise her hand two or three inches above the arm of the chair. She explained that the raising of her hand for the high tones was probably due to the fact that in teaching speech to blind children she habitually made an upward gesture when she raised her voice.

The significance of the results obtained from this *O* is that, in the first place, they show that volume is a true phenomenal dimension of tones in that it can readily be observed by an *O* unaided by visual imagery. Bannister,[2] in the course of his argument against the validity of volume as a category of experience, reported the case of an *O*, blind since the age of three, who "has never been able to experience or to understand the attribute of volume, although it has been suggested to him." It would be interesting to see whether, with systematic training, this *O* could not be taught to discriminate volume as Miss Fladeland did.

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[1] An account of tonal volume as a quantitative function of frequency and intensity for various *O*s, including the present *O*, is accepted for later publication in this JOURNAL.

In the second place, the work with Miss Fladeland shows that the systematic identification of tonal volume with the truly spatial aspects of experience must be made with caution. Although the congenitally blind habitually employ the words *space, distance, size,* etc., it may well be that they understand by them something other than what the same words connote to a person with normal vision. Sir Wm. Hamilton[3] insisted that the observations of Dr. Platner[4] were sufficient proof that the congenitally blind have no concept or experience of space as we know it. The blind, he argued, experience only time and effort or motion, and the distance from one side of the room to the other means to them only so much time and so much movement. In 1844 Hagen put forth a similar argument.[5] In speaking of the congenitally blind he said, "Was uns Raum ist, ist bei ihnen bloss Zeit." More recently Goldstein and Gelb have shown that a patient who lacked all capacity for forming visual imagery was completely devoid of the sense of spatiality.[6] They concluded that through the *Tastsinn* alone one can never achieve *Raumvorstellungen,* and that consequently "es gibt eigentlich nur einen Gesichtraum." If this conclusion is true, it must follow that if a person who is incapable of experiencing space is nevertheless able to experience tonal volume, tonal volume is not a spatial experience.

It is a fact, however, that most normal *O*s report that their judgments of tonal volume are based upon the 'space filling" aspect of tones. They also report the use of visual imagery in most instances. As Goldstein and Gelb pointed out, a similar situation is encountered in the case of tactual localization. The argument of these authors was that the persistent presence of visual imagery as a concomitant of tactual experience leads the normal *O* to believe that touch, as such, has spatial qualities. If the same argument is applied to the discrimination of tonal volumes, we may conclude that it is only because of the presence of a visual surrogate that tones seem to have true phenomenal extent.

Some *O*s have directly denied the spatiality of the tones whose volume they were successful in judging. For instance, one of Rich's[7] *Os* reported: "It is a different kind of size from visual size. . . . Space is so visual to me that, when I open my eyes, it seems a sort of folly to me; but when I close my eyes, I feel the massiveness of the tones." In the course of an experiment conducted by the author, one of the *O*s said: "There is nothing spatial about volume. I match two tones; I lon't compare two extents. It is more a matter of massiveness than size, because ize implies space." This *O* also reported that he was aware of no visual imagery vhen he judged volume.

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[2] H. Banister, Auditory theory; a criticism of Professor Boring's hypothesis, this JOURNAL, 38, 1927, 436-440.

[3] Sir Wm. Hamilton, *Lectures on Metaphysics,* II, 1861, 173-175.

[4] Ernst Platner, *Philosophische Aphorismen,* I, 1793, 446.

[5] F. W. Hagen, in R. Wagner's *Handwörterbuch der Physiologie,* II, 1844, 718.

[6] K. Goldstein and A. Gelb, Über den Einfluss des volstandigen Verlustes des optischen Vorstellungsvermögens auf des taktile Erkennen, *Zsch. f. Psychol.,* 83, 1920, 1-94.

[7] G. J. Rich, A preliminary study of tonal volume, *J. Exper. Psychol.,* 1, 1916, 19 f.

It would seem, then, that there is no good evidence that tones, as such, have ruly spatial characteristics, and that one is justified in attributing space or exensity to auditory experience only if he is willing to accept the existence of a patial surrogate as a sufficient basis.

Harvard University
S. S. STEVENS

PSYCHOLOGICAL JOURNALS

The members of the senior author's class in the "Use of Psychological Literaure" asked the question: "With a limited amount of money to spend what journals n psychology would be the better ones to buy?" In these days of reduced budgets his question should be widely pertinent. A corollary question might also be asked: 'Of what psychological journals should even a small working collection contain a :omplete file?" The present note aims to suggest answers to these questions in erms of the actual use of these journals as represented in current American litrature.

One number (usually the first or second) of the current (1933) volume of ach of the following journals was searched for references to the literature: *Amer. . Psychol., J. Abn. & Soc. Psychol., J. Appl. Psychol., J. Comp. Psychol., J. Educ. 'sychol., J. Exper. Psychol., Ped. Sem. & J. Genet. Psychol., J. Gener. Psychol., J. 'oc. Psychol., Psychol. Rev.* This list is a fair cross section of the experimental and heoretical literature of the present time.

In these numbers 1167 references to the literature were found distributed among 92 different periodicals, Table I gives the frequency distribution for those journals ited less than ten times.

Those journals cited more than ten times are presented in the following list with he numbers of citations in parentheses: *J. Educ. Psychol.* (109), *Amer. J. Psychol.* (104), *Psychol. Rev.* (76), *J. Exper. Psychol.* (70), *Ped. Sem. & J. Genet. Psyhol.* (55), *Psychol. Monog.* (49), *J. Abn. & Soc. Psychol.* (46), *J. Comp. Psychol.* (41), *Arch. Psychol.* (31), *J. Appl. Psychol.* (28), *Psychol. Bull.* (28), *Amer. J. 'hysiol.* (21), *School & Soc.* (21), *Univ. Calif. Pub. Psychol.* (18), *Zsch. f. Psyhol.* (17), *J. Gener. Psychol.* (17), *J. Anim. Beh.* (15), *Science* (15), *Genet. 'sychol. Monog.* (15), *Brit. J. Psychol.* (14), *J. Physiol.* (13), *Pflüger's Arch.* (12), *:omp. Psychol. Monog.* (12), *Yearbk., N.S.S.E.* (12).

Thus we find that 12.5% of the journals provide 63.5% the references. Furthernore the 192 journals referred to represent only about 25% of the current publicaions that may be considered pertinent to psychology and its related fields. Louttit[1] ias published a list of journals in psychology and its related fields which contains .084 entries of which only 380 have ceased publication. Therefore we must conlude that, in so far as this sample is representative, American psychologists do not lraw very widely from the available literature.

Table II presents, by half-decade intervals, the temporal distribution of these eferences. Nearly 75% of them were to papers published since 1920. Can it be hat work published previous to this time is so thoroughly a part of the tradition hat there is no need to refer to it?

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[1] C. M. Louttit, *Handbook of Psychological Literature,* 1932.

Of the journals cited more than ten times, *i.e.* those listed above, there are only three which have not been referred to as early as the first half-decade of their life. Of these the *Univ. Calif. Pub. Psych.* started in 1910 but had a long period when no numbers appeared. *Science* started its new series in 1895 but it was not referred to previous to 1910. Eleven of the twelve references to the Yearbook of the N.S.S.E. are to the famous 27th number.

The third factor of interest is the frequency of references to the various languages. As shown in Table III, nearly 70% are to journals in English—the majority of which are American. Only 25% are to German journals although there are about as many journals published in that language as in English. Among the twenty-four most frequently cited journals there are only one British and three German.

TABLE I

JOURNALS CITED LESS THAN 10 TIMES

Citations	Journals
1	103
2	31
3	11
4	8
5	4
6	5
7	3
8	0
9	3

TABLE II

TEMPORAL DISTRIBUTION OF CITATIONS

Period	No.	%
1930–32*	266*	22.8*
1925–29	433	37.1
1920–24	171	14.7
1915–19	77	6.6
1910–14	90	7.7
1905–09	57	4.9
1900–04	24	2.1
1895–99	21	1.8
1890–94	11	.9
1885–89	4	.3
1880–84	6	.5
1875–79	2	.2
1870–74	2	.2
1865–69	1	.1
1860–64	1	.1
1830–34	1	.1

* Only two years.

TABLE III

LANGUAGES OF JOURNALS CITED

Language	No.	%
English		
U.S.	111	57.8
Gr. Brit.	19	9.9
German	49	25.5
French	10	5.2
Italian	1	.5
Japanese	1	.5
Spanish		
(Argentine)	1	.5

Thus the answers to the original questions would appear to be that with a limited budget the twenty-four listed above should be purchased first. Another conclusion that might be drawn is that American psychologists make all too little use of the foreign literature.

Indiana University

C. M. LOUTTIT
LILLIAN L. LOCKRIDGE

THE TERM *Protensity*

The suggestion[1] that Professor Titchener coined the term *protensity* is incorrect. The first philosophical writer who used the term and its derivatives was Sir William Hamilton, who spoke of "Time, Protension or protensive quantity, called likewise

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[1] Cf. E. G. Boring (*The Physical Dimensions of Consciousness,* 1933, 128), who refers to Titchener's first use of the term in a short note in this JOURNAL, 35, 1924, 156.

Duration." Quotations from Hamilton appear under these terms in the *New English Dictionary.*

Protensity appears also in James Ward's article "Psychology" in the *Encyclopedia Britannica*[3] in 1886. Here the term is said to be one of Hamilton's, and its meaning is also Hamilton's "time." In Ward's *Psychological Principles* (1918) the term is used, but it means not time in a physical sense but the temporal attribute of sensation. On p. 119 he writes: "The protensity ascribed to a sensation is—in a sense—the equivalent of the duration of the stimulus upon which its presentation primarily depends. But of this duration as immediately experienced, the subject, and not the stimulation as an external change, furnishes the measure, a measure that varies widely from subject to subject—according to the *tempo* of each—and even somewhat for the same subject from time to time—according to circumstances—independently of 'objective' duration."

The word also appears in the third edition (1913) of G. F. Stout's *Manual of Psychology* as a name for an attribute of sensation, but it does not appear in the 1899 edition of the *Manual* or in the *Analytic Psychology* of 1896.

Rice Institute FRANK A. PATTIE, JR.

A FURTHER NOTE ON THE 'WOLF CHILDREN' OF INDIA

Since the publication of the last note on the 'wolf children' of India,[1] I have received word from Rev. J. A. L. Singh, in whose orphanage the children lived.

Rev. Singh writes that the records of the two children are soon to be published, and encloses in his letter a statement regarding the children from Dr. S. P. Sarbadhicari, the physician who attended them from their capture to their deaths.

Dr. Sarbadhicari's account, which is quoted below in full, is of particular significance, not only because it gives us additional information, but also because of its corroborative evidence. His statement runs as follows.

"I was called upon for the treatment of Amala and Kamala, the wolf children, aged about 4 and 9 years respectively, on the 11th of September, 1921.

"They were suffering from emaciation, loss of appetite, etc. I suspected round worms, and on suitable treatment they passed out a very large number of them.

"As they could not be induced to take mixed diet their health could not be improved upon and Amala, the younger girl, died on the 21st of September, 1921, of Nephritis and general Oedema after suffering from the same for about 3 weeks. Curiously enough the older girl Kamala died also of the same disease on the 14th of November 1929. As a post-mortem examination was not made, the structural and pathological state of the kidney and other organs was not ascertained.

"I noticed the following peculiar characteristics in the elder girl, Kamala. She was shy, averse to remain in company, gave furtive looks of peculiar keen and penetrating nature, answered questions in monosyllables, and would not look up to one's face. She never showed any ferocious instincts in my presence.

"By the untiring zeal of the rescuer, the Rev. Singh, she acquired some sort of education, and could express herself tolerably well later on. There was great difficulty in feeding the girl with a mixed diet as she would hardly relish a vegetarian diet except milk.

"The acquired instinct of taking raw animal food by being reared by a carnivorous animal was retained up to the last though all attempts were made to return to a normal human diet. The progress of education was very slow owing in part to the heredity of the subject (from aboriginal tribes) and also to nutritional defects, not contributing to the proper hormonal function.

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[3] *Encyc. Brit.*, 9th ed., 20, 1886, 65.

[1] For earlier notes, see P. C. Squires, 'Wolf children' of India, this JOURNAL, 38, 1927, 313-315; W. N. Kellogg, More about the 'Wolf children' of India, *ibid.*, 43, 1931, 508-509.

"In my opinion, if the unfortunate wolf girls could have been kept alive and induced to take proper mixed diet with properly adjusted vitamins, improvement would have been more marked and they could have returned to an ordinary human condition from the stage of animal."

These children stand out unquestionably as the most striking examples of 'wild children' in our time. The publication of the complete records are awaited with interest.

Indiana University W. N. KELLOGG

THE FORTY-FIRST ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The forty-first annual meeting of the American Psychological Association was held at the University of Chicago, Chicago, Illinois on Thursday, Friday, Saturday, Monday, Tuesday, and Wednesday, September 7, 8, 9, 11, 12, 13, 1933. Registration figures reached an all time peak of 825 persons, classified as follows: 158 Members, 323 Associates, and 305 persons not affiliated with the Association.

At the business meeting, 3 Members were elected, 13 Associates were transferred to the status of Member, and 135 Associate Members were elected. It was voted to accept the invitation of Columbia University for the 1934 meeting and to extend the usual three-day meeting to four days. The forty-second annual meeting will be held on Wednesday, Thursday, Friday, and Saturday, September 5, 6, 7, and 8, 1934. Professor A. T. Poffenberger was appointed a member of the Executive Committee for 1933-1934, and William T. Heron, Christian A. Ruckmick, and the Secretary were elected as the Program Committee for 1934. Newly-elected officers are: President, Joseph Peterson; Treasurer, Leonard Carmichael; Directors, Harold E. Burtt, and Florence L. Goodenough; Division of Anthropology and Psychology, N.R.C., John F. Dashiell, Walter R. Miles, and L. L. Thurstone; S.S.R.C. Gardner Murphy.

The Association voted to continue affiliation with the Inter-Society Color Council for the year 1933-34 and appointed Forrest Lee Dimmick, Clarence H. Graham and Sidney M. Newhall as delegates to the Inter-Society Color Council. Reports prepared by the newly established Committee on the Ph.D. in Psychology (W. S. Hunter, Chairman, Edwin G. Boring, Harvey A. Carr, A. T. Poffenberger, and Lewis M. Terman) were given, showing present practices in regard to standards for the Ph.D. degree and analyzing the present status of supply and demand for trained psychologists. The supply of both Ph.D.'s and M.A.'s so greatly exceeds demand as to constitute a serious problem. The Association voted to continue the committee and instructed the committee to prepare the facts for presentation to prospective graduate students in psychology who are thinking of teaching psychology as a career, and to consider the problem of directing graduate training in psychology in such a way that professional psychological service can make increasing contributions to community life.

Twenty sessions for formal papers were scheduled and 132 papers were presented. Again it appears that the present day emphasis in psychology is genetic as indicated by the volume of research activity devoted to the study of animals and young children. There is also marked interest in applied psychology, in the fields of abnormal and social psychology, and in the psychology of personality. Experimental work on sensation and perception and in psychophysics also exhibits marked vitality.

The policy of segregating research films and instructional films for special sessions seemed to meet with general approval. Nine research films and seven instructional films were shown. A Round Table on Instructional Films, with Walter R. Miles as chairman was held and was well attended. One of the twenty sessions for formal papers was devoted to papers on Clinical Psychology and two Round Tables on Clinical Psychology were also held. The Section on Clinical Psychology reëlected Edgar A. Doll as chairman for 1933-1934, and elected Harry A. Baker to be a member of the Executive Committee for 1933-1936. A total of 72 new Members and Associates was admitted to this section.

On Friday afternoon provision was made for the Psychological Corporation to hold a general meeting with papers and on Saturday afternoon a trip to the Mooseheart Laboratory for Child Research was arranged, at which eleven research papers were presented by members of the Child Research Staff.

On Monday evening, the Presidential Address, "The Vectors of Mind," was given by L. L. Thurstone. In brief, President Thurstone dwelt on the psychological significance of his recently developed mathematical method for determining the smallest number of factors which can account for the intercorrelations obtained from empirical measurements. Results of applying the Thurstone Method of Factor Analysis to measurements of vocational interests, to verbal and non-verbal intelligence test data, to attitude measures, and to data derived from case histories of psychopathic patients, were given to illustrate the usefulness and significance of this type of analytical approach.

Following the Presidential Address, the University entertained members in the Reynolds Club.

Professor Carr, his associates, and the administrative officers of the University of Chicago left no stone unturned to place the excellent facilities of the University at the disposal of our membership.

University of Minnesota DONALD G. PATERSON

Shepherd Ivory Franz: 1874-1933

Psychologists mourn the loss of one of the best loved and most productive of their number.[1] An early pupil of Cattell, and his assistant in developing the Columbia laboratory, Shepherd Ivory Franz, after obtaining his degree, devoted the next two years to physiology at the Harvard Medical School and taught this subject for a few years at Dartmouth before entering his characteristic life-work as psychologist in a mental hospital. This unique position he held first at McLean Hospital near Boston and then for many years at St. Elizabeth's in Washington. He was thus the pioneer, from the side of the psychologists, in a *rapprochement* between psychology and psychiatry, and his spirit of friendly coöperation combined with independence served him admirably in this relation.

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[1] On October 14, 1933, at the age of fifty-nine.

He was the pioneer also in combining psychology with brain physiology. As a young physiologist who had come over from psychology he sought some way of making his psychology of use, and he devised a method of attack in which he combined the brain physiologist's method of extirpation with Thorndike's then new experiment in animal learning. Thus he had a plan for studying the cortical localization of the higher, learned forms of behavior. By the use of this method he soon obtained results indicating a lack of rigid localization. An act once learned by an animal and then lost through removal of part of the cortex could be relearned, and might be lost again on removal of another portion of cortex, and even learned a third time. Pursuing the line of inquiry thus suggested to him, Franz found from the autopsies of demented patients, as collocated with their case histories, that neither the location nor the extent of a cerebral atrophy bore any constant relationship to the kind or severity of the behavior losses. On mapping the monkey's motor area very minutely, he found individual differences in the precise location and extent of the excitable area for any given movement. From these and similar results he drew the general conclusion that the nerve paths and centers available for any given performance were by no means rigidly laid down once for all, and "that the paths of reactions are not the simple anatomical unities which have commonly been believed in but that these paths are diffuse and that anatomically as well as physiologically they are complex." Lashley, initiated by Franz into this line of thought and experimentation, has developed the evidence still further in the same direction.

As a corollary of his concept of a "labile cerebrum" Franz doubted the incurability of paralyses resulting from cerebral lesions, and he succeeded, by patient reeducation combined with incentives such as games and the knowledge of actual improvement under practice, in restoring to many paralyzed patients a surprising amount of motility. He had some success also with aphasic patients, and with cripples of different types. He engaged in other varieties of clinical psychology, and one of his latest achievements was the redintegration of a case of multiple personality.

While serving as psychologist in St. Elizabeth's Hospital, he was also in charge of experimental psychology at George Washington University. The last decade of his life was spent as head of the department of psychology in the University of California at Los Angeles and in clinical practice. All in all, his career was one of notable service and achievement. He was for many years the editor first of the *Psychological Bulletin* and later of the *Psychological Monographs*. His fellow psychologists honored him in 1920 with the office of President of their Association. It is fortunate that we have an autobiography from his hands.[3]

Columbia University — R. S. WOODWORTH

Otto Lipmann: 1880-1933

Otto Lipmann's birthplace was Breslau in Silesia. He was led by Ebbinghaus and myself into psychology, to which he devoted a quiet student's life until the day of his death. Lipmann never held a university position. When at last in February 1933 the University of Berlin offered him an appointment, it was too late, for the political situation prevented the beginning of his teaching activity. It also put an end to his editorial activity in which he had collaborated with me for twenty-seven years. A few weeks before the appearance of the last *Heft* of the *Zeitschrift für angewandte Psychologie* that bore our names on the title page, he succumbed to a heart attack.[1]

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[3] *A History of Psychology in Autobiography,* II, 1932, 89-113.

Lipmann's entrance into psychology occurred at the time when our science began to exhibit its great significance for application to different spheres of life. He seized upon applied psychology at its beginning, and his life work contributed to the realization of what was then only a program for the future. The term *applied psychology* appears in the titles of both of the enterprises that we began in 1906-07: the *Zeitschrift für angewandte Psychologie* and the Institut für angewandte Psychologie und psychologische Sammelforschung in Berlin (later in Neu-Babelsberg).

The Institute, a private undertaking of Lipmann's, was not a laboratory in the sense in which the term was then used: it made compilations, transmitted scientific information, established groups of workers for particular problems, and promoted methods for the tests and for statistics. Lipmann's principal collaborators at this Institute beside myself were Baade, Bobertag, Bogen, Kern, and Plaut. Of the great variety of investigations which issued from the Institute we need mention only the numerous *Aussage* experiments, the researches of Kern on arithmetical skill, and the work on practical intelligence directed jointly by Lipmann and Bogen. This latter work is recorded in the well known book, *Naive Physik.* The difficulty of the economic situation, however, forced Lipmann several years ago to give up the Institute, which had no support at all from public funds.

Otto Lipmann became widely known in scientific circles through his activity as the joint founder and the joint editor of the *Zeitschrift für angewandte Psychologie.* From the obituary which I have devoted to my dead friend in the last number of the *Zeitschrift,* I quote the following passage: "The literary judgment, habitually displayed by him and constantly developed by him through the years, has in a high degree contributed to the result that applied psychology has become articulate as an independent scientific field in the total movement of the science of our time. He also participated editorially in the publication of the other sections and the accessory publications of the *Zeitschrift.* Thus the forty-five volumes, the sixty-eight *Beihefte* and the forty-seven *Schriften* form a permanent monument to the indefatigable and prolific efficiency of Otto Lipmann."

Exceptionally broad and many-sided is Lipmann's series of scientific publications. The *Psychological Register* and the complete bibliography which Paul Plaut contributed to the commemorative number on his fiftieth birthday provide a picture of the wealth of achievement.[2] He wrote outlines of psychology for teachers and for lawyers, and he published for physicians a collection of psychodiagnostic methods. He enriched theoretical psychology with interesting hypotheses and ideas, of which we can mention here only the concept of *Periphertrieb* introduced by him. His predilection for questions of method led him in the earlier days to the working out of new experimental procedures concerned with the investigation of memory, *Aussage,* intelligence, types of perception, and so forth. In his later days it was especially the statistical methods that he zealously developed and improved. His book, *Abzählende Methoden*, is much used; and his last contribution to the *Zeitschrift für angewandte Psychologie* bore upon a statistical problem of osculation.

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[1] This note has been translated, in the process of editing, from Professor Stern's German. Otto Lipmann was born on March 6, 1880. He died on October 7, 1933.

[2] P. Plaut, Die Arbeiten von Otto Lipmann, *Zsch. f. angew. Psychol.,* 36, 1930, 201-208.

As a technical advisor Otto Lipmann was much in demand, since his readiness to help and his absolute objectivity won him the confidence of all who applied to him. Often, by an expert psychological opinion upon the depositions of adults and youths before a court, he was able to bring about a correct judgment so that an acquittal could follow. As a leader in the Berlin *Lehrerverein* he gave support for many years to the pedagogical-psychological group of workers.

In the last decade of his life Lipmann was drawn especially into economic inquiries, and in this field he gained a recognition for the value of psychological methods and points of view. He was scientific secretary of a commission of the Reichstag, which carried on an investigation of the problem of working hours, and the voluminous report of this inquiry is essentially his work. In the same way he coöperated constantly with the governmental board for economy. From these activities there developed in Lipmann the conviction that *Arbeitswissenschaft* should be established as a new independent division of science. His *Lehrbuch der Arbeitswissenschaft* of 1931 is a first and noteworthy attempt at a delimitation of this discipline.

Very close to Otto Lipmann's heart lay the international quality of scientific relationships, and his friendly contact with foreign scholars were many. He attended almost all the International Congresses of Psychology, and his American professional colleagues will still recall with pleasure his presence at the Congress in New Haven and the charm and graciousness of his manner.

It remains to be said that both external considerations and personal inclination kept Lipmann from close contact with the outside world and directed him to work in the quiet of his study, a fact which explains his peculiar mode of research. He formed his psychological insights, not so much from the direct study of human beings, as from the factual material that is furnished by experimental findings, statistical tables, school performance, industrial performance, economic data, and so forth. To arrange this unorganized material, to classify it and make it intelligible, to reveal the hidden facts, to transform it into psychological knowledge, that has been the principal method and the final mastercraft of Otto Lipmann. In this his own peculiar way he has made fundamental contributions to the progress of our science which will preserve his name in honored memory.

Late of Hamburg W. Stern

Eleanor Acheson McCulloch Gamble: 1868-1933

Eleanor Acheson McCulloch Gamble, Professor of Psychology and Director of the Psychological Laboratory at Wellesley College, died on August 30, 1933. Miss Gamble was born in Cincinnati, Ohio, March 2, 1868. She received her bachelor's degree from Wellesley in 1889 and her doctorate from Cornell University in 1898. In 1906-07 she studied and did research work with G. E. Müller at Göttingen.

Miss Gamble began her teaching at Western College for Women, Oxford, Ohio, in the field of Greek and philosophy and later was appointed to the staff of the Normal School at Plattsburg, N.Y. In 1898 she was appointed instructor in psychology at Wellesley, in 1903 associate professor of psychology, in 1908 director of the psychological laboratory, and in 1910 professor of psychology. In 1930, at he death of Miss Calkins, she became head of the Department of Philosophy and Psychology. Miss Gamble was a member of the American Psychological Asociation, the American Philosophical Association, the Ninth International Congress of Psychology, Sigma Xi and Phi Beta Kappa. She was elected president of the Wellesley chapter of Phi Beta Kappa in the academic year of 1932-33.

While her career in psychology has left its impress largely on students in her elementary classes in psychology and while she was vitally and personally interested n them, both outside and in the classroom as is evidenced by the numerous inluential collegiate committees on which she served, she has to her credit almost a score of articles in the professional psychological journals. Several of these were written in conjunction with Miss Calkins or with others. Five of the papers are summaries of the experimental and theoretical literature on taste and smell, a field n which she was a competent critic, but the majority of them are contributions of an experimental nature in the field of memory.

Miss Gamble also edited two volumes of the *Wellesley College Studies in Psychology* (1909, 1916) and was engaged with the editorial work on the third volume of studies at the time of her death. She was responsible for the direction of more than a dozen master's theses, the highest degree given in course at Wellesley. Most of these studies are experimental approaches to problems of memory, but there are a number on tactual and olfactory discrimination and at least two attack the problem of *Gestalt*. There are also almost fifty minor studies by undergraduates who worked in the laboratory under her direction. Here the fields of research are more varied and include problems in word-reaction, mental tests, attention, suggestion, motor dexterity and consciousness of self, in addition to a large preponderance of problems in the field of memory and association.

In August, 1933, her *Outline Studies in the Essentials of Psychology* were published. These consist of some thirty topics arranged in the order of her lecture program. The book is an attempt to integrate the existential psychology of Titchener with the self-psychology of Calkins. In the foreword she has paid her respects o the systematic presentations of McDougall and of Smith and Guthrie. Sprinklings of behaviorism and psychoanalysis are also admitted and the influence of G. E. Müller, William Healy, and Theodore Reik are acknowledged.

During last summer Miss Gamble finished her collaborative work on Warren's *Dictionary of Psychology and Cognate Sciences*. She coöperated in the subject-matter ndex covering volume 6 of this JOURNAL for the index of the first thirty volumes which appeared in 1926. Her experimental investigation of three variables in memorizing, which also summarized her own unique techniques of learning, appeared as a contribution to the *Washburn Commemorative Volume* of this JOURNAL n 1927.

Miss Gamble's publications and researches show that her main field of endeavor was the field of memory and association and that her methodology was the methodology of Müller and Pilzecker. But she was also keenly aware of problems in the olfactory sense-modality as is indicated by her competent critique of the Henning prism and by other writings. Sometimes these two major fields were combined, as n her elaborate monograph on memory which formed the first of the *Wellesley Studies*.

Olfactory, visual and verbal materials were used as the groundwork of the memorial series.

Although physically handicapped from birth by amblyopia in her left eye, and, since her first European trip, by a continually developing glaucoma in her right eye, Miss Gamble applied herself unfailingly to maintaining the experimental attitude at Wellesley. At the same time she was a dearly beloved and effective teacher. This was acknowledged by her election as honorary member of the class of 1926. During her last year of academic service she fulfilled a long-growing ambition of having the elementary course in psychology and philosophy put entirely on an elective basis. Due to her unique ability and popularity the enrollment, even in these days of economic pressure, did not decrease when this change was made. Her natural and disarming wittiness was an integral part of her philosophy of life, a good example of which occurs in the foreword to her published outlines. Systematically she was well-informed by constant contact with the literature, usually through the more efficient eyes of faithful readers. She also made a valiant and fairly plausible attempt to bridge the gap between the Titchenerian and Calkinsian points of view on the doctrine of self. It should be noted, too, that she contributed largely to the questionary form on imagery in Titchener's laboratory manual.

Miss Gamble constantly used her prominent synaesthesias in learning, especially toward the end of her life when in spite of several surgical operations her vision became increasingly poor. Her number-forms were also quite prominent. Since the early years when intelligence tests were new, she took an active interest in them and personally computed many of the correlations for use in the administrative offices. Her final examinations were remarkable instances of her ingenuity. They nearly always included some short story or fable upon which the formal questions were based. Her interest in philosophy and theological history continued to the end of her life but, of course, occupied merely the fringe of her immediate attention. Nevertheless, her course in logic also proved to be very popular with the students. A further indication of her open-mindedness in regard to new movements appears in the fact that she went to Vienna in 1923-24 in order to study psychoanalysis at its source.

Her death has fallen most heavily on Wellesley College but psychology also suffers. Among the immediate losses is a contemplated third volume of the *Wellesley Studies* which was to contain Miss Gamble's final synthesis of the studies on chance word-reactions and memory.

University of Iowa CHRISTIAN A. RUCKMICK

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Seven Psychologies. By EDNA HEIDBREDER. New York, Century Co., 1933. Pp. ix, 450.

Psychology seems at last, as this author notes, to be coming of age. Instead of systems and the controversies about them, we are beginning to get the descriptions and the histories of them. An insight into one's own faults and virtues is the mark of a seasoned maturity, and this reviewer ventures the opinion that, if most psychologists could read Professor Heidbreder's book and make her wisdom their own, psychology would already have passed beyond scientific adolescence. This is no ordinary book. The depth of insight, the accuracy of information and the facility of expression set it off from most of the psychological composition that we are all obliged to read. There is in the book one joke, tinged with irony: the title page calls the volume a "Student's Edition," a statement which can be literally true only if the word *student* means *scholar.*

Since there are systems and systems, Dr. Heidbreder set out to describe seven that are of especial importance to American psychologists. She begins with an excellent historical introduction dealing most competently with prescientific psychology and the beginnings of scientific psychology. These chapters are not a mere rehash of recent books, but show the author's own integration of the subject-matter and make a contribution to perspective in the history of modern psychology. Dr. Heidbreder knows her history of philosophy well enough to bring it in where it helps and to leave it out where it hinders. The seven psychologies are: (1) structuralism, meaning Titchener; (2) William James; (3) functionalism, meaning Dewey, Angell, and Carr (Carr at last gets his due); (4) behaviorism, meaning just Watson; (5) dynamic psychology, meaning Woodworth (who thus also comes belatedly into his systematic own); (6) Gestalt psychology, of the original German triumvirate; and (7) psychoanalysis, meaning Freud.

Except for behaviorism and psychoanalysis, it is approximately correct to say that the author disposes of these systems by getting enthusiastically expository about each in turn. She enters the stage from the wings of history as champion of Titchenerism, and then, presently, the scene shifts, and she is a keen and appreciative Jamesian; and thus on through other metamorphoses. However, she develops no schizophrenia in this process for she emerges at the end with an integrated totality. She has shown the reader just what each system is, just what good it did to psychology, just how it is related to previous thought, and why it is new and an advance upon previous thought. The reader can take his choice between the one and the many psychologies: he can see in psychology continuity and slow elaboration if he wishes, or he can see the stepped changes that new systems introduce.

The author makes a much stronger case for the unique and positive contribution of each system than any partisan could ever have made.

Watsonian behaviorism comes in for more definite strictures than any of the other systems. Dr. Heidbreder plainly thinks that Watson's escape from reality was much more thorough than Titchener's, because Titchener could see behavior when he would not entertain it, whereas Watson completely turned his back upon consciousness. Psychoanalysis, we gain the impression, is pretty good for what it pretends to be. It has merely the difficulty of being unscientific, of being an hypothesis formulated in such a way as to be forever unverifiable by scientific means, so that its value has to be judged by its appeal to belief and intuition. That is a devastating criticism to those who find it a devastating criticism, but hardly to the psychoanalysts.

A system comes into its own when persons outside its confines recognize its influence and begin to write about it. Self-baptism lacks virtue. Thus Dr. Heidbreder's greatest service is not her reëxposition of the *isms* of Titchener, James, and Gestalt, but her development of functionalism up to the present through Carr, and her clear formulation of the dynamic psychology of Woodworth. Each of these systems has been very important in psychology, neither has tried to 'ismise' itself, and it is appropriate and fortunate that recognition of their places in psychology should now come effectively from outside. Besides, it is more satisfactory so. Dr. Heidbreder can probably do better in respect of both generosity and clarity by Woodworth than he could do by himself. There is a great deal to be said for having the systematists give up trying to discover themselves and having the discoveries made by properly qualified experts.

The first chapter of the book, echoed by the last, deals with the function and operation of schools and of systematic controversy in scientific psychology. It answers perfectly the question that has long troubled the present reviewer, the question as to how scientific open-mindedness can be integrated with the amount of prejudice that is necessary to secure the drive for effective research. Does the truth always transcend the individual who has drive enough to be productive? Dr. Heidbreder says 'Yes' and 'No.' Science is in the hands of scientists and scientists are persons, whose wisdom is the unpredictable mixture of truth and error (influenced by modesty and arrogance) which all human wisdom is.

It seems probable that almost every candidate for the Ph.D. in psychology in America is going to read this book. They all have to know something about psychological systems, and for range, clarity, and lack of bias this book is without peer among the very few that exist. It thus becomes possible that the book may ultimately be found to have played an important rôle in the change of the psychological temper in America from youth to maturity. By extolling the value of controversy and the need for scholastic differences, Dr. Heidbreder goes far toward making both of them seem unimportant, for she lifts the controversalist who will attend to her up to a higher plane where he sees himself as a pawn in the great game of the evolution of a science. Your reviewer would not be alone in welcoming that state of affairs in which all the younger psychologists, besides being filled with the lust of battle, would also be prepared once in a while to ascend to the clouds and there to contemplate the meaning of the war.

It is a piece of good fortune for all these graduate students and for psychologists in general that Dr. Heidbreder can write English so happily. One might think hat academic psychologists would always write at least felicitously without the wkwardnesses that make meaning obscure or that distract attention from the hought. What else is an academic? However, the blunt fact is that Dr. Heidreder stands out from the crowd. She is positively interesting. There is not an wkward sentence nor a stupid paragraph in the book. Her figures are vivid; er transitions are smooth; her choice of language is anything but trite. She has ot the courage of William James to arrest attention by the judicious colloquialism, ut give her time.

E. G. B.

The Laws of Human Nature: A General View of Gestalt Psychology. By RAYMOND HOLDER WHEELER. New York, D. Appleton & Co., 1932. Pp. xiv, 235.

The opening paragraph of the preface to this volume informs us that "the elativity movement in physics, the organismic movement in biology, and the *Gestalt* movement in psychology represent advances in a new conception of nature and a vast amount of startling experimental research which the new movement has promoted." On the next page, we are told, presumably of this volume, that "a new attempt has been made to direct the educated reader to the field of psychology as it appears when reinterpreted in terms of the new tool," the new ool being, presumably, the *Gestalt* theory. The reader might, therefore, expect o be told something about the "startling experimental research" and to have explained to him what *Gestalt* psychology is, as distinguished from any other sort of psychology, or at least what new advances it has effectuated. The eventual disappointment of the reader may of course be due to the fact that he is not 'educated.'

The first two chapters of the book are devoted to a historical account of the development of psychology from "primitive man" (who was a monist), through "the Greeks" (who seem to have been dualists), Aristotle (who failed completely o resolve dualism), William James (bracketed with Aristotle), "the Scholastics" (who "attempted to stem the tide of dualism by a return to the philosophy of Aristotle"), Descartes (who "put the finishing touches upon dualism"), occasionalism (apparently attributed to Descartes), Spinoza (whose psychophysical parallelism "is nothing but Descartes' out and out dualism over again"), "idealists" (not specified), "attention psychology" (whose exponents are apparently Descartes, Leibnitz, Herbart, and Wundt), to the "dead ends" of introspectionism (Titchener and Külpe), behaviorism (Watson), self psychology (Ward and Calkins), hedonic and hormic psychology (McDougall alone specified), and psychoanalytic theory.

This breath-taking journey through uncounted centuries, routed in less than seventy pages, is punctuated with conventional concepts of mechanism, vitalism, ntrospection, mind, matter and other familiars; and, for the most part, the bruised straw of these figures is left scattered along the path. Insight and instinct, however, are left still nicely tied together, and approved, which certainly is a tribute to some, at least, of "the Greeks."

This part of the volume is well worth reading, to impress the reader (educated or not), with the appalling need for more adequate cultivation of the rich field of the history of psychology; but it is to be hoped that it will not fall into the hands of students who have little orientation in this field. Space does not permit the reviewer to defend "the Greeks" against the manhandling to which they are subjected, but Aristotle perhaps gets his revenge through the author's proclaiming as important truths (perhaps as interpretations in terms of the 'new tool') some of Aristotle's fundamental views. Early in the third chapter even, the author announces that "content is not the determiner of principle. Principle is the determiner of content."

In the remainder of the volume, the reader searches in vain for the "new tool" and its alleged revolutionary results. All the usual topics of a course in psychology are introduced in a somewhat helter-skelter fashion; many sweeping generalizations are formulated, some of them being commonplaces, others accepted maxims, and some are not entirely clear in their bearing. Intermixed with these are detailed statements of empirical fact, some of which are open to question; but the bearing of the details on the generalizations is seldom made out.

Among the statements which might be taken as bearing on the *Gestalt* theory we find some which boil down to the following. Forces do not produce results supernaturally, but in accordance with the environments in which they act. When objects which are in relations to other objects are treated in isolation, they are treated abstractly. Parts of wholes, in the wholes of which they are parts, are different from what they would be if not parts of the wholes. The cause of a phenomenon is not necessarily its immediate antecedent, nor even one abstractly isolated antecedent. Emphasis on accepted principles of this sort may be important for the instruction of raw students, but the psychologists can hardly be expected to become excited about them. Many statements seem, however to be mere definitional formulae, and for these the author does not stop to offer justification. For example: "From a functional point of view, energy, anywhere, may be construed as mind . . . mind as thus defined is not an idea until structured by thought." "Mind is determinism . . . is causation." Passing over the question how energy can be an *ism*, we wonder why the conventional usages of 'mind,' 'energy,' and 'causation' are not satisfactory.

Passing on, we find emphasis put upon the continuity of principles of explanation in the inorganic and organic worlds. (Page the Greeks! Several of them!) We find that the total response of the animal is the important thing. We find the 'experience theory' of the development of behavior denounced more than once, although apparently this theory is identified with the simple scheme of bonds. We find the author deeply puzzled over the development of visual coördination, singling this out for some reason from the vast magazine of psychological problems. We find the doctrine of the mental life condemned as a "hodge podge of inarticulate, unrelated elements." The integrative aspect of perception seems to be approved, and perception is defined as equivalent to cognition, with the remark that the outstanding characteristic of perception is meaning; although later, feeling is apparently classified as a meaning. Such formulations as "Parts derive their properties from wholes," "the whole conditions the activity of its parts," "parts emerge from wholes through a process of individuation," and "wholes evolve as wholes," may be abstract formulations of the integrative nature of perceptual process, or they may be intended by the author as more sweeping, and unsupported generalizations.

The exposition of the "law of configuration" in the fifth chapter, may be intended as an explanation, or basis for explanation, for the 'new tool.' If so, it is far from enlightening. The "simple experiment of Wertheimer" in 1912, demonstrating apparent movement when there is no real movement somehow resulted in a "revolutionizing of modern psychology;" but why this phenomenon, familiar in our laboratories before 1912, failed to revolutionize anything until Wertheimer observed it, is not explained; nor are we told why it is more important than the various other discrepancies between 'appearance' and 'reality' which annoyed the epistemologists from Parmenides to Malebranche.

The chapter on the laws of learning presents a puzzle. First, instinct and the instincts are together drummed out of camp. "There are no instincts;" and, by implication at least, no instinct. Apparently, the author thinks the nervous system is incompetent to provide for variations in an instinctive response process. Yet we are shortly told that by a process of evolution there is produced in the animal "a nervous system dynamically so adjusted" that it conditions the complex and variable processes which we have been told cannot be instinctive. The discussion of learning which follows proclaims indiscriminately the failure and fallacy of all of the conventional conceptions regarding the learning process, and the "bankruptcy of associationism;" but no constructive substitutes are discernible. In one demonstration of the elasticity of the English language insight is set "*versus* determinism;" but insight seems to be merely a name for the fact that the determiners of response are patterns and not isolated stimuli, that in these patterns the relational nexus is highly important, and that, moreover, animals do learn, somehow or other.

It would, perhaps, be exaggeration to say that the volume is "a hodge podge of inarticulate, unrelated elements," yet it does create the impression of a note book in which vague ideas are jotted down from time to time for future elaboration or deletion, and eventually arranged in a formal order with a minimum of editing. What the real purpose of the book may be, except to serve as an outline of a course in general psychology in a metaphysical setting, it is impossible to decide. If the purpose expressed in the Preface is to be taken seriously, the reader has only three alternative conclusions from which to choose. Either the much heralded *Gestalt* psychology is a rather superficial bottling of an assortment of old wines in a new container, or else this volume is not in any wise an exposition of Gestalt; or else, finally, an interpretation in terms of a 'tool' can be grasped only by a peculiarly 'educated' reader.

Johns Hopkins University KNIGHT DUNLAP

The Use of the Self: Its Conscious Direction in Relation to Diagnosis, Functioning, and the Control of Reaction. By F. MATTHIAS ALEXANDER. Introduction by John Dewey. New York, E. P. Dutton & Co., 1932. Pp. xix, 143.

Those readers who know the author's earlier books—*Man's Supreme Inheritance* (1918), and *Constructive Conscious Control of the Individual* (1923)—are doubtless somewhat acquainted with the ideas and techniques outlined in this volume, the chief purpose of which is to present the history and development of that technique. Appeals have come to the author not only to make his methods available but also to provide for the training of other persons to extend and continue his work. He therefore announces in the present book the first payments to a trust fund for the endowment of an institution to train teachers for this work.

Mr. Alexander's central view is this: mal-functioning is not a matter involving only a single organ or individual parts of the organism; it is rather a bad use or direction of the entire self. Education in self-management is preferred as an idea to the popular notion of 'cures.' Conscious and reasoned control must replace 'instinctive' control and become fixed by actual use. The author was early interested in elocution and was successful enough to think of Shakespearean reciting as a career, but after some years his speaking was obstructed by throat trouble and hoarseness. No relief was permanently effected by the treatment given him by physicians. This led to various forms of self experimentation before mirrors to detect and avoid positions that were cramping to the larynx. Different hypotheses were set up and tested in a true scientific fashion. It was found that if the head were held upward and to the front the interference was avoided, but somehow the beneficial results in actual life-conditions when speaking was necessary were not realized—the trouble persisted. This resulted finally in the important discovery that the 'feeling' of the position taken was actually unreliable: when the author felt that he was successfully putting his head forward and upward (as if to enlongate himself) to relieve the pressure on the larynx, the mirror revealed to him that at the critical moment of attempting to speak he really fell into such obstructive bodily attitudes as actually to lower and bring backward the head. "By careful experimentation I discovered that I gave my directions for the new use in their sequence right up to the point when I tried to gain my end and speak, but that, at the critical moment when persistence in giving the new directions would have brought success, I reverted instead to the misdirection associated with my old wrong habitual use" (p. 30). This error, at first unavoidable, was attributed by the author to the lack initially of the sensory (kinaesthetic) data to indicate how to proceed in the performance of the right act, so that in the great hurry to get the act done he slipped into the old habitual attitude and 'feeling.' His solution was to learn to inhibit the act at this stage and to concern himself not with the act immediately to be done but with the means by which it could be directly carried out. He thus contrasts the 'end-gaining' with the 'means-whereby' motive or impulse. His system of training is to acquaint the student with his own wrong movements or adjustments and to help him get the "actual sensory experience" of the desired correct responses (but inhibit the critical act until he can maintain the set for these responses) and then to carry them out correctly. This is repeated until this new 'use of himself' gets to be habitual and is sensed as natural. It is, unfortunately, not made clear just how this new sensory experience is given the student, for we are told only "that the pupil receives from the hands of the teacher the actual sensory experience of the new use which he is consciously directing" (p. 104). Does he find valuable with persons this method of "putting one through the act" by physical means, which has proved of little value in training an animal, as American psychologists have found from experiments? Does he only guide the act gently when it is motivated and attempted by the student? Just what does he do? Those readers who are acquainted with such trial-and-error learning as star tracing, and ball tossing, will see much similarity between the author's method of bringing about control of new acts and those used by the learner, who has also to find out how it 'feels' to perform the new and correct act. But a trainer can do little for the subject directly in such experiments, except, of course, to tell him what to work for and use in the learning.

The author illustrates his methods and results fully in the training of a golfer, who could not keep his eyes on the ball just at the time of making the stroke, and in the education of a stutterer, who when excited fell into obstructive responses. The interest of physicians who have themselves had the training, or have had patients take it when their own methods failed to bring relief, is indicated in open letters in the appendix. The author claims wide fields of application for the method and indicates that it is not contradictory, but supplemental, to sound medical treatment; indeed, he maintains that diagnoses of many types of ills are not satisfactory if they do not include his method of functional diagnosis.

There are a few unfortunate uses of terms. 'Instinctive direction' in the wrong use of self (*i.e.* in wrong control) seems in several places to be used interchangeably with 'wrong habitual use.' There is, of course, a legitimate difference in these terms, and the latter indicates that something had interfered with the natural use and has developed a wrong use. The author might profit from an acquaintance with Jacobson's "progressive relaxation" technique and from some of Cannon's work, to neither of which there is any reference. His general point of view is unquestionably supported by experimental psychology of movement and of learning.

J. P.

The Mechanism of Nervous Action. By E. D. ADRIAN. Philadelphia, University of Pennsylvania Press, 1932. Pp. x, 103.

This book contains the second series of lectures of the Eldridge Reeves Johnson Foundation for Medical Physics. It is a résumé of the results of some of the experiments conducted by Adrian and his students at Cambridge, with a discussion of pertinent experiments of other investigators. No attempt is made to include all the literature of the topics treated. The first chapter gives a brief description of the development of electrophysiology and the changes in different types of nerves recorded by modern means of amplification. The changes are alike whether recorded from single motor or sensory nerve fibers or from total nerves and consist of a series of brief potential changes or impulses. In a given fiber these impulses are of uniform size but vary in frequency, that is, gradation of activity is accomplished in terms of the frequency and not the intensity of the impulse. The amplitude of the potential changes varies in nerves of different size. Adrian accepts the membrane hypothesis as the best explanation of the conduction of these potential changes down the nerve fiber. The next chapter is a discussion of the action of those sense organs which are stimulated mechanically, *e.g.* the muscle spindles, the Pacinian corpuscles and the hair endings. These endings differ in their rate of adaptation to a continuous stimulus showing differences in the time course of their discharge. Some react with a persistent discharge as long as the deformation lasts, while others respond only during the actual movement of deformation. In all these nerves "the frequency of the discharge depends on the rate of increase, the final intensity and the duration of the mechanical change." The rhythmical nature of the discharge to a continuous stimulus is explained in terms of a persistent depolarization of variable extent on the surface of the sense organ. The difference between pressure and touch sensations is explained in terms of the rate of adaptation of the sense organ, pressure endings adapting slowly and touch endings rapidly. The adaptation of the auditory nerve is rapid, as is touch, and in either sense the intensity of the stimulus is judged on the basis of the number of impulses making up each volley. He discusses the work of Wever and Bray and suggests that "the high frequencies found in the composite potential waves are most probably due to many fibers, each responding at a submultiple of the stimulus frequency and arranged so that each wave of the stimulus finds some fibers ready to respond to it." The pitch of a note is then judged from the frequency of the volleys without regard to the frequency in each nerve fiber.

In his discussion of pain (Chapter III), Adrian has revised some of his earlier views to conform to more recent findings. The ending responding to light touch does not give rise to pain when more intensely stimulated as was formerly held. The unmedullated fibers noted by Ransom and the small, slow conducting group C fibers of Erlanger and Gasser are thought to be concerned with pain although this sensation is not to be restricted to these fibers or to any particular group of pain fibers. As the impulses in the fibers giving rise to pain do not vary in size the intensity of the sensation is explained in terms of the summation of the changes resulting from the several impulses. The ultimate quality of sensation cannot be wholly attributed to specific fiber reactions. Factors of chronaxy, changes in the discharge frequency, and differences in fiber connections are other variables to be considered.

Chapter IV considers impulses in motor nerve-fibers. The translation from nervous change into movement depends on a fusion of the effects of repeated impulses. Gradation in contraction results from changes in the frequency of the impulses in individual nerve fibers and in the number of fibers which are stimulated to activity. Adrian suggests that much confusion would be avoided if the "all or none principle," through rewording, were limited to an "all-or-nothing relation between the stimulus and the propagated disturbance" and not used to cover all the local effects produced in a nerve by stimulation. Persistent discharge in sympathetic nerves do not show the regularity found in sensory and motor nerves, probably because there is "no opportunity for a steady excitation."

The last chapter deals with the activity of nerve cells and reviews some of the more recent experiments at Cambridge on the potential changes in the brain stem. Although admitting that the reflex arc has been a valuable conception in nerve physiology, Adrian opposes the extensive use that has been made of it and cites a number of experiments which tend to show the presence in the brain stem of automatic discharges not dependent upon afferent control. Rhythmic slow breathing after vagotomy is explained as due to an automatic beat of the respiratory center. The hypothesis is advanced that slow potential changes taking place in the nerve cells and dendrites give rise to these rhythmic discharges. This depolarization of the cell is not the explosive type found in the nerve fiber but takes place at a slower rate, persists over longer periods and is capable of gradation. This slower surface breakdown gives rise to a discharge of increasing and decreasing frequency. The periodic discharges in injured nerve fibers are ascribed to a similar mechanism, the synchronization of these discharges resulting from the electric fields set up by the impulses. The author presents these views only as tentative hypothesis, frankly admitting that definite conclusions about functional mechanisms of the central nervous system are still premature.

The book is very readable. It is adaptable for courses in physiological psychology and should be readily understood by students having had the usual background of experimental work. A bibliography of 62 references is appended.

University of California. C. W. Brown

Present Day Psychology: An. Objective Study in Educational Psychology. By ALENE RALSTON and CATHERINE J. GAGE. Philadelphia, J. B. Lippincott Co., 1931. Pp. lxiv, 404.

This book attempts to meet the three-fold purpose of putting "the fundamental facts of educational psychology in a language simple enough for the uninitiated," "to answer the many demands from students for something easy and good, and at the same time authentic, for their parents and friends who are not familiar with psychology;" and "to help the teacher in elementary school service who entered the profession at a time when the facts of modern educational psychology were not available" (p. vii). The authors attempt to maintain an objective point of view and make no claim to originality.

Psychology is defined as "an attempt to understand life as it is lived;" but a departure from objective methods comes early when the reader is told that guidance leads to controlled behavior, and that "control presupposes an urge, *a purpose, a reason*" [italics reviewer's]. As to the fields of psychology, adolescence is characterized chiefly according to the old view that "natural tendencies" ripen and create "storm and stress" problems. Such types of individuals as visiles, audiles, motiles are mentioned conspicuously in this *Present Day Psychology* as individual psychology problems; and social psychology is made to deal "with the reactions of crowds and mobs as contrasted with those of individuals" (p. 6).

The general outline of the book follows closely Thorndike's three volumes, *Educational Psychology*, with fillings-in from other authors and from the authors' own interpretations. "Satisfaction and annoyance" are not regarded merely as aspects of response, but as *verae causae* actually determining the sequence of responses (p. 10). The recent controversy about instincts, which for a time took so much space in the journals, has not in the least affected the authors' fidelity to Thorndike's view published in his *Original Nature of Man*, approximately twenty years ago. The excellent work of Cannon on the emotions, extending through many years and widely known, is not even mentioned in the text. This is true also of Lashley's work on learning and cerebral functions. Certainly the importance of these omissions and of others relating to recent contributions on the selective factors in learning, including Thorndike's repudiation of his earlier position on frequency and recency as selective factors, serve to indicate that the book is hardly a "present day" psychology, even in its own field. Similar omissions of recent work in association and memory are noticeable. But psychoanalysis is not overlooked; neither is it handled uncritically. While it is pointed out that psychiatrists (*sic!*) and psychologists "were slow to take up psychoanalysis," G. Stanley Hall and R. S. Woodworth are named as psychologists "who became interested in psychoanalysis" (p. 240). This subject and behaviorism, dynamic psychology, purposivism, and Gestalt psychology, all receive some consideration, but, as we have seen, much excellent experimental work is omitted. Functionalism and structuralism—treated among other current trends in psychology—are defined and discussed briefly, and we are informed that William James and Titchener "were advocates of" these "opposed views" (p. 246). Thus the authors frequently reveal a naïveté and an unfortunate lack of contact with developments in psychology. There are tendencies, also, to reflect the popular fear of mechanism despite an ostensible acceptance of the methods of objective science. 'Freedom' seems to be thought of in the old absolutistic manner rather than as a relative independence of control by immediate circumstances.

There is serious confusion in the treatment of color mixture of pigments (by the subtraction method) and mixture with color disks on a color wheel (mixture by addition and in part also by subtraction). Also Hering's theory does not assume, as is stated, "three pairs of colors." To say that "in imagery no stimulus is present" is grossly erroneous. Moreover, the definition of imageless thought is inaccurate and misleading, and it is certainly a reflection on the careful experimental studies bearing on this problem to say that "one may test himself by a little introspection" (p. 252). Moreover, the old notion of the "composite image" was certainly quite different from that of imageless thought. The statement that the after-image "is due to retinal fatigue" is subject to challenge, as is also the one (p. 254) that "no mental state is possible without the first pure sensation."

Several inaccuracies occur in the treatment of the nervous system. Here, under the caption. "The Autonomic Nervous System" is the following interesting information: "The cranial or upper part allows eating; the sympathetic or middle part permits fear or anger; the sacral or lower part reënforces love. There is an antagonism of the first for the second and third parts. The cranial system favors digestion. This is not true of the other two. There, emotional excitement is paramount. Hence, the wisdom of no food, or little, when one is grieved or worried or in love. . . . The sight of, hearing or thinking about, food, anger, or love will arouse one of the three emotions" (65 f.). A student in love, for instance, may be somewhat puzzled as to what to do at meal time if he has absorbed this half-knowledge!

Association fibers are misconceived. Memory is curiously defined as "*the awareness in consciousness* of an event or fact that has been experienced before" [italics reviewer's]. Old views of attentions are given and we read that "non-voluntary attention is the kind one gives because he wants to attend." Non-psychological, and formal treatments are also given to thinking, concept, reasoning, and will. Several statements are positively wrong; *e.g.* "The function of the will is to cause or to inhibit action" (p. 262).

The last third of the book deals in a practical way with tests and statistical terms and procedures. Concrete illustrations abound but do not obscure the principles involved. There is a wrong interpretation of correlation coefficients (p. 369), and unfortunate errors in the forms of the normal distribution curves shown (pp. 372 and 374).

The book seems to have grown out from needs felt in elementary teacher-training institutions, and for the purposes stated—except for being an *authentic* text—it has certain commendable features. Numerous references given in footnotes are reproduced in the nine-page classified Bibliography at the end of the book. Exercises and questions follow each chapter. It would be fortunate if the book could have such thorough revision, and be put so fully abreast with present day psychology, as to receive the recognition of psychologists. It is desirable that authoritative and excellent texts arise in the fields where they are needed.

J. P.

The Delinquent Child. By FREDERICK P. CABOT and others. The White House Conference on Child Health and Protection. Report of the Committee on the Socially Handicapped. New York, The Century Co., 1932. Pp. xx, 499.

This is without doubt the best general book on the delinquent child. The work includes seven chapters: "The Delinquent Child in Relation to Himself;" "The

Child in Relation to His Family;" "The Child and the School;" "The Child and the Church;" "The Child and Industry;" "The Child and Community Influences;" and "The Child, the State, and the Municipality." A "General Report" of 31 pages ably summarizes the viewpoint and recommendations of the Committee. An Appendix of 90 pages provides much factual material, some of it new, on which the authors base their beliefs. Extensive "Subject-Reading Lists" are also provided, as well as a generous bibliography of 384 titles. A ten-page index completes the book.

This White House volume bears much evidence of careful, conscientious, and competent effort on the part of the Committee. The Committee has expressed current beliefs so clearly and well that one wonders if this book will not, by the eminence of its authorship, serve to indoctrinate as well as to illuminate. Such an effect would be most unfortunate; for there is very much in this volume which demands deep critical thought.

We find in the book the usual emphasis on the individual delinquent *vs.* the delinquency; and the usual emphasis on the essential normality of the behavior-mechanisms that move the child to delinquency. We find also the usual arraignment of society's failure to satisfy the essential needs of its children. The Committee, however, fails to advance much beyond these clichés. To the writer, it seems that progress in the problem of delinquency is more likely to be made on a very different tack from that implied by the Committee. In the first place, we must cease paying our respects to "the naturalness and ubiquity of those processes which lead up to" delinquency. Delinquency is neither ubiquitous nor natural among the greater proportion of the population. It is well enough to adopt a sympathetic, humanitarian attitude toward the delinquent; it is quite another thing to let this attitude warp our insight into delinquency. The prime requirement, now, is to discover and stress the ugly *differences* (both qualitative and quantitative) between the delinquent and the non-delinquent child. We shall not make the progress that we should, until studies of delinquency are broadened to include (more definitely than they are included in the present volume) companion-studies of the non-delinquent.

In the second place, we think that if society is to be arraigned, the Committee should have done a more thorough and honest job of it. Possibly the White House affiliation embarrassed the Committee in this regard; possibly the personnel of the Committeee—which is lacking in a fair representation from sociology and anthropology—was inadequate for the performance of this unpleasant task. Whatever the cause, the fact remains that issues of the utmost importance receive little or no recognition in this volume. Given a prevailing individualistic and skeptical philosophy, operating in an increasingly urbanized civilization, with little or no eugenic restrictions, can the proportion of abnormal self-assertion (both in the direction of delinquency and neuroticism) ever be significantly reduced? Given a money-economy, the profit-system, and an extremely uneven distribution of wealth, advantages, and opportunities, shall we ever be significantly relieved of those who seize society by its forelock in an effort to get illegally what they cannot in fact get by any other means?

One further issue must be mentioned; this is the Committee's slighting of the influence of heredity as a cause of delinquency. No one nowadays would urge that crime is directly inherited as such. Is it not significant, however, that feeblemindedness—certainly largely hereditary—is a definite handicap, both to society and the child, in the problem of delinquency (p. 65)? Who can say to what extent other hereditary factors operate in any individual case? The Committee strenuously urges that we study the individual delinquent; but on closer inspection it seems that this study is to be restricted mainly to discovering the *environmental* causes of the delinquency. Lange's famous study of criminality in twins suggests that we might better study the factors of heredity. From the point of view of practical therapeutics, of course, environment is the proper center of attention. But a scientific analysis would certainly go beyond the merely practical and therapeutic phases of the problem. We think that the predominance of the clinical, therapeutic viewpoint has injured the scientific value of the present book. The predominance of the clinical viewpoint probably accounts, too, for the occasional crusading note in certain passages of the volume.

The book suffers from excessive generalization, and insufficient recognition of the need for more strictly quantitative expression. For example, the statement that "adult crime has its roots in juvenile delinquency" (p. 19) is probably truer of the crude crimes of violence than of bribery, embezzlement, forgery, dishonest bankruptcy, stock-swindling, and crooked business practice in general. A strict quantitative statement of the influence of early delinquency on later crime is not yet possible; but this very absence of quantitative data should have led to greater caution and qualification.

Of the individual chapters, "The Child and the School" seems the best written. "The Child and the Church" makes little contribution.

Despite our criticisms, we still consider, as stated at the very outset, that this is the best general book on the subject of child delinquency. The book is a landmark, and it should be owned by every single worker whose interests touch on the broad field of delinquency.

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History of Psychology in Autobiography. Vol. I. By JAMES MARK BALDWIN and others. Edited by Carl Murchison. Worcester, Mass., Clark University Press, 1930. Pp. xvii, 516.

We have here the first volume of a series designed to give the history of psychology in autobiography. The idea of such a series originated with Boring in 1927 while preparing an article on Titchener (cf. this JOURNAL, 38, 1929, 489-506). Realizing that many important facts concerning an individual's scientific development could be obtained only at first hand, he proposed that such information be placed on record in this JOURNAL in a series of autobiographical articles by the foremost psychologists of the world. Attractive and worthy as this suggestion appeared, it could not be adopted without radically altering the scope of the JOURNAL. The idea was, then, temporarily abandoned only to be presented later to Dr. Murchison of the Clark University Press, who, fortunately, found it both attractive and feasible; and we now have as a consequence the autobiographies of 15 eminent contributors to psychology.

The authors of these autobiographies, and of those subsequently to be published, were selected by an international editorial board. Eminence and seniority in psychology were not the sole bases of inclusion, as individuals in peripheral and neighboring fields have also been invited to contribute.

This volume contains the autobiographies of James Mark Baldwin, Mary Whiton Calkins, Edouard Claparède, Raymond Dodge, Pierre Janet, Joseph Jastrow, F. Kiesow, William McDougall, Carl Emil Seashore, C. Spearman, William Stern, Carl Stumpf, Howard C. Warren, Theodore Ziehen, and H. Zwaardemaker. The death of Calkins and Zwaardemaker, since the publication of the volume, lends point to the timeliness and need of the undertaking.

The autobiographies are very uneven in their value to psychology, and show the need of a firm editorial hand. Dictation on the part of the editor is of course out of the question with such a galaxy of luminaries, and requests for revision might have led to withdrawals, but the kind of article desired should have been stipulated in advance, and non-compliance should have met, despite the risk of withdrawal, with diplomatic requests of revision.

Some of the autobiographies defeat the purpose of the series, as they tell us little or nothing about the author's intellectual and scientific development. One is an out and out success story; one is a mere résumé and review of the author's books and papers; and at least two must be described as "reflections at sixty." Some, on the other hand, are revealing and fulfill the purpose of the series. McDougall's article, for example, contains the key to his psychology. He writes with a frankness and disinterestedness that few could bring themselves to emulate, and no one can read his self-analysis without obtaining a fuller understanding and appreciation of his psychology.

Pertinent biographical data, such as one would expect to find in an autobiography, are frequently omitted. For example: 9 of the 15 authors do not give place or date of birth; 4 do not give their full names, and 3 do not give even their first names. If this information be desired, it must be sought elsewhere!

Six of the articles are translations: Claparède, Janet, Kiesow, and Ziehen from original manuscripts, Stern and Stumpf from articles published in *Philosophie der Gegenwart in Selbstdarstellungen* (vol. 6, 1927, ed. Dr. Raymond Schmidt). The translations, with the exception of Janet's article, are good.

The volume has not been carefully proof-read. Numerous omissions and inversions of letters occur, and in one instance (p. 155, center) the linotype slug is inverted.

K. M. D.

Endocrine Medicine. By WILLIAM ENGELBACH. Springfield, Ill., and Baltimore, Md., Charles C. Thomas, 1932. 3 vols. Pp. 1800, 933 illustrations.

The matter of internal secretions is more and more claiming the attention of the medical and allied professions because of the profound influence which these secretions exert on growth and metabolism and on the specific capacities of other tissues of the body.

In the past decade a number of volumes have appeared on the subject but the march of knowledge in this new field is so rapid that a timely summary is needed at frequent intervals.

Endocrine Medicine by the late Dr. William Engelbach is probably the foremost compendium in this country on the subject of glands of internal secretion up to 1932. The three volumes are well printed and bound. They contain a wealth of well selected illustrations, tables, charts, and clinical data from reliable sources. There is an appreciative foreword by Dr. Lewellys F. Barker. The arrangement is systematic, the treatment is orderly and the text is commendable for its clarity and avoidance of obscurity. The volumes deal with the hypophysis, thyroid, parathyroid, gonads, thymus and suprarenal cortex, and the disorders resulting from their secretory disfunctions. They do not include the suprarenal medulla, the pancreatic islands, and liver: the triad of organs which deals primarily with the functions of carbohydrate metabolism.

Volume I, entitled "General Considerations," is tersely written and abundantly informative on the anatomy, physiology and nomenclature of the glands, and their better known secretions. It discusses the functional units, their interrelations and their effects on the non-endocrine systems. The causes of glandular disfunction and the most available diagnostic procedures for the recognition of such disturbances are fully presented. Emphasis is placed on a thorough medical survey in all cases. Numerous tables and charts of anthropometric and physiological measurements, of tests and other data are provided, by reference to which the presence of normal function or disfunction may be judged. In conclusion the author aims to assay the relation of endocrine disorders to public health. Here the student of abnormal psychology may derive some idea of the relation of endocrine disfunction to abnormal mental development. The author estimates that about 10% applying for medical opinion in whatever field are afflicted with some uncomplicated endocrine disorder.

Volume II deals with simple and combined disorders of the thyroid, hypophysis, gonads, and thymus as they affect children in infancy and in the juvenile period. It may be of special value to the child psychologist interested in the interpretation of certain rather common aberrant physical and mental types. The common birth injuries and mental deficiencies are not included in the treatise. Anthropometric and roentgenologic methods and physical comparisons are extensively used for differential diagnosis. In this volume many examples typical of endocrine disorders of childhood are illustrated. In the young the physical and constitutional characteristics are usually well defined as the unbalanced growth processes proceed to maturity. In the concluding chapter only a brief reference is made to the relation of such conditions to mental activity and intelligence.

Volume III deals in a similar way with adolescent and adult endocrinopathies. The treatise is largely clinical, based on reference to experimental data. It is well illustrated by typical cases.

It is a growing concept that the secretions of this group of glands are so essential for the internal chemistry and growth processes of the body that they ultimately shape the physical and functional constitution of the individual, and in the broad sense that of the race. Because of this relation there is much in these volumes that will be of interest to students of genetics, anthropology, physiology, and abnormal psychology, although these branches have been invoked only as handmaids to the main purposes of clinical endocrinology.

Cornell University Medical School JAMES W. PAPEZ

Sorcerers of Dobu: The Social Anthropology of the Dobu Islanders of the Western Pacific. By R. F. FORTUNE. Introduction by B. Malinowski. New York, E. P. Dutton & Co., Inc., 1932. Pp. xxviii, 306, 7 appen., 8 pl., 5 fig., drawing, map.

The literary as well as the scientific world is indebted to the author for the wealth of primitive legend, dance song, and "secret" ritualistic spells contained in *Sorcerers of Dobu.* In addition, through a series of concrete pictures the reader is enabled to view the primitive mind at work and to see the functioning of this lore in the lives of the Dobuans. The text is replete with factual and interpretative data, resulting from a five-months' study of exceptional method and success in the "unmissionized island of Tewara."

Adoption into a tribe, with immediate use of its language ("acquired by contagion"); allaying the suspicion and jealousy accorded the stranger; procuring a house and garden, made with due magic ritual; joining in land and sea expedition for hunting or barter; sharing in dour looks for truculent trader or shout of triumph at bringing home the wild boar (substitute for former cannibal victim); trussing up or drenching with cold water the maniac "running amuck with a spear;" healing with salts and good food the victim "dying" from sorcerer's spell—these were the intrepid means of first hand observation. Moreover, the author listened to the pronouncements of government official, the "innocent" interpretations of near-by missionary, the island gossip of trader living with native woman, and the whispered revelations wrested (by white or black magic!) from the most secretive of the sorcerers themselves.

Magic, with the fears, hopes, and cruelties it engenders, is at the basis of every act of Dobuan life. Sorcery (if practised by a male) or witchcraft (if by a female) controls or accounts for all the toward or untoward events, from change of season to a birth or a death. The diviner must determine the secret enemy who wished, ergo caused, demise. Though naturalistic means may be employed in the growing of yams or the poisoning of an enemy, yet it is the letter-perfect weaving of a charm that controls the mystery beyond man's reach—the alchemy of growth or decay. Social obligations, moral and economic, are observed, if at all, through fear of a vengeful magic. This primeval fear, not the white man's government or religion, is as yet, according to the author, at the root of all personal control.

Sex life among the Dobuans is marked by the speech prudery and actual depravity attributed by twentieth century sensationalists to nineteenth century Nordics. The "*susu*," or matrilineal clan, is more powerful than the marriage group. Exogamy is practised, always with the hazard of entering an enemy group whose magical powers are unknown. Divorce is common, where the debacle is not ended by attempted or threatened suicide.

The author questions the ability of the church by milder teachings or of the government by severity of punishment to stamp out sorcery. Recognition of sorcery, by punishment for instance, but confirms the natives' belief in its reality, prevents its dying from its own failures, and serves to keep intact a field for the anthropologist. He questions also the oft repeated stories of "the ease with which the native dies" from sorcery alone, unassisted by poison administered by an enemy or induced by suspended vital functioning. Whether or not the hypnotic spell cast by "meeting the enemy face to face" is profound enough to result in death; whether or not in Dobuan or other societies strong matrilineal loyalties may be a factor in clannishness or exclusiveness; and whether or not there may be some humane shortcut from primitive, barbaric fears and cruelties to a utopian fellowship and coöperation, are questions intriguing the interest of the reader and inviting the further investigation of the physiological psychologist and philanthropist as well as the social anthropologist.

Woman's College of Alabama SARAH M. RITTER

Psychology and Psychiatry in Pediatrics: The Problem. BRONSON CROTHERS and others. The White House Conference on Child Health and Protection. Report of the Subcommittee on Psychology and Psychiatry. New York, The Century Co., 1932. Pp. xiii, 146.

The deliberations and investigations of the Committee, here reported, deal largely with the question of guidance of normal personality development and the treatment of personality difficulties in children. Is this a task for the general medical practitioner, pediatrician, psychologist, psychiatrist, clinical psychologist, social psychiatric worker, parent, educator, or for a combination of two or more of these? The obvious answer, of course, is that responsibility lies with all of those mentioned, depending on particular circumstances and the nature of any disorder present. "Without question, the home and the school must do most of the work." The problem, therefore, resolves itself into one of leadership and initiative. The Committee here makes a clear-cut distinction between guidance and preventive measures, on the one hand, and treatment of palpable aberrations of the individual personality on the other. The former aspect of the problem is left largely to the parent, teacher, and psychologist. Individual distress, however, is held to be primarily the concern of medical men. "Adequate medical care of the child cannot be given without intelligent attention to any intellectual and emotional difficulties which may be present. . . . When trouble arises and the individual child is in distress a well informed and alert physician is the obvious adviser."

Since the problem is, after all, a practical one, it would be beside the point to take issue with the questionable justification of even a working dichotomy between normal and abnormal, prevention and therapy. This schism, of course, is common in medical thinking and practice. What can and should be seriously considered, however, is the practicability and wisdom of placing primary responsibility for the mental health of his patients on the already heavily burdened shoulders of the family doctor. There are, clearly, arguments both for and against such procedure.

Quite certainly the prestige and confidence enjoyed by physicians gives them an important advantage in any kind of therapeutic work. Certainly, also, the capable physician has learned to supplement his medical procedures invaluably by proper "psychological" factors: his bedside and consulting-room attitude. But this brand of psychology, which is to a limited extent part of the equipment of good salesmen, "ad" writers, lawyers, and confidence men as well as of doctors, touches but lightly the field of scientific psychology. The question, then, is whether scientific psychology has anything to contribute towards treatment of mental disorder and, if so, whether the practicing physician can and should be the one to apply such discoveries in dealing with individual cases.

The Committee differentiates clearly the "psychiatric attitude" and the "technical resources" of psychologist and psychiatrist. The former, it is felt, can be acquired readily by any intelligent doctor and will suffice for most cases of behavior abnormality. This, we believe, is a gross underestimation of what the psychiatric attitude can and should be. With experience, some (by no means all) doctors acquire the ability to handle behavior problems with considerable "insight" and success. It is no less than reasonable to expect, however, that a background of psychological training, thorough acquaintance with the field if not mastery of all the techniques, would enormously benefit even those endowed with an "intuitive" understanding of human nature. The "technical resources" are considered less essential for the family doctor than the psychiatric attitude. "Obviously there are details of . . . methods of testing intelligence and so on which are reliable only when interpreted by specially trained individuals. . . . If he [the doctor] feels that his time-table allows patient and often prolonged conversations, he can begin to acquire technical resources." With this viewpoint the psychologist and psychiatrist cannot well agree. Few if any psychologists would maintain that it is possible to dispense with the services of a physician in dealing with mental cases; they realize that a discoverable organic difficulty may be at the root of the manifested behavior and, further, that the doctor's experience with sick people may have given him an invaluable tool in sympathetic understanding of the difficulty. But neither would the psychologist agree that it is possible to handle behavior problems adequately and intelligently without thorough psychological and psychiatric training. A standardized psychological test procedure, for instance, administered and interpreted by an expert, would seem to be as necessary a preliminary to subsequent procedure as is a thorough medical examination. The point is one more easily settled by opinion than by demonstration. At any rate, we may expect in the future a steady increase rather than any decrease in the body of specialized knowledge and techniques pertaining to the field of juvenile personality.

Whether or not the physician, the family doctor, of the future can afford to shoulder the burden of thorough psychological and psychiatric training, remains to be seen. The problem is closely related to the general one of specialization among medical practitioners. In this connection both doctors and psychologists might do well to bear three points in mind. First, the similarity between physical disease and behavior difficulties implied in the statement that "when . . . the individual child is in distress a well informed and alert physician is the obvious adviser," is, in the present state of science, largely a verbal one. Secondly, behavior troubles cannot be excised as can be a tumor nor can they often be treated successfully by a prescription. Diagnosis and cure of mental disorder are usually time-consuming processes. Thirdly, the average psychologist of to-day is rarely either interested in or fitted for the job. It is essential to recognize, also, that the organization and intra-group responsibility found among medical men makes it far safer to entrust decisions involving radical procedures to them rather than to psychologists who, at present, have no comparable safeguards to offer. The ultimate solution, we may expect, will come through gradual evolution rather than by sudden decision or decree, and will be strongly influenced by the direction of scientific progress as well as by social and economic trends.

Yale University HENRY W. NISSEN

Fields of Psychology. By GLENN DEVERE HIGGINSON. New York, Henry Holt & Co., 1931. Pp. xviii, 613.

Higginson's book is designed for three classes of students, those in whom a bird's-eye view may awaken particular interests, those "who do not plan to continue their psychological studies, but who, nevertheless, desire a fuller understanding of the subject matter than an elementary course can possibly provide," and those who find themselves in smaller departments where a diversified curriculum is not offered. Part I (72 pages), briefly discusses scientific method and psychological systems. Part II (234 pages) considers evolution, animal psychology, the development of the human race, child psychology, and social psychology. Part III (182 pages) deals with educational psychology and applied psychology. Part IV (117 pages) is devoted to abnormal psychology.

The general point of view is functional and biological. The attempt to cover a very large number of topics has rendered the treatment in many places brief—a difficulty recognized by the author. Practically no space is given to physiological psychology, a chapter is given to comparative, to child, and to social, two chapters to educational, three to applied, and five to abnormal. This division seems markedly out of balance. Also a chapter on evolution and another devoted to archaeologic findings on primitive man seem a little out of place. Perhaps the chief contribution of the present work is that of sensing a need and assembling a wide variety of material under one binding.

University of Wisconsin KAI JENSEN

The Free-Association Method and the Measurement of Adult Intelligence. By HERBERT S. CONRAD and DANIEL HARRIS. University of California Publications in Psychology. Berkeley, University of California Press, 1931. Pp. 45.

To test the intelligence of adults who are averse to submitting to the process, the authors have sought to utilize the free-association technique and to construct from it a valid intelligence-test. The authors chose 120 words as stimuli, attempting to avoid ambiguity, slang, and polysyllables. The list contained 68 nouns, 29 adjectives, 22 verbs, and 1 preposition. The words were presented to 297 Jewish students, aged 11 to 15 yr., as a validating measure, since the results from this testing could be correlated with National Intelligence Test scores from the same subjects. The measure of validity which resulted was 0.78. Hence, it was supposed that the test would measure equally well or better the intelligence of adults.

The results from the free-association test were classified into three categories: type of relation between response and stimulus, such as the relationships of coördination, opposites, specifications, and inadequacy; vocabulary designation, such as hard-words, good-words, and misspellings; and reaction-time or speed, somehow (not clear to the reviewer) obtained from the number of omitted responses, revised omissions, and total omissions. These criteria were found to be very useful, while predication, definition, word-habit, repetition, slang, non-specification, multi-verbality, and exceptional and unclassifiable responses were discovered to be useless.

Apparently the reliability of each criterion, and its validity against the Army Alpha score, was measured on 15 adults, aged 18 to 22 yr. "Hard-word" responses proved to possess the highest reliability (0.87 on 166 children).

The paper contains 15 tables, 2 figures, and a bibliography of 18 references. The discussion ends with a somewhat deductive, non-experimental comparison of the test with Stanford-Binet and other standard tests. Among its multifarious virtues are its novelty, acceptability to adults, appropriateness for group (and even mail) administration, sufficient disguise to ward off cheating when taken at home, and its proved validity for group comparison.

University of Oregon R. D. ADAMS

Sociologues d'hier et d'aujourd'hui. By GEORGES DAVY. Paris, Félix Alcan, 1931. Pp. 308.

It is Davy's contention that an exact knowledge of contemporary French sociology is impossible unless we are acquainted with its sources. From this point of view, the title of the book is somewhat misleading. It is not a review of past and present sociologists but is rather an analysis of certain recent authors whom Davy feels are largely responsible for present sociological trends in France.

The volume is divided into four parts, each of which is almost entirely devoted to the work of a single author. Part I is a survey of the works of Espinas. Part II considers a single aspect of Durkheim's sociology which best illustrates that author's method; namely, his theory of the origin of the family. Part III is a review of the method of McDougall, and Part IV is an integration of the writings of Lévy-Bruhl.

Each part of the book gives an excellent digest of its particular topic. The exposition is, however, almost entirely uncritical. Davy has made very little attempt to show the relationship of one author to another or to trace the modifications of older doctrines as they appear in more recent writings. This is due, perhaps, to the fact that the book is a series of separate studies. The scope of the volume is extremely nationalistic. Even if we grant that France was the cradle of sociology, it seems unfortunate that McDougall and Spencer should be the only non-French authors considered. The names of Gumplowicz, Ward, Giddings, Small, Sumner, and Pareto as well as those of more recent sociologists are entirely omitted.

Harvard University HADLEY CANTRIL

Prediction Factors in Probation: A Study of 1515 Probation Cases of Ramsey County, Minnesota, for the Years 1923-1925 Inclusive. By ELIO D. MONACHESI. Minneapolis, The Sociological Press, 1932. Pp. ix, 117.

The author, using the court records of individuals' histories before and after being put on probation, found some facts such as age, easily classified; whole other facts such as social status required conceptual treatment. The reliability of the latter classifications was tested by reclassification after a number of months, and 60–80% of the individuals were classified the same.

The violators of probation were placed in one group and the non-violators in another, and tables were constructed to show what facts were most characteristic of each group. Of the children of foreign born parents 73% were non-violators, and of native born parents, 68%. Children of good economic status, good and unbroken homes, and those with parents of the same religion, also showed a large percentage of non-violators. Children who leave home at an early age and those who have had home difficulties, are numerous among the violators of probation. The number of non-violators is directly related to the amount of education previously received. Space does not admit of giving the results of the comparison of numerous other factors. The author concludes that although court and probation records are not complete or exact and although no one type of fact is a sufficient basis for predicting the outcome of probation, yet combinations of a number of facts make possible fairly accurate predictions as to which persons will be likely to violate probation.

Tables constructed on the basis of such facts as these may be used either to select those to be refused probation or to decide upon the amount of supervision the individual probationer will need. The author, using the methods employed by Burgess, Glueck, and Vold in the study of paroled criminals, has begun the construction of prediction tables which can undoubtedly be made to serve as guides in dealing with candidates for probation. He has also made a contribution to the scientific study of predisposing tendencies toward conduct, and to a future scientific ethics.

A good bibliography and an index are appended.

Leominster, Mass. EDWIN A. KIRKPATRICK

Tonpsychologie und Musikästhetik: Art und Grenzen ihrer wissenschaftlichen Begriffsbildung. By HEINRICH SCHOLE. Göttingen, Vandenhoeck & Ruprecht, 1930. Pp. 139.

This work is not intended to be a systematic presentation of tone-psychology and musical aesthetics. The author's purpose is rather, as the subtitle indicates, to consider the types of concepts which are valuable in these fields and to show their limitations. Schole lays stress upon two methodological principles. In the first place, a careful "phenomenological" examination of the elementary phenomena of music is needed. The concept of atomistic elements must be abandoned, the criticisms by speculative *Geisteswissenschaftler* of experimental psychology thus losing much of their force. Secondly, psychological research must devote itself more than hitherto to complex musical forms.

Schole's treatment of musical elements, in the first part of the book, is limited to their psychological or experiential aspect. He disregards neural factors completely and refers to stimuli only for the sake of convenience. Partly under the influence of Köhler's studies of simple tones, Schole has been led to a peculiar doctrine of tonal attributes. A simple tone possesses but one single attribute, *Tonkörper*. Pitch, intensity, *Tonfarbe,* brightness, vocality, and volume must be considered results of relational activity "die aus der vergleichenden Betrachtung von Qualitätsmannigfaltigkeiten gewonnen werden, aber der einfachen und erlebnismässig unzerlegbaren Qualität kein neues Merkmal hinzufügen können" (p. 13). Among these six concepts, intensity and *Tonfarbe* are found in the *Tonkörper* itself, while the other predicables, in so far as they are not synonyms for the first two, owe their existence to still more complicated relational functions leading beyond the simple quality itself. Schole's insistence that true attributes must be found immediately within the tone is quite foreign to the more generally accepted view that tonal attributes are conceptual products resulting from comparison of tones under particular instructions.

Much of the remaining material in the first part of the book is critical rather than constructive. The author attacks mercilessly Ernst Kurth's approach to the problems of the psychology of music.

The second part consists primarily in an analysis of the first movement of a Beethoven sonata (*Op. 31*) and concludes with a summary of the author's theory of the nature of musical hearing. This portion of the book will be of greater interest to musicians and aestheticians than to psychologists.

Students of the psychology of music will find throughout the book many stimulating suggestions, even though the writer's arguments are not always convincing. American readers will note with surprise that Scholė has cited only writings in the German language.

University of Rochester C. W. Fox

Intelligence and Disease. By SHEPHERD DAWSON, assisted by J. C. M. CONN. Special Report Series, No. 162, issued by the Medical Research Council. London, His Majesty's Stationery Office, 1931. Pp. 53.

The work of this study was begun in June, 1922, by Dr. H. J. Watt, who died in 1925. The investigation is complementary to a research conducted by the late Professor D. Noël Paton, Dr. Leonard Findlay, and their collaborators, the results of which were published by the Medical Research Council in 1926 under the title *Poverty, Nutrition, and Growth,* forming part of a scheme for the study of child life initiated by the Council. In that study it was found on some 14,000 children living in the slum areas of Glasgow, Edinburgh, Dundee, in a rural mining area, and in an agricultural area that of the various external factors studied "maternal efficiency, as indicated by the care of the child and of the home, was the only one which played a significant part in determining nutrition in the child. Over-crowding, estimated either by the size of the family or by the amount of air-space available per person, and poverty, as indicated by income per person, did not seem to influence to any significant degree the child's nutrition" (5 f.). Whether this maternal efficiency is innate intelligence or a result of environmental conditions under which the mother lives was a question that needed further study. The present investigation was continued in Dr. Findlay's department at the Royal Hospital for Sick Children and the Ruchill Hospital, Glasgow, with a view to throwing light on that problem. The mental testing since 1925 was done by J. C. M. Conn, who had formerly worked with Dr. Watt.

The testing involved over 1020 patients, ranging in ages from 3 to 14 yrs. The 3-yr.-olds were evidently superior samplings (p. 11), and the 56 above 11 yr. were probably inferior. The mean age of the entire group was 7.7 yr. Burt's translation of the 1908 Binet scale was used. The correlation between age and I.Q. was found to be approximately zero. The mean I.Q. of the different age groups ranged from 0.82 to 0.96 (highest and lowest ages, respectively), but usually was near 0.90. The children seem to have been fair samplings of the normal children of the same area, so far as intelligence is concerned, when not affected by certain specific brain diseases. The problem, then, centered on the question: "Does illness in general retard intellectual development or is it only certain diseases that have this effect, and what are they?" (P. 21.) Every patient was tested before, during, and immediately after his illness; and a retesting was given during convalescence and also when the patient had recovered. In addition, patients suffering from any particular disease were compared with their healthy siblings, who were brought to and tested in the hospital with the same tests as those used in the case of the patients. Full data are presented in tables, and results are carefully considered as to reliability and to the effects of spurious factors.

It is concluded that "disease on the whole does not appear to have any appreciable effect on intelligence" (p. 51), even though diseases of the spinal cord be included, as well as neuritis and chorea. On the other hand brain diseases—epilepsy, postencephalitis lethargica—and certain ductless gland diseases were found to be associated with mental deterioration. No such clear evidence of deterioration was found generally in the relatively few cases of the Parkinsonian syndrome, spastic diaplegia, and cerebral and cerebellar tumors. While "chronic illness did not appear to retard seriously the development of intelligence," acute stages of illness was on the whole associated with a "slight improvement in mental efficiency" (p. 51). This improvement seemed to be due to a slight hypersensitivity in the acute stage. Unfortunately the number of cases studied was small in the case of several of these specific brain diseases.

There is a bibliography of 56 titles. J. P.

Handbook of Psychological Literature. By C. M. LOUTTIT. Bloomington, Ind., Principia Press, 1932. Pp. viii, 273.

Designed by its author for those psychologists who find themselves at a loss while searching in unfamiliar fields, this volume may be of interest to others, if only to remind them of the bulk of published material that has accumulated in psychology and its allied disciplines. After early chapters devoted to bibliographic techniques and to the various sources of reference for the student of psychology, the author presents as his chief contribution a list of 1084 periodicals which contain varying proportions of material of interest to researchers in the several phases of psychology. He warns us early that "the number of papers of psychological value may be conservatively estimated as over 100,000" (p. 4). Later, in a more expansive mood he ventures the statement that "conservative and very approximate estimates . . . indicate something over 150,000 as the number of contributions to the literature" (p. 79); and as one reads over this appalling catalogue of journals, living and dead, one is not tempted to deny the author the added 50% of his second estimate.

The list which, the author tells us, "has been selected almost entirely from the *Union List of Serials* and its *Supplement*" (p. 137) arranges alphabetically periodicals which contain matters of psychological interest. In most cases the name of the publishing organization or place of publication, together with the date of first publication and an indication of its later fate, is given; but this procedure is not followed rigidly. Here are to be found the names of journals of all sorts, from the most staid of the *Zeitschriften* down to long defunct periodicals of phrenology, hypnotism and animal magnetism. Since numerous journals in foreign tongues are included, one might almost write a history of psychology by scrutinizing the list in detail and noting the few hardy survivors among the large numbers of decadent, decayed and fossilized remains interred in these columns.

The author points out that such a catalogue can have final value only after careful revision, and he writes that "every effort has been made to ensure accuracy, but if errors have occurred (*sic*) the author would appreciate their being called to his attention" (p. 137). The space-limits of this review will not admit of detailed criticism, but the following are offered as typical of changes which should be made:

(1) Some consistent rule should be applied to the use of upper and lower case letters. Thus *Psychological Index* appears with both initial letters capitalized on one page (29) while only the first is so treated on the next page. Such vagaries of capitalization occur frequently, and could be eliminated by careful editing. The

present reviewer is unable to see any gain in the use of lower case letters for all but the initial word in the titles of the journals themselves; it would seem advantageous to follow the more customary procedure in this regard.

(2) The names of the journals appearing in the earlier chapters should be carefully checked and corrected. It is astonishing that the author should have failed to notice three references to a French contemporary as "L'Annee Psychologie" (pp. 94, 101, 102) and one to what the author calls "Wundt's Philosophisches Studien" (p. 23). At least two other journals not available to the reviewer appear to have suffered distortions in title which are contrary to fundamental rules of the German language. In the section on handbooks, there is no reason to abbreviate the title of Abderhalden's *Handbuch der biologischen Arbeitsmethoden* in the curious fashion employed by Louttit.

(3) Newer sources which were available two or more years before the publication of this volume should be included. Thus Dumas' *Nouveau Traité de Psychologie* (1929) should replace the earlier edition cited. The general bibliographic citations from various issues of *Science* (p. 10) might well give way to a single reference to the article in the 1929 *Psychological Bulletin* which presents the standard form of reference adopted by the conference of editors of psychological journals. Some reference should be made to Bethe's *Handbuch* in the appropriate section; and Eisler seems at least as deserving as Baldwin of a place in the encyclopedic section, instead of being relegated to a corner among the dictionaries.

The volume closes with appendices which contain such items as an index of subjects and names, a categorical index of journals, and a list of references documenting the volume, as well as lists of special collections which may be of value to psychologists. Several tables throughout the volume are provided to show distribution of journals by countries and by years. If the volume meets with sufficient demand to justify revision, the author would gain by submitting his manuscript to psychologists here and abroad in an effort to make it as representative and as accurate as possible.

Cornell University JOHN G. JENKINS

660 Runaway Boys. Why Boys Desert Their Homes. By CLAIRETTE P. ARMSTRONG. Boston, Richard G. Badger, 1932. Pp. 208.

In this monumental statistical study, 660 runaway school boys, whose entire records, covering physical, psychological, psychiatric, neurological and serological examinations, were on file in the clinic, were compared with similar records of two control groups who had never run away from home—one group charged with incorrigibility at home and the other with unlawful entry. All three delinquent groups were compared in various measurements and ratings with unselected school children. All the runaways in this study were "delinquents." Runaways held in court as "neglected children" or on an "improper guardianship" complaint were excluded. Scientifically objective almost to the point of dullness in earlier chapters, this book rises to heights of oratory in the summary and conclusions. The author, having shown that many criminal careers start with running away from home, thinks that society should shoulder the responsibility and do something to prevent it.

Moralists of an earlier age might have reasoned so far—in fact they evidently intended to stop runaways when they made it legally a delinquency for the child to desert his home without just cause. The author, however, has discovered that remaining in the home may be to many children a severer hardship than any penalty the law can impose on them for leaving it. The common assumption that boys leave home chiefly from *Wanderlust* proved erroneous. Of the 660 boys studied, 590 never left New York. The basic motive for nearly every runaway was escape. In 12% of cases, the boys had been severely beaten. Another 32% feared punishment. When no physical fear was involved, there was often fear of humiliation. The public school with its rigid requirements and lack of provision for the dull nonverbal type of boy is held by this author to be a major cause of "truancy, so often the stepping-stone to other delinquencies." "A problem of first importance, practical and lending itself to immediate solution, confronts the schools as they stand today. It is not primarily how to cure nervous, maladjusted children but how to avoid making them."

West Springfield, Mass. MABEL F. MARTIN

Untersuchung der Entwicklung des Zeichnens. By HERBERT GRAEWE. Halle (Saale), Hermann Schroedel, 1932. Pp. vi, 180.

In this study, Græwe has attempted to trace the course of development in children's drawings from a broader, less specialized point of view than many of his predecessors have taken. Instead of obtaining a small number of drawings from each of a large number of subjects, he secured from each of his 143 subjects drawings of many different types made under a variety of conditions. In all, 6250 drawings were obtained. The subjects included a group of 37 kindergarten children ranging in age from 3 yr., 2 mo. to 6 yr., 6 mo.; 96 grade school children between the ages of 6 and 14 yr.; and 10 adults. The drawings tests for the kindergarten children included copying geometrical figures and pictures of common objects, drawing simple objects from a model placed before them, drawing the same objects from memory after a brief interval, drawing a picture of their own mother, a man, a child and a table from memory, and drawing a witch from imagination. The tasks set for the older children were of a similiar nature but slightly more difficult.

Those who have even a slight acquaintance with previous work in the study of children's drawings will not be surprized to find that specifying that the children were to draw "their own" mother had no effect upon the schematic type of drawing produced by these young children. Drawing the witch proved to make no more demand upon "imagination" than the other types of drawing, since the children merely attempted to reproduce the conventional Hallowe'en figure.

No statistical analysis of the results is given apart from a few percentages, but there is a general descriptive treatment of the changes that occur with age in each type of drawing. There is little or no attempt at correlational treatment even in terms of casual observation, hence whatever advantage might theoretically accrue from the intensive study of a small number of cases is lost. Each type of drawing and each general aspect of drawing (as motor coördination, representation of space, etc.) is considered separately.

A bibliography of 263 titles and reproductions of 26 drawings are appended. There is no index.

University of Minnesota FLORENCE L. GOODENOUGH

Mutter und Sohn. By Karl Clauss. Pädagogisches Magazin. Langensalza, Hermann Beyer & Sons, 1931. Pp. 104.

As a result of the World War many children were educated without the guidance of a father. The author made an intensive study of 189 boys in this class of ages ranging from 8 to 20 yr. Information was obtained from the subject's mother, teacher, and companions, by means of interviews, questionnaires, and casual conversations. The results were treated statistically. Special attention was given to the occupational status of the family and the psychological type of both mother and child, the author adopting G. Kerschensteiner's simple division of personalities into active and contemplative types, corresponding broadly to the well known classification of extraverts and introverts. In general, boys of both types suffer an educational handicap from purely maternal direction. This is greatest when mother and child are of opposite psychological type, but boys of contemplative type suffer most. Exaggerated attitudes, one-sidedness and failure to develop a sense of reality, cover most of the defects encountered. Where a vivid and pleasing memory of the father persists (*Vaterideal*), as is often the case in children from the military and official classes, no bad effects occur. It would be interesting to make similar studies among boys in other countries affected by the war, and the method might well be extended to groups of children of either sex, deprived during developmental years of either parent, whether by war, accident, divorce or other cause of separation.

University of Cincinnati — Charles M. Diserens

The Psychology of Exceptional Children. By Norma V. Scheidemann. Boston, Houghton Mifflin Co., 1931. Pp. xi, 520.

This treatise is comprehensive in the topics with which it deals and in the treatment of each. The style is lucid, to the point, and very readable. The comprehensiveness of the text can be indicated adequately by a list of the subjects discussed: origin of trait differences, the concept of general intelligence and its measurement, speech disorders, handedness, the subnormal child, special types of feeble-mindedness, gifted children, the psychoneurotic child, the psychopathic child, the deaf child, the blind child, the word-blind child, the delinquent child and other types of exceptional children, such as the epileptic, the hysterical, and the undernourished.

There is a marked difference, however, in the quality of the various chapters. The handling of the material on the psychoneurotic, the psychopathic, and the congenitally word-blind child fall far below the treatment of speech disorders, handedness, and feeble-mindedness. One has a feeling that the author was not always sure of herself, and through uncertainty, did not treat some subjects with the same insight as she did others. The citations throughout the book are good. Illustrative cases are used throughout.

Western Reserve University — L. Dewey Anderson

Experimental Child Study. By Florence L. Goodenough and John E. Anderson. New York, Century Co., 1931. Pp. xii, 546.

This book is "frankly experimental and observational in tone rather than philosophical." As such it is a distinct contribution to the field of child psychology.

The authors have followed a somewhat novel plan of presentation in organizing the book into forty-seven relatively short chapters. Most of these chapters have brief preliminary discussions of the problem under consideration, followed by one or more suggested experiments or test exercises for students to perform. Thus, the book in many ways is an elaborated laboratory manual which has been so arranged that it can be used as the basis for a course outline for class discussions.

Two of the very best features of the book are its bibliography and glossary. The bibliography of 403 titles is rather carefully selected, and has the added feature of being based largely on publications in English. This is a real contribution to students, both from the standpoint of their own reading and from the standpoint of the availability of library materials. Then, the glossary is of practical assistance to students who are incompletely oriented in psychological terminology.

The book furnishes an excellent reference for classes, but its main limitation is probably that it is better adapted to extra work than to regular class work. It appears to assume a foundation in psychology more extensive than is possessed by the average student just beginning child psychology. At the same time, its policy of treating subjects extensively rather than intensively does not seem to appeal to graduate students with special interests in the field of child psychology. Many of the experiments are not adapted to graduate work. A second matter which should be considered is that the book is pointed avowedly towards the pre-school or early-school child. This limited scope or perspective circumscribes its usefulness in classes where the instructor must present child psychology "from the cradle to the grave." Withal, the book could be used profitably as a second text in many courses.

George Peabody College for Teachers PAUL L. BOYNTON

Die Psychologie des Grundschulkindes in ihrer Beziehung zur kindlichen Gesamtentwickelung. By OSWALD KROH. 10th ed. Pädagogisches Magazin, Heft 1122. Langensalza, Hermann Beyer & Sons, 1931. Pp. 353.

This book emphasizes the child's personality. The main phases of development are reviewed and then the child of the primary grades is more thoroughly discussed.

The ego, the basis for intellectual development; the perceptual, learning and memory processes; the intellectual mechanisms; the development of emotions and values, of moral, religious and esthetic experience; the development of social attitudes, of expression, of interests in play and work, are treated in a stimulating fashion. The four years of *Grundschule* are treated as corresponding to a definite stage in child development. The author discusses the functions of the *Grundschule* from the standpoint of his findings.

The book cannot claim to be a complete treatment of the subject. Developments abroad are completely ignored; the words "test" or "intelligence" hardly occur, the psychology of learning and adjustment are treated rather sketchily. Yet the viewpoints are stimulating, the use made of the findings of experimental psychology is interesting and the general emphasis on personality development and on mental content and functioning as aspects of this development, is very valuable. The author is not at all concerned with the "efficiency" of teaching in a narrow sense. To him the teacher's fundamental responsibility is that of guiding the child's harmonious development.

Wayne County Training School, THORLEIF G. HEGGE
Northville, Michigan

The Measurement of the Intelligence of Young Children by an Object-Fitting Test. By RUTH ELLEN ATKINS. Monog. Series No. V, The Institute of Child Welfare. Minneapolis, Univ. Minn. Press, 1931. Pp. xii, 89.

Like practically all the studies in child psychology which come from the Institute of Child Welfare, this study is a real contribution to the literature of the field. It has evidently been planned thoughtfully and conducted carefully. The cogency of the interpretive analyses deserves particular mention.

The test is probably more self-motivating for young children than some examinations which have been designed to cover the same age ranges of one and one-half to five and one-half years. It is based on such commonplace objects as spools, cups, salt shakers, tops, and pencils, with all of which most children in these age ranges like to work and play. The test is short; it is available in two forms; its reliability coefficient is around 0.87; it is strictly a non-language examination; the situations involved are sufficiently controlled so as not to confuse the subject by their complexity; and estimates of its validity show that it has a practical clinical significance. The last chapter in the monograph is a manual of directions for giving and scoring the test and for translating its scores into mental ages.

George Peabody College for Teachers PAUL L. BOYNTON

The Wild Boy of Aveyron. By JEAN-MARC-GASPARD ITARD. Tr. by George and Muriel Humphrey. Introduction by George Humphrey. New York, The Century Co., 1932. Pp. xxiv, 104.

In 1799 there appeared in the *Journal des Débats* a letter by a certain Bonaterre describing a wild boy (probably about twelve years of age) who had been taken in the woods of the Department of Aveyron. The boy "had been found almost unclad, wandering about the outskirts of the forest in which he had apparently lived for some years" (p. vi). "Other indications of an entirely isolated, precarious and wandering life," writes Itard, "are the nature and the number of scars with which the child's body is covered" (p. 9). The boy had been seen over five years earlier entirely naked and fleeing at the approach of man. He was taken to Paris where inquisitive people "looked forward with delight to witnessing the boy's astonishment at the sights of the capital." Some people otherwise "commendable for their insight . . . believed that the education of this child would only be a question of some months, and that he would soon be able to give the most interesting information about his past life. In place of all this," says Itard, "we see a disgustingly dirty child affected with spasmodic movements and often convulsions, who swayed back and forth ceaselessly like certain animals in the menagerie, who bit and scratched those who opposed him." Though Pinel examined the boy and reported him an incurable idiot, instead of merely a wild boy, Itard, who had early attained some distinction in medicine and at twenty-five years of age had been appointed physician to a new institution for deaf mutes, was willing to try his view of the effect of training on the boy.

Pinel's report is unfortunately not available today. Of the two memoirs translated in this book, the first one, "First Developments of the Young Savage of Aveyron," was originally published in 1801, and the second, "A Report Made to His Excellency the Minister of the Interior," which is divided into three series on the development (a) of the "functions of the senses," (b) of the "intellectual functions," and (c) of "emotional factors," was submitted to the minister by request in 1806. The translation is made from the reprinted edition of 1894, *Rapports et Mémoires sur le Sauvage de l'Aveyron,* now out of print. Itard devoted five years to the training of the boy who died in 1828 at approximately forty years of age. Itard lived ten years beyond this time.

The story of the aims and methods used in the laborious attempts at the education of this boy, results obtained, and Itard's own views, hopes, and discouragements, are interestingly and clearly narrated. The translators and publishers are to be complimented on the excellency of the translation and for making available these memoirs to English readers.

J. P.

The First Two Years. By MARY M. SHIRLEY. Monog. Series No. VI, Vol. I, The Institute of Child Welfare. Minneapolis, Univ. Minn. Press, 1931. Pp. xv, 227.

This is distinctly an intensive research. It is a report of an analysis of 16 babies from birth to two years of age. Though the study is primarily concerned with locomotor development, records of each baby were kept throughout the entire period on (1) anthropometric measurements, (2) health, nutrition and general well-being, (3) motor coördination, (4) sensory development, (5) speech development, (6) interests, and (7) behavior and personality traits. The babies involved were from an admittedly "upper class" group of homes, but since the author is not attempting to generalize for all babies, this selectivity of cases does not vitiate the study. The investigation was made under as carefully controlled conditions as one could expect in a field where private homes have to be laboratories and where the infants in these homes have to serve as subjects. Child psychology needs more of such analytical studies.

George Peabody College for Teachers PAUL L. BOYNTON

Neuropsyche und Hirnrinde: I. Der bedingte Reflex und seine Bedeutung in der Biologie, Medizin, Psychologie und Pädagogik; II. Physiologische Grundlagen der Tiefenpsychologie unter besonderer Berücksichtigung der Psychoanalyse. By N. E. ISCHLONDSKY. Berlin and Wien, Urban & Schwarzenberg, 1930. Pp. xviii, 328; xv, 356.

In two laborious volumes the author expounds Pavlov's theory of the conditioned reflex and applies it to the fields of psychology and medicine. The first volume recapitulates the contents of Pavlov's two books concerning conditioned reflexes. The method and the theory derived from its use are described at length with the aid of numerous colored diagrams which suggest to the teacher their suitability for blackboard illustration. The few pages of the first volume (71-75), however, in which the author describes his experiments on tactile localization in skin areas grafted from forearm to face and explains the "education" in localization of the grafts is exciting reading. For the rest, the reader can gain nothing from a perusal of the first volume which cannot be better acquired by consulting Pavlov's writings. The author's exposition is devoutly orthodox and hence uninteresting.

The second volume is a *tour de force* of mental gymnastics in the explanation of complex mental processes and their aberrations in terms of conditioned reflex theory.

Cornell University H. S. LIDDELL

Our Movie Made Children. By HENRY JAMES FORMAN. New York, The Macmillan Co., 1933. Pp. viii, 288.

Mr. Forman has summarized, in popular form, the findings of twelve separate investigations on the influence of motion pictures on children. These investigations were carried out under the auspices of the Motion Picture Research Council and supported by the Payne fund. The titles and authors of the studies are listed with no reference to their place of publication or availability.

On the average 28,259,000 minors, of whom over eleven million are under fourteen years, attend the movies every week in the United States. The average movie attendance per child is one movie a week. These children, even the young ones, remember almost as much as do adults of what they saw in a picture. The pictures are powerful instruments of propaganda, *e.g.* a single picture was shown clearly to establish or to remove, race prejudices. The children are deeply stirred emotionally by even mildly exciting movies; this is shown by psychogalvanic reactions and pulse rate, and by the fact that their sleep is disturbed for several nights after seeing a motion picture.

The section of life displayed by the movies is a distorted one, with sex and crime themes and luxuried leisure-class predominating. Since the children were found to accept the pictures as true representations, and to grasp only individual scenes rather than the story as a whole, the pictures appear to have encouraged many children to delinquency and crimes, both by having stimulated desires for luxury and leisure, and by giving instruction in methods of 'petting' and techniques in committing crimes.

Psychologists will probably prefer to read the reports of the original investigators. The present volume, as a popular interpretation which emphasizes the need of social and educational control of children's movies, is impressive. At times, however, it labors a point by unnecessary repetition.

University of California NANCY BAYLEY

Developing Attitudes in Children. Proceedings of the Mid-West Conference of the Chicago Association for Child Study and Parent Education, March, 1932. Pp. vii, 156.

This volume consists of ten papers presented at the Mid-West Conference on Developing Attitudes in Children. These papers are varied in content and differ in their general approach. Some of them are philosophical in nature. This is true of the papers by Harry Elmer Barnes, Glenn Frank, and Harry Gideonse. There is a persistent warning throughout some of the papers relative to racial bigotry, international relations, and citizenship training, lest we carry over into the teachings of children the prejudices and biased attitudes of our present time. The papers vary from the strictly scientific approach made by Ruth Peterson, "The Effect of Motion Pictures on the Social Attitudes of Children," to the rather colorful philosophical approach already suggested. There is, however, a general unity in purpose and coherence in basic principles and foundations upon which the papers are built.

In the first paper Hartshorne emphasizes the relation between ethical self-control and desirable attitudes. Throughout the various papers the importance of the early childhood years for training and attitude-development is stressed. The complexity of modern life, with its many materialistic developments in a complicated social and political order, makes it most essential that growing boys and girls do not develop undesirable, twisted and warped attitudes. This point is stressed in many of the papers. Parents, teachers, and others concerned with child training will find these warnings and varied suggestions helpful.

North Carolina State College K. C. GARRISON

Die juvenile amaurotische Idiotie. Klinische und erblichbkeitsmedizinische Untersuchungen. By TORSTEN SJÖGREN. Lund, Berlingska Boktryckeriet, 1931. Pp. 197-425.

This monograph deals with a careful study of approximately one hundred cases of amaurotic idiocy in the institutions for the blind of Sweden. Characteristic changes in structure of the retina, progressive neurological symptoms (posture, gait, and speech), as well as more psychological patterns of behavior (dementia, negativism, and compulsions) are recorded in detail for each case from the onset to the termination of the disease in death. A differential diagnosis of the symptoms of amaurotic idiocy from other juvenile diseases (paresis, juvenile diffuse cerebral sclerosis) is made.

From a statistical study of the family histories the author has found no pathological evidence in the heredity differing from the normal, but he did find greater percentage of blood relationship among the parents of amaurotic idiots than among normal individuals. He concludes that amaurotic idiocy is a recessive monohybrid hereditary character.

A statistical report of the rate of appearance of the disease in Sweden during the last fifty years as well as its prevalence in different geographical areas is also included.

Sarah Lawrence College THEODORA M. ABEL

The Development of Learning in Young Children. By LOVISA C. WAGONER. McGraw-Hill, New York, 1933. Pp. xiv, 322.

This is a practical and very useful book on child psychology, mainly restricted to the first five years of life. The author stresses the knowledge of what may be expected of a child. She warns against the frequent interpretation of normal features of behavior as "indicating "a problem child." The first five chapters cover the general ground of learning in animals and the human species and end with "maturation versus learning." Control of the body in walking, manipulating, dressing, etc., is then described. Social relations and the elimination of undesirable actions are then made clear. Sensible advice is given on sex education beyond the early years of life. Speech development, learning to eat, to sleep, to eliminate from the bowels are well discussed. The last chapters make it very clear that adults cannot deal with children so as to help them to grow up, if they regard their duties with the child as a mere avocation or, worse, as a mere nuisance. The book is a good child psychology for junior college students or young mothers.

University of Missouri MAX F. MEYER

Jugendpsychologie der Gegenwart. By OTTO TUMLIRZ. 2nd ed., enlarged. Berlin, Junker & Dunnhaupt, 1933. Pp. 97.

This is a valuable review of the contemporary psychology of childhood and adolescence. There are numerous references, mostly to German literature. The following quotations indicate the point of view taken.

"The tendency now prevails to disregard the observation and collection of single traits. We do not review mental development in longitudinal cuts, studying the development of single abilities from early childhood to adulthood. On the contrary, we make horizontal cuts and sketch broadly the various *levels of development.* These levels are related to personality, to a whole which expresses itself in definite main characteristics." "The example of Meumann, the influence of educational interests on psychological research, the preference for a few special problems, as for example that of intelligence and intelligence testing or that of vocational aptitude and its measurement, have resulted in innumerable specialized investigations while at the same time the child himself, until quite recently, remained relatively unknown." Earlier tendencies are criticized.

University of Michigan THORLEIF G. HEGGE

The Dynamics of Therapy in a Controlled Relationship. By JESSIE TAFT. New York, The Macmillan Co., 1933. Pp. ix, 296.

This is largely narrative description of 47 hours the author spent with two 7-yr.-old "problem" children. The section dealing with Helen appeared in the *Psychoanalytic Review* of October, 1932. The 31 "treatment contacts" with John were intended as preparation for placement in a foster home. Psychological data are conspicuous by their absence. The philosophy and methodology of the author are admixtures of those of the psychoanalyst, the social worker, and the free-activity advocate in education.

University of Cincinnati MILTON B. JENSEN

Case Studies of Normal Adolescent Girls. By ELSIE M. SMITHIES. New York, D. Appleton & Co., 1933. Pp. iii, 284.

This is a book of case studies of high school girls who have had scholastic difficulties because of personality maladjustment. Each case is described in detail with regard to symptoms, environmental conditions, health, mental tests, treatment, and degree of satisfactory recovery. It is an excellent book for parents and teachers interested in typical problems of the adolescent and is full of wholesome remedial suggestions.

Wesleyan College, Macon, Ga. RALEIGH M. DRAKE

The Philosophy of Physical Realism. By ROY WOOD SELLARS. New York, The Macmillan Co., 1932. Pp. 481.

This book is a treatise on epistemology in which the author develops his theory of critical physical realism. There is little in the book of interest to the psychologist.

West Virginia University JOHN E. WINTER

Scepticism and Construction. By CHARLES A. CAMPBELL. New York, The Macmillan Co., 1931. Pp. xxiv, 322.

This essay is an attempt to revive Bradley's sceptical epistemology and extend it to support the phenomenological concept of reality or truth.

Hobart College FORREST L. DIMMICK

Common Sense and the Child. By ETHEL MANNIN. Philadelphia, J. B. Lippincott & Co., 1932. Pp. 314.

This is a plea for leaving the child alone. For parents to keep the child clean and tidy and to make it "behave nicely," is only to "do them credit" at the child's expense in freedom, initiative, and happiness. On the other hand allowing the child freedom, naturalness, and lack of restraint, except for physical dangers, results in normal sex life and emotions, greater honesty and directness in the activities of life, and greater happiness. The prescription is probably as good for the majority of American as for English children. The book is gauged for the average mother and for elementary school teachers. J. P.

Klinische Untersuchungen. By FRANZ KOELSCH. Handbuch der biologischen Arbeitsmethoden. Abt. IV, Angewandte chemische u. physikalische Methoden, Teil 16, Heft 2. Berlin, Urban & Schwarzenberg, 1932. Pp. 289-496.

A report of the clinical methods of determining occupational diseases due to physical, chemical, and biological defects. The use of the ergograph and dynamometer is indicated but no record of results is made. The article is of interest from the strictly medical point of view.

Sarah Lawrence College THEODORA M. ABEL

Peace of Mind and Body. By W. S. WALSH. New York, E. P. Dutton & Co., 1933. Pp. 249.

This book, written for the general reader, is concerned with the effects of suggestion upon mental and physical health. Such topics as distraction, fatigue, the influence of drugs, temperament, psychoanalysis, etc., are treated in simple language without marked loss of accuracy. The book may prove helpful to those who strive to follow its advice.

University of Vermont P. H. EWERT

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THE INFLUENCE OF AREA, POSITION, AND BRIGHTNESS IN THE VISUAL PERCEPTION OF A REVERSIBLE CONFIGURATION

By H. GOLDHAMER, University of Toronto

By no means a new problem, but certainly one that has acquired new emphasis in recent years, is the determination of the structuration of perceptual experience. The present paper deals with the influence of three factors, area, position, and brightness, in the visual perception of a reversible configuration. The form employed was a circle divided into six sectors, each alternate sector subtending the same angle at the centre. The form could be seen in two ways according to which set of alternate sectors was perceived as figure (see Fig. 1).

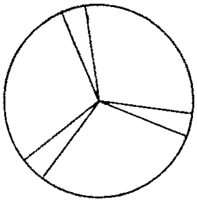
FIG. 1. THE CONFIGURATION EMPLOYED

In terms of this specific form the study here reported attempts to answer the following questions: (1) To what extent does the area of the sectors (or the distance separating their boundary lines) determine which set of sectors will be seen as figure? (2) To what extent are the results of (1) influenced by varying the position of the sectors within the circle? (3) To what extent are the results of (1) influenced by introducing a marked brightness difference in the alternate sectors?

HISTORICAL REVIEW

Doubtless the present problem could easily be characterized as a study in Gestalt psychology. The use of simple visual forms to determine the principles involved in the organization of perceptual experience has, however, had a history antedating the rise of the Gestalt movement. Hempstead, for instance, in 1900 published a study

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* Accepted for publication June 3, 1932. This study was carried out under the direction of Professor W. Line, to whom grateful acknowledgment is made for his assistance.

in which she made use of simple forms tachistoscopically exposed in order to investigate the perception of visual form. She concluded: "In looking at forms liminally different from their background we are likely to continue lines and to complete figures under the guidance of the two principles of symmetry and similarity."[1] The resemblance of certain of Hempstead's phrases to the Gestalt concepts of "figure-ground," "Prägnanz," "closure," and "law of similarity" is not without interest to the historian. It remains true, nevertheless, that prior to the Gestalt movement there was a distinct tendency to account for the organization of experience by means of some special act. Thus we have the "creative synthesis" of Wundt, the "determining tendencies" of the Würzburg school, the "producing process" of Benussi and other members of the Graz school. Mach and Von Ehrenfels struck a note of originality when they denied the necessity of an act to account for organization and spoke of form as itself being an independent sensory element (the *Gestaltqualität*).[2] From this point on to the emergence of *Gestalttheorie* there was an increasing tendency to discountenance the part played by "attention," "meaning," etc., and to investigate in their stead the objective conditions in the stimulus-object that might determine the perception of form. Köhler, however, has recognized that "the fact in itself that the change of subjective attitude towards the field can to a certain degree alter its properties, units, etc., must be regarded as being a very interesting problem. As yet it has not been studied thoroughly enough."[3]

Among the Gestalt psychologists Wertheimer has been the most prolific in the enunciation of formal principles; nor is this surprising in view of his stress on "inner necessity." In his now famous 1912 article,[4] he was already investigating changes in the experience of movement resulting from variation in the objective conditions of the stimulus-object. In the so-called Gestalt "declaration of independence," he trenchantly expresses the theoretical supposition underlying his investigations of the "laws" of the Gestalt; our experiences are determined, he writes "bei dem Erfassen eines inneren Zusammenhangs, dem Spüren einer inneren Notwendigkeit . . . wo das Geschehende nicht durch blinde *äussere Faktoren* sich bestimmt, sondern durch sachliche *'innere Gefordertheit.'* "[5]

In 1923 followed an article in which the principal determining conditions (the forms of "inner necessity") were listed together with numerous illustrative figures.[6] Only those principles investigated in this study need be considered here.

In this study the factor that, following Graham[7], has been termed "area" (as being more appropriate to the figure employed) is called by Wertheimer the "factor of nearness." "Die Zusammengefasstheit resultiert—ceteris paribus—*im Sinn des kleinen Abstandes (Faktor der Nähe)*."[8]

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[1] L. Hempstead, The perception of visual form, this JOURNAL, 12, 1900, 192.

[2] Cf. I. M. Bentley, Psychology of mental arrangement, *ibid.*, 13, 1902, 269-293.

[3] W. Köhler, An aspect of Gestalt Psychology, *Psychologies of 1925*, 1926, 177.

[4] M. Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Zsch. f. Psychol.*, 51, 1912, 161.

[5] Wertheimer, Untersuchungen zur Lehre von der Gestalt, *Psychol. Forsch.*, 1, 1922, 57.

[6] Wertheimer, *idem, ibid.*, 4, 1923, 300-350.

[7] C. H. Graham, Area, color, and brightness difference in a reversible configuration, *J. Gen. Psychol.*, 2, 1929, 470-481.

[8] Wertheimer, *op. cit.*, 1923, 308.

According to this principle, when we have forms in which all other factors are held constant, the figure that will be perceived will be that which is formed by the parts in closest proximity. Thus in Fig. 2 one usually perceives ab/cd/ef, and not a/bc/de/f. Although such an example may appear almost ludicrously simple, it must not, says Wertheimer, be passed over with a "smug assertion of self-evidence." A reference to Wertheimer's article will convince the most skeptical that there are numerous complicating factors that enter into the demonstration of even such an apparently simple principle as that of "nearness." Although Wertheimer's article may now be taken as the classical exposition of the internal laws of the Gestalt, the argument is largely empirical; no specific experimental results are quoted.

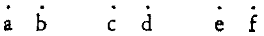

FIG. 2. ILLUSTRATING THE FACTOR OF NEARNESS

The work of Rubin in the field of visual perception,[9] although done for the most part before Gestalt psychology became an articulated movement, has been largely incorporated in Gestalt literature. Rubin's investigation was particularly directed toward an experimental elucidation of the figure-ground phenomenon; with this aspect of his work we are not now concerned. During the course of his experimentation, however, Rubin employed a circle form that is the same as the one used in the present study with the exception that it had eight instead of six sectors. Rubin did not attempt to isolate any of the factors later mentioned by Wertheimer; his general conclusion involves but two factors, area and colour. He says, "One can advance the following, fundamental law: If of two homogeneous, differently coloured fields the one is noticeably larger than the other and surrounds the other, then there is an overwhelming probability that the small, inclosed field is perceived as figure."[10]

Wever has published two articles dealing with the figure-ground phenomenon;[11] like Rubin he was particularly interested in discovering the distinguishing properties of figure and ground. He found that "one of the factors in equivocality is equality of the areas of the two stimulus fields."[12]

Much has been written by Köhler on the principles of the Gestalt, the emphasis being rather on the theory underlying Wertheimer's principles. Although Köhler has apparently worked with the form used in the present study, no specific references to experimental studies could be found. He writes,

> "In (the form) we are confronted by a pattern formed by three narrow sectors. Looking at the center of it, most persons will suddenly see another pattern after a while. In the new shape those lines, which belong together in the first pattern as contours of one arm, belong to separate arms and vice versa. The organization is changed. By appropriate experimentation, making our subject fixate the center for a long while, we can reduce the time for either shape to a minimum value, so that the alternations follow each other very quickly. If now the figure is turned around a little, so that the arms have another position, the figure becomes as stable again as it was at first, and only after some time the alternation will quicken, as in the first position."[13]

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[9] E. Rubin, *Visuell wahrgenommene Figuren*, 1921, 79.

[10] *Ibid.*, 79. The translation of the excerpt is taken from Graham (*op. cit.*, 470).

[11] E. G. Wever, Figure and ground in the visual perception of form, this JOURNAL, 38, 1927, 194-226; Attention and clearness in the perception of figure and ground, *ibid.*, 40, 1928, 51-74.

[12] *Op. cit.*, 1928, 55.

[13] Köhler, *Gestalt Psychology*, 1929, 186.

Although Köhler is here more interested in the phenomenon of fluctuation than in the factor of area, the passage is of some importance as references to positional variation of such figures are not abundant.

Graham, using a circle form with six sectors, investigated the influence of area, color, and brightness. His method differed considerably from that employed in the present study. His circles, 15 cm. in diam., pasted on a neutral gray background, were hung on a wall 230 cm. from the *O*. From this distance *O* fixated the center of the circle for 16 periods of 30 sec. each; during fixation *O* pressed a telegraph key when the small sectors were seen as figure and released the key when the large sectors were figure. The results were recorded on a kymograph. In investigating the influence of area, Graham varied the angles subtended by the alternate sectors. Beginning with a circle whose alternate sectors were 10° and 110°, he increased the angle of the small sectors by intervals of 10° until the alternate sectors were equal (each 60°). Brightness was investigated by presenting a series in which the alternate sectors were colored blue and red, the red sectors being the brighter. The influence of color was determined by presenting other series in which the alternate sectors were blue and white, blue and red, red and green. Although Graham makes no statement regarding the position of the sectors when they were presented to his *O*s, the illustration accompanying the article shows one of the small sectors perpendicular to the base of the form. Graham's conclusions were:

> "Our results show that in this type of configuration (1) the system of smaller fields is chosen as 'figure,' and a relative decrease in the size of these fields makes them more dominant, *i.e.,* they persist as 'figure' longer in reversals. Dominance varies inversely in a liminal manner with area. When the systems are equal in area, the configuration is most equivocal. (2) When 'figure' and 'ground' are of equal brightness, area is a most important factor in the perception. This seems to be the case, regardless of color differences. (3) When 'figure' and 'ground' are not of equal brightness the influence of area is greatly affected, and, if the brightness differences are great enough, the tendency is for the brighter system to dominate."[14]

Graham's results for the series in which area was the variable were quite consistent for his 3 *O*s. Measuring dominance in terms of the percentage of the total time that a figure was perceived, he found that the smallest sectors (10°) were seen as figure about 73% of the time and the sectors of 60° (all sectors equal) were seen 50% of the time, the time the small sectors were seen as figure decreasing with fairly even gradations from 73% to 50% as the small sectors were increased in size. The results for the brightness series indicated a well-marked dominance of the red bright sectors over the blue less bright sectors. In view of the fact that Graham has here introduced a color difference as well as a brightness difference, and especially since his own results show that when brightness is equated red tends to be more dominant than blue, his results for the brightness series may be open to question. It may also be noted that Graham's results for the brightness variable are only based on the reports of 2 *O*s for only two different forms (30° and 60°).

In summary, it may be seen that among recent investigators there is a fairly definite conviction that the variation of area in the stimulus-object does result in a variation in the perception of figure, with opinion unanimous that the smaller or more closely circumscribed unit (or units) of a configuration is usually seen as figure (all other factors, of course, being constant). The influence of brightness does not appear to be quite as definitely known and is more open to question. The influence of positional variations in a reversible configuration of the type used in this study also appears to have received less attention on the part of experimenters than such factors as area, similarity, closure, etc. Although Wertheimer has worked with such variables as area, etc., their range of application is still undetermined; for, although Wertheimer himself has formulated a number of "laws" to express their operation, he does not hesitate to warn his readers that experimentation would have to be performed with a greater diversity of figures to permit the formulation of laws that would have a wider application and significance than can now be claimed for them.

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[14] *Op. cit.,* 481.

METHOD AND PROCEDURE

Apparatus. The exposure apparatus was a modification of the Dodge tachistoscope.[15] It comprised two wings, one for the prefixation-field and one for the fixation-field. The former was illuminated by 6 and the latter by 2 12-volt miniature bulbs. As the prefixation-field was seen indirectly by means of a mirror arrangement, a larger number of bulbs was required to secure illumination equal to that of the fixation-field. Equality of illumination was only subjectively estimated. The fixation- and prefixation-fields were each supplied with a slot from which a circle 5 cm. in diam. had been cut. The forms were placed behind the slot of the fixation-field and held tightly against it by means of a strong spring; they were seen at a distance of 38 cm. from the *O*'s eyes. The tachistoscope could be raised or lowered to the height most suitable to *O*.

Illumination of the fixation- and prefixation-fields was secured by means of 3 points making contact with the brass drum of a spring kymograph. By means of 2 sets of slots in the insulating paper covering the drum, the 2 fields could be illuminated for the required time, the prefixation-field being darkened synchronously with the lighting of the fixation-field. The third contact point and a third set of slots illuminated a small warning light 2 sec. before the fixation-field came into view. Eight 1.5-volts dry-cell batteries wired in series provided the necessary electrical energy.

Materials. In the investigation of the three factors of area, position, and brightness five different series of the circular form shown in Fig. 1 were used.

Series 1. This comprised a set of 7 circles, each 5 cm. in diam. The variable was the area of the sectors as measured in terms of the angle subtended at the centre. The alternate sectors were equal. The angles of the sectors for each circle were as follows:

Circle	*Small Sector*	*Large Sector*	*Circle*	*Small Sector*	*Large Sector*
1	10°	110°	5	50°	70°
2	20°	100°	6	55°	65°
3	30°	90°	7	60°	60°
4	40°	80°			

It will be noted that small sectors are increased by 10° each time except for the sixth and seventh circles where the increase was reduced to 5°, preliminary experimentation having indicated that finer gradations were required as the sectors approached equality. In Series 1 the circles were so drawn that the lowermost of the small sectors deviated 45° to the left of a perpendicular drawn from the center of the circle to its base (see Fig. 3). For convenience in designation this will hereafter be called the "indifferent position."

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[15] R. Dodge, An improved exposure apparatus, *Psychol. Bull.,* 4, 1907, 10, 13.

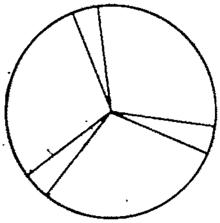

Fig. 3. Position of Sectors in Series 1, 4, and 5

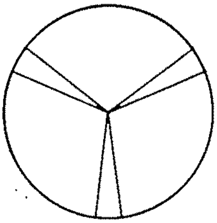

Fig. 4. Position of Sectors in Series 2

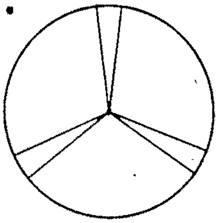

Fig. 5. Position of Sectors in Series 3

Series 2. This was the same as Series 1 with the exception that the lowermost of the small sectors in each circle was drawn from the center perpendicular to the base of the circle, the large sectors being perpendicular to the top of the circle (see Fig. 4).

Series 3. This series is the complement of Series 2, the small sectors being drawn perpendicularly from the center of the circle to the top of it, the large sectors being perpendicular to the base (see Fig. 5). As the 60° circles for Series 2 and 3 would be identical, this form was omitted from Series 3.

Series 4. This series and the succeeding Series 5 were designed to investigate the influence of a marked brightness difference in the alternate sectors on the perception of the figures. Graham, as we have already seen, investigated brightness by having the alternate sectors blue and red, the red sectors being the brighter. This method, however, introduces, in addition to the brightness difference, a color difference. If color were to be introduced at all it would appear preferable to use two brightness values of the same color. Since, however, Graham found that color exerts some influence in the perception of the figures in the circle form, it appeared preferable to vary brightness in an achromatic rather than in a chromatic scale. Hence in Series 4 black and white were used for the alternate sectors, the small sectors being white and the large sectors black. In all other respects this series is identical with Series 1. It is not to be supposed, however, that the use of black and white is free from difficulties. As Graham, himself, states, "equivocal figures presenting black and white areas would not seem to be good, for the subject's past experience with printed material, etc., would create a set for the black as 'figure'."[16] Black and white were used, nevertheless, for two reasons: (1) if a chromatic scale were used an unknown factor would still be entering into the experiment, whereas the use of black and white would at least enable one to determine whether results for achromatic brightness difference were different from results obtained with a chromatic scale; and (2) it seemed desirable to discover whether a marked brightness difference such as black and white would give results that were the same as, or different from, results obtained from the blue-white form used by Graham.

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[16] Graham, *op. cit.*, 478 f.

Series 5. This was the complement of Series 4, the large sectors being white and the small sectors black.

All the forms were drawn on white filing cards. Only the boundary lines demarcating the sectors were drawn, the contour of the circle being formed by the fixation-field diaphragm from which a circle 5 cm. in diameter had been cut. The forms were so drawn that when inserted behind the diaphragm the point where the apices of the sectors met fell exactly at the center of the circular perforation. The lines bounding the sectors were drawn with India ink at a constant width of 0.4 mm. In Series 4 and 5 in which the alternate sectors were white and black, the black sectors were filled in with India ink.

Observers. Three graduate students in psychology, S. S. Freedman (*F*), R. S. Jaques (*J*), and D. J. Wilson (*W*), served as the *O*s.

Experimental procedure for Series 1, 2, and 3. The 20 cards comprising these three series were presented together. As the diaphragm that formed the contour of the circles was painted black, the form stood out very clearly. The background for the prefixation-field was likewise black. During the prefixation period *O* saw a white circle with a dot in the center; when the fixation-field was illuminated the prefixation-field immediately gave way to a white circular figure with six sectors. The exposure-time for the form was 1 sec. As the preliminary experimentation showed that the perception of 3-sector figures had a temporal course, it was necessary to select an exposure time that would (1) be long enough to give adequate perceptual conditions and permit accurate reporting from the *O*s, and (2) be short enough to prevent reversal of the figures. Between successive presentation 15 sec. elapsed, during which *O* gave his report which was recorded by *E*. In order to facilitate reporting, a duplicate set of 20 cards was drawn. On each of these cards were drawn two circles with sectors corresponding to the cards presented in the tachistoscope. In one circle the big sectors and in the other circle the small sectors were shaded. These two circles were labelled "A" and "B." After each exposure of a form the corresponding checking card was presented to *O* and he reported "A" or "B" according to which set of three sectors he perceived. To prevent *O* from forming any habits of reporting "A" or "B" in a majority of the cases, the circles on the check card with small sectors were in half the cases labelled "A" and in the other half "B," with the opposite procedure for the circles with the large sectors. The instructions given the *O*s were as follows:

> You are going to be shown tachistoscopically a number of circular figures. You are to fixate the dot in the center of the field. About 2 sec. before the field changes and the form appears, I shall say "ready." After seeing the form you will find a card in front of you on the table, on which are drawn two forms, "A" and "B," each form representing one way in which the form presented in the tachistoscope can be seen. If figure "A" on the check card represents what you saw, say "A"; if figure "B" represents what you saw, say "B." If you should experience a reversal of figures during the exposure time report on the basis of the first figure that you saw. Remember that in using the check cards you have two clues in identifying the figure that you saw; namely, the position of the sectors and their size. In one or two of the cards where the sectors appear to approach equality your only clue may be the position of the sectors. After making your report you will refixate the dot and await the next "ready" signal.

The 15-sec. interval between exposure was ample as none of the *O*s was pressed for time; from 6 to 8 sec. elapsed between their report and the next "ready" signal.

Before the experiment proper began each *O* was given about an hour and twenty minutes of training. In the training period *O* made 8 reports on each of the 20 forms (160 exposures). After the series of 20 forms had been presented three or four times the *Os* reported clarity in the perception of the figures and found no difficulty in giving a report of "A" or "B." At times when the 60-60 form was presented only two of the three sectors of a figure would be clearly perceived, but there was no doubt as to which figure the sectors belonged. Graham noted the same phenomenon in his experiment, although here the cause was the prolonged fixation and apparently occurred with several of his forms.[17]

In order to verify the accuracy of the reports, a form was at times left in the tachistoscope and after the *O* had made his report he was referred back to the form on which he had reported, and asked to compare it with the check card to see if any error in designating the figure had been made. No errors of this type were found. In order to discover if the *Os* remembered whether their previous judgments of particular forms had been "A" or "B," they were asked to make reports of "A" or "B" without referring to the check card. Although there were only two possible reports, none of the *Os* scored 50% correct memories. This, of course, did not preclude the possibility of the *Os* remembering whether in a particular form the majority of their reports had been in favor of the small or large sectors. In the extreme cases, such as those where the small sectors were 10°, 20°, and 30°, there was no doubt that they did remember which set of sectors they had been seeing as figure. The possible influence of this memory factor on the results obtained can, however, be more advantageously discussed when the results are dealt with.

In the experiment the set of 20 forms was presented 25 times. In order to guard against fatigue the 25 presentations of the set were divided into 3 sittings. Another safeguard against fatigue was the fact that after the set of 20 forms had been exposed once, there was an interval during which *E* shuffled the cards into a chance order, arranged the checking cards in the same order and rewound the spring kymograph. A longer rest interval was given at the end of each half hour. None of the *Os* reported fatigue or discomfort.

Experimental procedure for Series 4 and 5. As the alternate sectors in these series were black and white it was necessary to have a neutral background, *i.e.* a background midway between black and white. In lieu of the more exact photometric measure, the required gray of the background was determined by equating it subjectively with the midpoint of the Munsell achromatic scale, a standard system employed by artists. The background of the prefixation-field was also painted with the same value of gray.

The *Os*' manner of reporting for this set of 14 forms was much simpler than that employed with the earlier series. They were instructed to report verbally "black" if the black sectors were seen as figure, "white" if the white sectors were seen as figure.

The exposure-time for these two series was reduced to 3/5 sec. as it was found that with the black and white forms the figures were seen more quickly and exposures of 1 sec. permitted reversals. As the *Os* did not require checking cards for these series, and as this decreased the amount of time required by *E* between each exposure, there was only an interval of about 7 sec. between successive presentations of the forms.

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[17] *Op. cit.*, 473.

Before beginning the new black and white series the *O*s were given another training period, but one much shorter than that required for the earlier series.

As in the previous series, the forms for Series 4 and 5 were each presented 25 times. Throughout the entire experiment each *O* made reports on 650 exposures exclusive of training periods.

RESULTS

Series 1. (Small sectors in the indifferent position; see Table I). In all cases where there was a difference in the area of the alternate sectors the small sectors were seen most often as figure. Up to and including the 40-80 form, *W* gave 100% reports in favor of the small-sector figures; for *F* and *J*, 100% reports extended to the 30-90 form. In the remaining forms (excluding from consideration the 60-60 equality form) we find that for all the *O*s an increase in the size of the small sectors is accompanied by a decrease in the number of "small" reports. This tendency was least marked in *W* and most marked in *F*. Despite the decrease in the number of "small" reports, the "small" and "large" reports did not become equal as the sectors approached equality. The increase in "large" reports was not evenly

TABLE I

RESULTS FOR SERIES 1: SMALL SECTORS IN THE INDIFFERENT POSITION

(Fig. 3)

O	Angles	Number		Percentage	
		Small	Large	Small	Large
W	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
	40– 80	25	0	100	0
	50– 70	22	3	88	12
	55– 65	21	4	84	16
	*60– 60	23	2	92	8
F	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
	40– 80	23	2	92	8
	30– 70	16	9	64	36
	55– 65	14	11	56	44
	*60– 60	17	8	68	32
I	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
	40– 80	22	3	88	12
	50– 70	18	7	72	28
	55– 65	17	8	68	32
	*60– 60	2	23	8	92

* Note that the categories "small" and "large" do not apply to the 60–60 form,

gradated; for all 3 *O*s the greatest single increase in "large" reports occurred between the 40-80 and 50-70 forms. The 60-60 form requires separate comment. Although one might expect that the "small" and "large" reports would approach nearer to equality in this form, this did not occur. *W* and *F* saw one set of alternate sectors in this form 88% and 68% of the times respectively; *J* saw the other set of alternate sectors 92% of the times.

Series 2. (Small sector perpendicular to the base, large sector perpendicular to the top; see Table II). With the small sector perpendicular to the base we find a marked difference in the results. For *W* and *F* there was a decided increase in the number of "small" reports, *W* making 100% reports for all forms in the series in favor of the small-sector figures, and

TABLE II

RESULTS FOR SERIES 2: SMALL SECTORS PERPENDICULAR TO THE BASE; LARGE SECTORS TO THE TOP

(Fig. 4)

O	Angles	Number		Percentage	
		Small	Large	Small	Large
W	10–110	25	0	100	0
	20–100	25	0	100	0
	30–90	25	0	100	0
	40–80	25	0	100	0
	50–70	25	0	100	0
	55–65	25	0	100	0
	60–60	25	0	100	0
F	10–110	25	0	100	0
	20–100	25	0	100	0
	30–90	25	0	100	0
	40–80	25	0	100	0
	50–70	24	1	96	4
	55–65	22	3	88	12
	60–60	21	4	84	16
J	10–110	25	0	100	0
	20–100	25	0	100	0
	30–90	23	2	92	8
	40–80	15	10	60	40
	50–70	10	15	40	60
	55–65	10	15	40	60
	60–60	7	18	28	72

F making only 4 "large" reports (exclusive of the 60-60 form) as compared with 22 "large" reports in the previous series. In the 60-60 form *W* reported the figure with sector perpendicular to the base 100% of the times, *F* 84% of the times. We note, then, that with the small sectors perpendicular to the base, there is a marked increase in the number of "small" reports from *W* and *F*. *J*'s results also show a decided difference when compared with his results for Series 1, but a difference, the direction of which is exactly the reverse of that exhibited by *W* and *F*. In this series *J* made 42 "large" reports (exclusive of the 60-60 form) as compared with 18 such reports for the previous series; both in the 50-70 and 55-65 forms the large-sector figures[*] (perpendicular to the top) were dominant (*i.e.* seen more often than the small-sector figures). In the 60-60 form *J's* results are also the reverse of the other two observers, 72% of the reports being in favour of the figure with sector perpendicular to the top. For *W* and *F*, therefore, perpendicularity of a figure to the base adds to the dominance of that figure;[18] for *J* perpendicularity of a figure to the top adds to the dominance of that figure.

TABLE III

RESULTS FOR SERIES 3: SMALL SECTOR PERPENDICULAR TO TOP OF FORM; LARGE SECTOR TO BASE

(Fig. 5)

O	Angles	Number		Percentage	
		Small	Large	Small	Large
	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
W	40– 80	25	0	100	0
	50– 70	24	1	96	4
	55– 65	7	18	28	72
	60– 60	0	25	0	25
	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
F	40– 80	21	4	84	16
	50– 70	12	13	48	52
	55– 65	8	17	32	68
	60– 60	4	21	16	84
	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
J	40– 80	24	1	96	4
	50– 70	24	1	96	4
	55– 65	23	2	92	8
	60– 60	18	7	72	28

Series 3. (Small sector perpendicular to top, large sector perpendicular to the base; see Table III). Since the forms in this series are simply the reverse of those in Series 2 and serve as a corroborative series, we should expect on the basis of the results already considered that for *W* and *F* there

would be a marked increase in the number of "large" reports (since for them perpendicularity of a figure to the base adds to its dominance), and that for *J* there would be an increase in the number of "small" reports (since for him perpendicularity to the top adds to a figure's dominance); a reference to Table III will show that such are the actual results. In *W's* results we find that up to and including the 50-70 form the small-sector figures (at top of form) were still seen in almost 100% of the trials, but that when the sectors are 55-65 the large-sector figures (at base) were seen in 72% of the trials as compared with 16% for Series I and 0% for Series 2. In the 60-60 form the figure with the sector at the base was seen in 100% of the trials. (This result is taken from Series 2, the 60-80 form being identical for both series.) *F*'s results are substantially the same as *W*'s with the exception that for the former large-sector figures (at the base) became dominant with the 50-70 form instead of with the 55-65 form. *J*'s results are again (as they should be) opposite to those of *W* and *F*. For him the small-sector figures when perpendicular at the top were increased in dominance. Referring to Table II it will be noted that for *J*, as for *F*, the large sector figures become dominant with the 50-70 form when the factor of perpendicularity is working against the small-sector figures, whereas the turning point for *W*, as we noted above, was the 55-65 form. This is consistent with, and would appear to corroborate, the indication in the results of Series 1 that the small-sector figures have a greater dominance for *W* than for *F* and *J*.

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[*] When a figure is spoken of as being perpendicular to the base or top of a form, it is to be understood, of course, that only one sector of the figure is perpendicular.

Summarizing the results of Series 1, 2, and 3 we may say: when the small-sector figures were in the indifferent position all *O*s reported a majority of small-sector figures seen, the number of such reports decreasing as the alternate sectors approached equality. When a figure was perpendicular to the base its dominance was considerably increased for *W* and *F*, even large-sector figures in this position becoming dominant when the area difference was not too great. For *J*, however, perpendicularity of a figure to the top of the form increased the dominance of that figure, large-sector figures in this position becoming dominant when the angular difference of the alternate sectors was 20° or less.

Series 4. (Sectors in the indifferent position, small sectors white, large sectors black; see Table IV). If we compare the results of Series 1 (Table I) with those of Series 4 we find that when the large sectors are black the influence of area is markedly decreased, the large sectors when black becoming dominant over the small-sector figures for *W* when the angular difference of the alternate sectors is reduced to 10°, and becoming dominant for *F* and *J* when the angular difference is 40° or less. This individ-

TABLE IV

RESULTS FOR SERIES 4: SMALL SECTORS IN THE INDIFFERENT POSITION; SMALL SECTORS WHITE; LARGE SECTORS BLACK

(Fig. 3)

O	Angles	Number		Percentage	
		Small	Large	Small	Large
W	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
	40– 80	25	0	100	0
	50– 70	25	0	100	0
	55– 65	8	17	32	68
	60– 60	0	25	0	100
F	10–110	25	0	100	0
	20–100	24	1	96	4
	30– 90	16	9	64	36
	40– 80	5	20	20	80
	50– 70	0	25	0	100
	55– 65	0	25	0	100
	60– 60	0	25	0	100
	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	24	1	96	4
	40– 80	12	13	48	52
	50– 70	2	23	8	92
	55– 65	1	24	4	96
	60– 60	1	24	4	96

ual difference is again consistent with the results of Series 1, 2, and 3 where it was found that the small-sector figures have a greater degree of dominance for *W* than for *F* and *J*.

Series 5. (Sectors in the indifferent position, small sectors black, large sectors white; see Table V). Since we found in Series 4 that blackness added to the dominance of a figure for all 3 *O*s, we should expect in this series, in which the small-sector figures are black, that the dominance of the small-sector figures would be greater than in Series 1 in which both sectors were white; and these are the results that we actually do find. *F* and *J* did not report a single large-sector figure throughout the whole series; *W* also gave 100% report in favor of the small-sector figures up to the 60-60 form where the black figure, although still dominant, was only reported 60% of the times. This series, then, corroborates in a very striking manner, the tendency found in Series 4 for black to add to the dominance of a figure.

Before leaving these results some comment on their reliability should be made. In the previous section, we dealt with certain checks that were used to ensure accuracy in the *O*s' reports and to insure that memories of "A" and "B" reports did not influence the results. The results of these checks were entirely satisfactory. The question was raised, however, to what extent would the *O*s remember that they had been seeing a certain figure in a specific form, and how would this memory factor influence the results? When the angular difference of the alternate sectors was pronounced, and 100% (or slightly less) reports were made in favor of a certain figure, there was no doubt that the *O*s were aware of the consistency of their reports in favor of that figure. That this memory factor did not influence the results (at least to any significant degree) can be substantiated, in the writer's opinion, by the fact that in forms for which there was a very great preponderance of reports in favor of a certain figure, the appearance of the second figure on rare occasions was so startling, so strong in its effect and so changed the appearance of the form, that the *O*s spontaneously remarked on the change in their perception. The writer would contend, therefore, that since there is such a striking difference in perception between the two figures of a form, the effect of memory in creating a "set" in favor of the figure most often perceived would be negligible.

TABLE V

Results for Series 5: Small Sectors in the Indifferent Position; Small Sectors Black, Large Sectors White

(Fig. 3)

O	Angles	Number		Percentage	
		Small	Large	Small	Large
W	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
	40– 80	25	0	100	0
	50– 70	25	0	100	0
	55– 65	25	0	100	0
	60– 60	15	10	60	40
F	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
	40– 80	25	0	100	0
	50– 70	25	0	100	0
	55– 65	25	0	100	0
	60– 60	25	0	100	0
J	10–110	25	0	100	0
	20–100	25	0	100	0
	30– 90	25	0	100	0
	40– 80	25	0	100	0
	50– 70	25	0	100	0
	55– 65	25	0	100	0
	60– 60	25	0	100	0

Although the forms were presented in a chance order and were shuffled after each set had been presented, there may still be a possibility that the order of presentation affected the results. As it was scarcely feasible to arrange beforehand the order of presentation of the 650 exposures for each *O*, there is no certain means of checking the possible influence of this factor. The raw data of the experiment, however, give certain indications that the order of presentation was not a disturbing factor. The data for the three sittings during which the forms for Series 1, 2, and 3 were presented together show consistent results for every *O* for each sitting. Since there was a chance order of presentation it is scarcely possible that the first card presented at each sitting, or that the subsequent order of presentation, was the same. Furthermore, we find that the *O*s exhibit the same trends and that these trends are so well marked that the order of presentation could only have caused minor divergencies.

How do the results of this experiment agree with those of other investigations? The general tendency reported by Wertheimer, Rubin, Graham, and others for the small areas to be seen as figure is further substantiated by the results of the present study. It has not been found possible to compare the influence of perpendicularity found in Series 2 and 3 with any other studies; the results, however, appear unequivocal enough. The results for the brightness series differ from those of Graham; he found that the bright, red sectors were seen more often than the darker, blue ones. Using an achromatic scale the present study showed the darker set of sectors to be more dominant. This may possibly substantiate Graham's claim that black creates a special "set" due to past experience with printed matter. It should be noted, however, that although Graham's results with blue and white sectors were not entirely satisfactory, his most reliable and consistent *O* gave reports indicating blue-sector figures to be dominant over white-sector figures; it may be, then, that the dominance of the blue sectors in Graham's study is not due to a color difference, nor the dominance of black sectors in this experiment due to a brightness difference, but that both are due to a specific white and not-white difference in which the not-white figure usually dominates. This, again, may possibly be interpreted in terms of past experience in which white may be said to be our customary ground. Nor is it possible, with the limited amount of data at hand, to conclude that brightness differences in an achromatic scale will always give results that are opposite to those obtained from brightness differences in chromatic scales. The question must remain open for further experimentation.

If one examines the tables for this and Graham's experiment, a striking difference will at once be observed. Graham, because he made use of a fixation and measured dominance in terms of the amount of time that a figure was seen during fixation, naturally found no forms for which a single figure was dominant throughout the entire fixation period; thus with the 10-110 form he found the small-sector figures dominant only about 73% of the time. Furthermore, as the small sectors were increased in size their dominance decreased with fairly even gradations. In the present experiment, in which each form was presented each time as a discrete perception, several forms in the same series often gave 100% results in favour of a certain figure. In contrast with the evenly gradated results of Graham, the results indicate certain critical stages in a series at which abrupt transitions from one figure to another take place, or at which there is a sudden decrease in the number of reports in favour of a figure, although that figure may still remain dominant. It seems fairly certain that these differences in results are due to differences in the methods employed. Which method is best it would be difficult to say. The use of a fixation may have the advantage that it measures finer differences between figures. Finer gradations and the absence of complete dominance of a single figure, may, however, be due to the fact that fixation exaggerates fluctuation from one figure to another in a single form. The method of the present experiment more closely approximates normal perceptual conditions and hence the results obtained might, perhaps, be expected to give a truer picture of the influence of the variable investigated than would results secured by the fixation method.

CONCLUSIONS

Since the scope of the various "laws" that determine the perception of figure is relatively unknown, no attempt has been made in this section to draw conclusions that will be applicable to all forms that might be constructed. Only when the many possible kinds of visual forms have been systematically reduced to a limited number of main types, and these have been subject to experimentation, will it be possible to determine the extent to which we are justified in generalizing our conclusions. Experimentation of this nature will certainly have to be undertaken if the laws, principles, factors, etc., enunciated by various investigators are to have any general validity. For the present we content ourselves with the more sober policy of drawing our conclusions in terms of the specific form used in this experiment.

Since in Series 1 the only variable was the area of the sectors in the form, and since the small sectors were seen as figure in the great majority of the times with a corresponding decrease in such reports as the alternate sectors approach equality, we conclude (1) that area is highly effective factor in determining the perception of figure as figures with small areas are on the whole dominant over figures with larger areas—dominance decreasing as difference in area decreases.

When, however, a special positional factor, perpendicularity of the small- and large-sector figures was introduced, it was found that perpendicularity of a figure to the base greatly increased the likelihood that it would be seen as figure (*W* and *F*), and also that perpendicularity of a figure to the top of the form also increased the likelihood that it would be seen as figure (*J*). Since but 3 *O*s were used we would only tentatively suggest that there may be an "individual difference" in the type of perpendicularity that renders a figure more likely to be perceived. The results, however, are decisive enough to permit the conclusion (2) that perpendicularity of a figure markedly increases the likelihood that that figure will be perceived.

Since in Series 2 for *J*, and Series 3 for *W* and *F* the small sectors were seen as figure up to, but not including, the 50-70 form (*F* and *J*) and 55-65 form (*W*), although the factor of perpendicularity was working against the small-sector figures, and it was not until the alternate sectors approached equality that the factor of perpendicularity became dominant over that of area, it follows (3) that the factor of area exerts a stronger influence than the factor of perpendicularity, as it is only when area-differences are minimized that perpendicularity becomes dominant.

Since in Series 4 and 5 the introduction of black in alternate sectors markedly increased the dominance of the black sectors, these series point to the conclusion (4) that in an achromatic form the introduction of a black-white difference adds to the dominance of the figure that is black. In view of the complicating factors already discussed (*vide supra* 203), this conclusion may be subject to revision with further experimentation.

Since in Series 4 the large, black-sector figures did not become dominant until the angular difference of the alternate sectors had been decreased, it follows (5) that the factor of area exerts a stronger influence than the factor of least brightness, and that only when area differences are minimized does the factor of least brightness become dominant.

The results of this experiment have indicated certain problems that require further experimentation. In view of the marked influence of perpendicularity it would seem highly profitable to inquire more closely into the exact nature of this factor. Was, for instance, perpendicularity of a sector so influential in perception because the sector was perpendicular to the base of the form, or because the sector was perpendicular to the horizontal of the eyes, or because the sector was parallel with the perpendicularity of the bodily posture? What would result if instead of rotating the sectors of the form, the *O*s themselves were rotated?

During the course of the experiment certain individual differences came to light; these would bear further investigation. Did the factor of area have a greater degree of dominance for *W* than for *F* and *J* because of a perseverative factor? Would the following passage from Stevens explain the fact that for *W* and *F* perpendicularity of a sector to the base, and for *J* perpendicularity to the top of the form, increased dominance?

"It will be shown that the eyes of many persons are normally adjusted much above the horizontal plane, that a deviation of 8° or 10° of arc above the horizontal plane is not very unusual and that there is another large class of persons in whom the deviation of the plane of normal direction of the eyes is materially below the horizontal plane."[19]

As already indicated, the influence of brightness requires investigation to determine whether it is an effective factor in perception *per se,* or whether brightness differences in chromatic and achromatic scales give conflicting results; or, again, whether a brightness difference is just a special case of a more general contrast factor.

Experiments of the present type may also carry wider implications. If it is impossible to generalize one's conclusions from the results based on a specific form, it becomes a primary necessity to reduce visual forms to a restricted number of main types. With these to experiment with, the possible variables should be studied that may determine the perception of form, experimenting with these variables both singly (where possible) and in more and more complex combinations. In this way it might be possible to determine more fully than we now can the factors entering into the perception of the complex visual fields of every-day life, rather than being restricted to the demonstration of the perception of form in the highly simplified figures of the text-book type.

SUMMARY

The purpose of the study was to determine the influence of area, position, and brightness in the visual perception of a reversible configuration, a circle form with six sectors in which the alternate sectors were equal, permitting two figures to be seen. Area was investigated by varying the angles of the sectors, position by varying the position of the sectors in the circle, and brightness by introducing black into the alternate sectors. Measurement of the influence of the variables was in terms of the number of times that each of the two figures in the form was reported seen when exposed tachistoscopically. The results of the experiment show: (1) that figures with small areas are on the whole dominant over figures with larger areas, dominance decreasing as difference in area decreases; (2) that perpendicularity of a figure markedly increases the likehood that that figure will be perceived, but (3) that this factor can make a figure dominant only when the influence of area has been minimized; (4) that in an achromatic form the introduction of a brightness difference adds to the dominance of the figure that is least bright, but (5) that the factor of least brightness can render a figure dominant only when the influence of area has been minimized.

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[19] G. T. Stevens, *Motor Apparatus of the Eyes,* 1906, 196.

ASSOCIATION IN RELATION TO FEELING AND GROSS BODILY MOVEMENT

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In 1925 Burtt and Tuttle reported the results of an interesting and suggestive experiment on "The patellar tendon reflex and affective tone."[1] They presented pleasant, unpleasant, and indifferent stimulus-words just prior to elicitations of the patellar tendon reflex, and the extent of the reflex and the association time were measured with suitable apparatus. They found a 16% average reduction in the extent of the tendon reflex for the unpleasant stimulus-words as compared with the indifferent, and the average association-time was slower for the unpleasant stimulus-words than for the indifferent. Their experiment suggested that the depression of the reflex and the slowing of the association-time involved the same aspect of the stimulus-words; namely, the affective tone. The present study is similar in several respects to this investigation by Burtt and Tuttle, and in a general way our results corroborate their findings.

METHOD AND PROCEDURE

We used the word-association method and measured the amount of gross bodily movement which occurred during and for a short time after verbal responses to pleasant (P), indifferent (I), and unpleasant (U) stimulus-words. The 300 different stimulus-words used included the items of the Kent-Rosanoff list (referred to as K-R words), and the *Ss* classified each of the stimulus-words as pleasant, indifferent, or unpleasant before the word-association experiment began. During the experiment *S* was seated in a specially constructed chair mounted on springs, and the chair was connected with recording apparatus in such a way that a quantitative measure could be obtained of his gross bodily movements while he was responding to the stimulus-words. The series of stimulus-words was given twice to each *S*, and comparisons were made between the results obtained when the second response was the same as the first and the results obtained when the second response was different from the first. The data which have been secured form an interesting link between several studies of word-association and the expressions of the emotions. The experiment is closely related to the investigations of word-association, physiological complex indicators, expressions of the emotions, and motor psychology;[2]

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[1] H. E. Burtt and W. W. Tuttle, *op. cit.*, this JOURNAL, 36, 1925, 553-61.

[2] Some references to the related literature on word-association may be found, for example, in C. G. Jung, *Studies in Word-Association*, 1919; and C. C. Miles and L. M. Terman, Sex difference in the association of ideas, this JOURNAL, 41, 1929, 165-206.

but the present paper is principally concerned with a description of the experiment itself. Few previous studies have been made of the correlation between feelings and gross bodily movements, and we hope the chair apparatus may prove useful in investigating several of the problems of complex indicators and the detection of guilt.[3]

Material. Three hundred words were used as stimulus-material. One hundred of the words were the Kent-Rosanoff words[4] and the remaining 200 were taken from a list which had been used by the writer in a previous study.[5] The 300 words were mimeographed on 10 pages or data sheets with 30 words on a page, and the last 10 words on each page were from the Kent-Rosanoff list. The first column of the data sheet was used to record the *S*'s judgment of P, I, or U for each word. The stimulus-words were in the second column, and on the right of the stimulus-words were columns in which a record was made of the time of the word-association response, the response word given by the *S*, and the amount of gross bodily movement which occurred during and for a short time after the verbal response. There were 3 other columns on the data sheet in which a record was made of *S*'s later reactions when the stimulus-words were given the second time.

Apparatus. During the experiment *S* was seated in a light but rigidly constructed chair, and the chair was placed in the middle of a platform which was 36 x 42 in. and which was 4½ in. above the floor. The seat of the chair was 20 in. above the platform, and the distance between the arms of the chair was 16½ in. in the back, and 21½ in. in front. The back of the chair sloped at a comfortable angle, and extended 20½ in. above the seat. Each of the chair's four legs rested on a vertically-placed, coil, steel spring, which was 2⅝ in. long and 1⅛ in. in diameter. The spring was securely attached to the platform and to the lower end of the leg of the chair. The distances between the legs of the chair were, for the front legs 15½ in., for the rear legs 14½ in., and for the legs on each side 16½ in. The lower ends of the chair's legs were prevented from moving in any horizontal direction by steel wires which were firmly attached to the lower ends of the legs and to 8 vertically-placed, rigid posts on the edge of the platform. The steel wires were tightly stretched between the two legs on each side, between the front legs, and between the rear legs, and they were continued in all 8 directions to the posts on the edge of the platform. When no one was seated in the chair, the point of attachment of the 4 wires to the lower end of each leg was 4 in. above the surface of the platform and ⅜ in. above the point of attachment to the posts. When a person weighing about 170 lbs. was sitting in the chair, the point of attachment of the 4 wires to the lower end of each leg was 3¼ in. above the platform and ⅜ in. below the point of attachment to the posts. The distance from the legs of the chair to the posts was approximately 11 in. The chair was quite springy to sit in, and moved with practically no friction in any direction, up and down, forward and back, and sidewise. The springs were strong enough to support a person weighing about 210 lbs. before they were completely depressed.

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[3] Word-association responses and the movements of the hands have been used by Luria in a study of the detection of guilt in criminals. (A. R. Luria, Die Methode der abbildenden Motorik in der Tatbestandsdiagnostik, *Zsch. f. angew. Psychol.*, 35, 1930, 139-83.)

[4] G. H. Kent and A. J. Rosanoff, A study of association in insanity, *Amer. J. Insanity*, 67, 1910, 37-96, 317-390.

[5] The learning and retention of pleasant and unpleasant activities, *Arch. Psychol.*, 21, 1932, (no. 134). The 200 words included all items from "absent" to "generous" in the alphabetical list of words given in Table XII of the above study, with the exception of those words that are in the Kent-Rosanoff list.

A wooden strip ¾ in. square extended from the middle of the chair's seat on the lower side directly to the right in a horizontal direction for a distance of 32 in. beyond the right edge of the seat. The inner ends of two other strips of the same material were attached to the front and rear legs 9 in. below the seat, and the outer ends of these 2 strips were securely fastened together and to the outer end of the horizontal strip. The result was a light but rigid arm extending to the right in the horizontal direction a distance of 32 in. beyond the right edge of the seat. A second extension arm constructed in the same way extended from the back of the chair. When the chair moved in any direction, the outer ends of the two extension arms showed a much greater movement, principally in the vertical direction.

Swinging by a string 5 in. long to the end of each extension arm was a piece of hard rubber 9 x 1 x ¼ in., which was free to move up and down in a fixed slide whenever the chair and the extension arms moved. Along the middle line of each piece of hard rubber was a row of 16 round-headed contacts, 5/32 in. in diam. and 5/32 in. apart. A thin strip of copper 2 in. long served as a brush, and the end of this brush touched the contact points on the hard rubber slider whenever the slider moved up and down. The sliders and the outer ends of the extension arms were shielded from the *S*'s view.

The principal features of the chair apparatus were as follows. Any slight bodily movement of the *S* in the chair caused a vertical movement of the two sliders, and produced a number of electric contacts, the number of contacts made being proportional to the amount of gross bodily movement of the *S*.[9] A large number of contacts was made if the *S* shifted his bodily position, moved his leg, raised his arm, or coughed, and some contacts were frequently made when, according to *S*'s retrospective reports, he thought that he had been sitting perfectly still. In most cases the *S* paid very little attention to his gross bodily movements. This apparatus could be made even more sensitive by using smaller round-headed contacts and by placing them closer together.

The recording and timing apparatus was placed on a table some 6 ft. from the *S* and was screened from him. Each of the hard rubber sliders was in circuit with an electric counter and a telegraph relay; and when the recording apparatus was being used, the counters registered the total number of electric contacts made by the sliders during a given interval of time. The electric counters were covered with several layers of sound absorbing material, and placed in front of the dials of the counters were 6 sheets of glass which separated 6 layers of air. *E* could readily read the dials of the counters, but *S* could not hear the counters and did not know that any instruments of this kind were being used.

An apparatus was constructed which made it possible to keep constant the interval of time during which the manual movements of the *S* were being registered by the counters. Current was shunted from the electric light current by means of a laboratory rheostat of 350 ohms capacity, and the shunted current was in circuit with a telegraph relay and a slow speed motor from a color mixing set. The motor operated a speed reducing system, and a slow moving string belt held taut by a rubber band drew a copper slider, T-shaped in cross section and inverted, along on the surface of a horizontally-placed and slightly convex copper strip. The copper slider moved at a uniform speed and made an electric contact with a projecting horizontal bar after travelling a distance of 3 in. in 5 sec. When the slider was drawn back to its original position by a simple movement of *E*'s left hand, the timing and recording apparatus was ready to be used again.

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[9] The apparatus which we have devised and described above is similar in some respects to that used by J. S. Szymanski, Aktivität und Ruhe bei den Menschen, *Zsch. f. angew. Psychol.*, 20, 1922, 192-222; and Samuel Renshaw and A. P. Weiss, Apparatus for measuring changes in bodily posture, this JOURNAL, 37, 1926, 261-267.

The general procedure with the apparatus was as follows. *E* spoke the stimulus-word, and at the same time closed a single throw, 3-pole switch with the left hand. One pole of the switch was in circuit with each of the counters registering bodily movements, and the third pole was in circuit with the motor operating the timing apparatus. At the end of a 5-sec. interval the T-shaped copper slider made an electric contact with the projecting horizontal bar and closed a fourth circuit through the break sides of the three telegraph relays, so that the counters registered *S*'s movements only during the 5-sec. interval. One relay broke the circuit through the motor, and each of the two other relays broke a circuit through one of the counters.

While looking at a stop watch, *E* gave the stimulus-word and at the same time closed the 3-pole switch which started the timing and recording apparatus. The verbal response given by *S* and the time of the response were noted by *E*, and the apparatus stopped itself after 5 sec. *E* recorded *S*'s verbal response and the time of the response, and performed the simple calculation necessary to obtain the sum of the differences between the readings on the two counters before and after the 5-sec. interval; 120 trials of this kind could be made without hurrying in one hour, and the apparatus had a definite advantage over some other methods in that all of the data could be recorded without delay.

Almost all of the previous studies of physiological activities involved in feelings and emotions have been concerned with limited portions of the body, and it is of considerable value to determine the correlation between P and U feelings, on the one hand, and gross, random, bodily, or 'manual' movements, on the other, partly because the feelings involve a large mass of bodily tissue in comparison with many other mental activities. On *a priori* grounds it seemed possible to obtain a higher correlation between feelings and gross bodily movements than between feelings and the functioning of any particular portion of the body.

In the present experiment the *S* was seated in the chair apparatus in a natural and comfortable position and was free to move about in any way he desired. Any definite or overt movements of trunk, legs, arms, and head which were the result of contractions in striped or voluntary muscles were measured by the apparatus. The movement record was a measure of the *S*'s fidgetiness, nervous restlessness, physical composure, or persistence of postural attitudes. When the chair did not move at all, *S* was holding himself in position and was maintaining his postural attitudes, and this was a positive response which involved the tonic contractions of a number of striped muscle patterns. The movement records that we secured may not be considered as measures of either relaxation or tension, and one must proceed with care in drawing conclusions in regard to particular muscle groups or specific nervous processes. The measures obtained were measures of gross bodily movements, and we feel obliged to leave their precise physiological interpretation to others.

Procedure. The writer acted as the *E* for all of the *S*s, and the following procedure was used in each case.

(1) *S* was given 1, 2, or 3 mimeographed pages of stimulus-words, and each page contained 30 words arranged in a column. There was a total of 10 different pages, or 300 different stimulus words, and the pages were rotated with different *S*'s so that each page was used 8 times.

(2) *S* was instructed to take in order the words that were to be used with him, to look at each word for a moment, and to grade it P, I, or U according to his personal reactions. The principal basis of judgment was of course the associations that were naturally evoked by the word, but in a few cases the word itself seemed P, I, or U. *S* was asked to proceed through the list slowly enough to give each word a fair chance, but not so slowly that the associations would not be typical and representative of the word. He was asked to spend the same amount of time on the different words, and if possible to give approximately the same number of grades of P, I, and U. After grading the words, *S* was asked to go through the list again and change any grades he desired. He examined the grades once more after an interval of at least one day, and made any further changes in the grades that he considered advisable.

In the large majority of the *S*s the number of grades changed on the second and third gradings combined was less than one grade out of 10. The total number of changes in grades for all of the *S*s was as follows.

		Later grading			
		P	I	U	Total
Earlier grading	P	—	29	10	39
	I	24	—	28	52
	U	21	35	—	56
	Total	45	64	38	147

These figures show that 21 grades were changed from U to P, 10 from P to U, etc.

(3) *S* was seated in the chair apparatus. He was asked to sit in a relaxed and comfortable position and to do anything that he desired provided he carried out the instructions.

(4) He was told to speak the first word he could think of, other than the word spoken by *E*, as quickly as possible after the stimulus-word was given.

During the original gradings of the words as P, I, or U, each word called out the associations that were naturally connected with it, and *S* frequently thought of life situations from his own past experiences. He also frequently thought of life situations from his own past experiences during and after his responses in the word-association procedure. The verbal response was only a part of the total psychological reaction which also included other verbal activities, movements, and feelings. The mental reactions evoked by isolated stimulus words are fairly complicated. "One word used by itself," Downey writes, "is a much richer experience than the same word used in a sentence. The 'word' as a detached consciousness has a tendency to blossom into all manner of images, feelings, impulses."[7] The word-associations in the present experiment were obtained under instructions for speed, and they differ in some respects from those that have sometimes been obtained in a more leisurely manner in some of the studies of complex indicators.

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[7] J. E. Downey, Individual differences in reaction to the word-in-itself, this JOURNAL, 39, 1927, 323-342.

E gave any further directions that were necessary, but nothing was said about *S*'s gross bodily movements or his previous affective classification of the stimulus-words.

(5) The following routine procedure was followed for each trial. (a) While looking at a stop-watch which ran continuously, *E* gave the stimulus-word without a ready signal, and at the same time closed the 3-pole switch which started the timing and recording apparatus. (b) He noted *S*'s verbal response and the time of the response in units of 1/5 sec. (c) The apparatus stopped itself after 5 sec., and at the end of this interval the *S* could hear the sound made by the 3 relays as they broke the circuits through the counters and the motor. (d) *E* opened the 3-pole switch, and drew the T-shaped slider back into position, thereby closing the make sides of the relays again, and preparing the apparatus for the next trial. (e) *E* recorded the response, the time of the response, and the amount of gross bodily movement which had been registered by the two counters during the 5-sec. interval. (f) If *S* did not respond properly or did not respond at all during the 5-sec. interval, *E* gave the stimulus-word again on a later occasion. (g) During the experiment *E* did not pay any attention to which words *S* had previously graded P, I, and U, although these grades could be seen if desired in the left-hand edge of the data sheet. The words were graded quite differently by different *Ss*.

(6) After one set of reactions had been obtained to all of the stimulus-words which were to be used with *S*, the experiment was repeated without delay in the same way as before, and the stimulus-words were again taken in the same order. The experiment generally lasted for one hour.

(7) At the end of the experimental period, *S* was asked what he considered the purpose of the experiment to be, what kind of records he thought had been made, and whether he had had any suspicions in regard to what was being investigated. Very few of the *Ss* had thought that their manual movements were being measured, but some thought that a record had been made of whether the chair moved some or did not move at all. None of the *Ss* thought that a measure had been obtained of the relative speed of word-association responses to P, I, and U stimulus words, or that a comparison would be made between their affective classification of the words and the frequency scores of responses to the K-R words.

Subjects. Most of the 39 *Ss* were students in the writer's class in general psychology, and their age and sex distribution was as follows.

Age	17	18	19	20	21	22	23	Total
Women	1	6	8	9	2	0	0	26
Men	0	1	4	2	1	4	1	13

RESULTS

The experimental data are summarized in tabular form, and in some cases the calculations have been made for individual *Ss* as well as with the scores from all of the *Ss* combined. The customary statistical formulae for variability and reliability have not been used because none of the distributions is even approximately normal, but the reliability of the results can be fairly accurately judged from the figures in the tables. Movement scores for a few of the trials are lacking because of some difficulty with the apparatus at the beginning of the experiment.[8]

The 300 stimulus-words that were used included the 100 Kent-Rosanoff words and 200 words which the writer had prepared for a previous experiment. The method of selecting these 200 words and the relative number of words that were judged P, I, and U by the present group of *Ss* is of general interest. The procedure originally used in selecting the 200 words was as follows.[9] We began with the 3000 most common words in the English language, according to Thorndike's *Teacher's Word Book* (1921), and reduced the list to 2000 by omitting all words of less than 4 letters, hyphenated words, and proper names. The 2000 words were then classified as P, I, or U by 13 judges. One group of words was made up of those words that were most frequently and consistently judged P, another group of the same size was made up of words that were most consistently judged I, and a third group, also of the same size, was made up of words that were most consistently judged U. These 3 groups of words were then made equal to each other in the number of words generally used as nouns, verbs, and adjectives. The 200 words used in the present study are an alphabetical selection from the above list of words;[10] and the *Ss* in the present experiment should theoretically have given an equal number of P, I, and U grades for these 200 words. According to the judgments of the present group of *Ss*, however, there were for these 200 words 1212 grades of P, 1022 grades of I, and 966 grades of U. For the 100 K-R words which were also used in the present study, there were 702 grades of P, 482 grades of I, and 416 grades of U. The total number of grades of P, I, and U was as follows.

	P	I	U
200 Words	1212	1022	966
100 K-R Words	702	482	416
Total	1914	1504	1382

These results give additional proof of the existence of an important tendency towards optimism in the judgment of affective activities, which we have previously discussed.[11]

(1) Speed of word-association responses. The data on the speed of word-association responses to P, I, and U stimulus-words are given in Tables I and II. Table I shows the relative ranks of P, I, and U stimulus words for different *Ss*, and Table II gives the results obtained by combining the

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[8] One of the *Ss*, who was extremely fidgety in the chair, graded practically all of the words either P or U, and also gave the same verbal response on the second trial in practically all cases. It has seemed the most justifiable procedure to omit her results from the calculations of Tables IV and XIII.

[9] The learning and retention of pleasant and unpleasant activities, *Arch. Psychol.*, 21, 1932, (no. 134), 39-41, 44-46.

[10] *Op. cit.*, Table 12.

[11] *Op. cit.*, 72.

scores from all of the *S*s. In Table I, for 16 out of 39 *S*s the responses to the P stimulus-words were slower than to the I or U stimulus-words, but for 13 *S*s the responses to the P words were faster than to the I or U words. The responses were faster to the I words than to the P or the U words. These results for individual *S*s are not as reliable as the combined results of the 4800 responses of all of the *S*s which are given in Table II. In this table all time measures from 15/5 sec. to 25/5 sec. inclusive have been counted as 15/5 sec. Counting the time measures above 15/5 sec. at their face values would not have made any difference in the relative values of the central tendencies for P, I, and U. Responses to the U words were slower than responses to the P or I words, but the speed of responses to the P and I words was approximately the same. The U feelings associated with the stimulus words have the effect of slowing up the word-association responses;[12] and this result is in harmony in a general way with the view that U activities are relatively slow and have an inhibiting influence on other processes. The slower time of the responses to the U stimulus-

TABLE I

RELATIVE RANKS OF P, I, AND U STIMULUS-WORDS IN THE SPEED OF THE FREE ASSOCIATION RESPONSES OF DIFFERENT *S*s

(Ranks for each *S* based on median. No. of *S*s = 39.)

Affectivity	Rank (1 = long time)					Av. Rank	Rank Order
	1	1½	2	2½	3		
P	16	1	8	1	13	1.92	1
I	8		16	3	12	2.14	3
U	14	1	11	2	11	1.94	2

TABLE II

SPEED OF FREE ASSOCIATION RESPONSES TO P, I, AND U STIMULUS-WORDS: COMBINED RESULTS

(Time in 1/5 sec. No. of *S*s = 39.)

	Stimulus-Words		
	P	I	U
No.	1914	1504	1382
Av.	6.81	6.80	6.94
Q_1	4.35	4.44	4.56
Q_2	6.27	6.32	6.43
Q_3	8.22	8.15	8.27

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[12] F. L. Wells found that the reactions to visual stimuli in affective settings (*J. Exper. Psychol.*, 8, 1925, 64-76) and the reaction-times to affects accompanying smell stimuli (this JOURNAL, 41, 1929, 83-86) occurred in less than 1 sec. and that there was no difference in the speed of response to these P and U stimuli. The activities which he studied, however, were different from those involved in free word-association.

words probably indicates also that these connections are weaker than those involved in the responses to P or I stimulus-words.[13]

The average speed of response of our *S*s using a procedure without a ready signal was considerably greater than that obtained by Jung,[14] but less than that reported by Burtt and Tuttle.[15] The special problem of the relative speed of word-associations to P, I, and U stimulus-words has been previously studied by Birnbaum, Tolman and Johnson, Anderson and Bolton, and Burtt and Tuttle.[16] Our results agree fairly closely with those obtained by Burtt and Tuttle.

TABLE III

Relative Ranks of P, I, and U Stimulus-Words in the Amount of Movement Made by Different *S*s During and After Free Association Responses

(Ranks for each *S* based on median. No. of *S*s = 38.)

Affectivity	Rank (1 = large amount of movement)					Av. Rank	Rank Order
	1	1½	2	2½	3		
P	10	1	19	1	7	1.92	2
I	5	2	11	2	18	2.34	3
U	20	3	3	1	11	1.74	1

TABLE IV

Amount of Movement During and After Free Association Responses to P, I, and U Stimulus-Words: Combined Results

(No. of *S*s = 37.)

	Stimulus-Words		
	P	I	U
No.	1442	1162	1058
Av.	2.65	2.58	2.61
Q_1	1.17	1.14	1.20
Q_2	2.06	2.03	2.08
Q_3	3.46	3.35	3.45

(2) Amount of movement. The results on the amount of movement during and after the responses are given in Tables III and IV. Table III shows that for 18 out of 38 *S*s less movement occurred during and after responses to the I words than to the P or U words. According to these results, the greatest amount of movement occurred for responses to U words,

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[13] See, for example, G. E. Müller and A. Pilzecker, Experimentelle Beiträge zur Lehre vom Gedächtniss, *Zsch. f. Psychol.*, Erg. I, 1900, 1-300.

[14] C. G. Jung, *Studies in Word-Association*, 1919, 227-265.

[15] *Op. cit.*

[16] Karl Birnbaum, Über den Einfluss von Gefühlsfaktoren auf die Assoziationen, *Monatsschr. f. Psychiat. u. Neurol.*, 32, 1912, 95-123, 194-220; E. C. Tolman and I. Johnson. A note on association-time and feeling, this Journal, 29, 1918, 187-195; A. C. Anderson and F. J. Bolton, Inhibition of the unpleasant, *J. Abnorm. & Soc. Psychol.*, 20, 1925-26, 300-302; H. E. Burtt and W. W. Tuttle, *op. cit.*

an intermediate amount for responses to P words, and the least movement occurred for responses to I words. These results are not as reliable as the combined results of all of the *S*s which are given in Table IV. In this table all movement scores above 10 have been counted as 10. Counting the movement measures above 10 at their face values would not have made any difference in the central tendency values of the movment scores for P, I, and U. In both of the tables, the amount of movement for the responses to I words was less than the amount of movement for the responses to P or U words. The movement scores during and after responses to the P and U words were approximately the same. It appears therefore that the presence of the affective factor slightly increased the amount of gross bodily movement during and after the responses, and this seems to show that manual movements are in some way associated with the milder forms of feelings and emotions. These results are similar to those secured by Nunberg with Sommer's apparatus.[17]

TABLE V

RELATIVE RANKS OF P, I, AND U KENT-ROSANOFF STIMULUS-WORDS IN THE FREQUENCY SCORES OF THE RESPONSES OF DIFFERENT *S*s

(Ranks for each *S* based on median. No. of *S*s = 39.)

Affectivity	Rank (1 = low frequency score)			Av. Rank	Rank Order
	1	2	3		
P	18	14	7	1.72	1
I	8	13	18	2.26	3
U	13	12	14	2.03	2

TABLE VI

FREQUENCY SCORES OF FREE ASSOCIATION RESPONSES TO P, I, AND U KENT-ROSANOFF STIMULUS-WORDS: COMBINED RESULTS

	Stimulus-Words		
	P	I	U
No.	702	482	416
Av.	83.7	109.6	105.9
Q_1	1.4	4.6	2.4
Q_2	22.4	40.5	39.5
Q_3	117.9	186.8	177.1

(3) *Affective responses to the Kent-Rosanoff words.* The results on the frequency scores of responses to P, I, and U K-R stimulus-words are given in Tables V and VI. Table V shows, for example, that for 18 out of 39 *S*s the frequency scores of responses to P words were lower (responses less common) than the frequency scores of responses to U or I

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[17] See C. G. Jung, *op. cit.*, 531-560.

words. In Table VI all of the frequency scores from 200 to 650 inclusive have been grouped together, and these frequency scores have been given a value of 325 in calculating the averages. The method of grouping all of the K-R scores is shown in Table XI. The results of Tables V and VI show that the frequency scores of responses to P words are the lowest (responses most unusual), the frequency scores of responses to U words are next in order, and the frequency scores of responses to I words are the highest (responses most common). The reliable order of rank for peculiarity of the responses to the K-R words is P-U-I, but the difference between the scores for P and U is much greater than the difference between the scores for U and I. It seems justifiable to assume that the responses associated with the P stimulus-words are the weakest and the least habitual.

TABLE VII

NUMBER OF DIFFERENT RESPONSES GIVEN BY KENT AND ROSANOFF'S *S*s TO WORDS JUDGED P, I, AND U BY THE PRESENT GROUP OF *S*s

(Each Kent-Rosanoff word was judged P, I, or U by at least 6 out of 8 of our *S*s.)

P				I		U	
K-R Word	No. Diff. Rsps. in K-R Tables	K-R Word	No. Diff. Rsps. in K-R Tables	K-R Word	No. Diff. Rsps. in K-R Tables	K-R Word	No. Diff. Rsps. in K-R Tables
3. music	172	59. health	146	1. table	124	4. sickness	145
5. man	169	61. memory	227	20. chair	106	19. command	214
7. soft	155	63. bath	93	32. citizen	192	31. rough	201
8. eating	168	64. cottage	143	49. eagle	109	34. spider	126
9. mountain	142	66. blue	116	51. stem	120	38. anger	280
13. comfort	169	69. ocean	110	60. bible	128	43. sour	91
16. fruit	126	75. child	157	72. long	178	45. trouble	255
18. smooth	156	79. city	131	77. hammer	109	67. hungry	173
21. sweet	96	86. joy	172	81. butter	101	76. bitter	138
27. river	127	90. baby	151	96. street	163	78. thirsty	86
29. beautiful	182	91. moon	116			83. loud	141
37. sleep	130	94. green	123			84. thief	170
56. justice	178	99. blossom	99			88. heavy	158
58. light	142					100. afraid	189
No.			27		10		14
Av.			144.3		133.0		169.2
Q_1			121.2		107.5		132.0
Q_2			143.0		122.0		164.0
Q_3			169.0		170.5		207.5

In Table VII we have brought together all of the *K-R words* that were *judged P, I, or U* by at least 6 out of 8 of our *S*s. On the left of each K-R word is the number of the word as it appears in the K-R tables, and in the column to the right of each K-R word is the number of different responses given to this stimulus-word by all of the 1000 K-R *S*s. For example, the third word in the K-R list is "music," and Kent and Rosanoff's *S*s gave 172 different responses to this word. Although the total number of words used in the present calculation is only 51, it appears that the amount of scatter, as measured by the number of different responses given by Kent-Rosanoff's *S*s, is greatest for the U words, second in order for the P words, and least for the I words.

TABLE VIII

CASES OF FAILURE TO RESPOND

Number and percent of failures to respond to the different stimulus-words			Amount of movement during failure to respond		Amount of movement during and after later response		Percent of later changed responses after *S* failed to respond on first trial
P	I	U	Av.	Med.	Av.	Med.	
(1914)	(1504)	(1382)					
25 1.3%	32 2.1%	27 2.0%	3.6	2.0	2.6	2.0	48.3%
N=84			N=79		N=84		N=60

(4) *Failures to respond.* When *S* did not respond during the 5-sec. interval, *E* noted the fact and in most cases made a record of the amount of movement occurring during the interval. The data on these cases are given in Table VIII, but they are not particularly reliable because of the small number of cases. Table II shows that 1914 words were judged P, 1504 were judged I, and 1382 were judged U, when all of the stimulus-words and *S*s are combined. If the calculation is based on the total number of judgments of P, I, and U, there was only 1.3% of failures to respond to the P words, as compared with 2.1% of failures to respond for the I words and 2.0% for the U words. The average amount of movement which occurred during the failures to respond (3.6) was greater than that which occurred in an equal interval of time during and after the responses (2.6, in Table IV); but the median amount of movement which occurred during the failures to respond (2.0) was approximately the same as that which

TABLE IX

RELATIVE RANKS OF P, I, AND U STIMULUS-WORDS IN THE PERCENTAGE OF RESPONSES CHANGED BY DIFFERENT *S*S ON THE SECOND FREE ASSOCIATION RESPONSE

(No. of *S*s = 39.)

Affectivity	Rank (1 = large percentage changed)				Av. Rank	Rank order
	1	1½	2	3		
P	17	1	13	8	1.76	1
I	8	1	12	18	2.24	3
U	12	2	12	13	2.00	2

ordinarily occurred during and after the responses (2.1, in Table IV). There was no difference between the amount of movement during and after the responses that were obtained later, after failure to respond, and the amount of movement which ordinarily occurred; and there was also no reliable difference between the percentage of later changed responses after *S* failed to respond on the first trial (48.3) and the percentage of responses that were ordinarily changed (50.6, in Table X).

TABLE X

PERCENTAGE OF RESPONSES CHANGED ON THE SECOND FREE ASSOCIATION RESPONSE TO P, I, AND U STIMULUS-WORDS: COMBINED RESULTS

(No. of *Ss* = 39.)

Responses changed	Stimulus-Words P	Stimulus-Words I	Stimulus-Words U
Percentage	51.9%	49.5%	50.4%
No.	957	752	691

(5) *Change at second response.* The percentage of responses that were changed at the second word-association response to P, I, and U words are given in Tables IX and X. A slightly larger percentage of responses was changed to the P stimulus-words than to the U words, and a slightly larger percentage of responses was changed to the U words than to the I words. These results suggest that the general stability or consistency of the responses was highest for the I words and lowest for the P words.

It may be recalled that each *S* graded the stimulus-words that he was to use as P, I, or U according to his own personal reactions. The feelings associated with most of the words were quite mild, but this seems to make the results all the more important because mild feelings are the kind of feelings that are most commonly associated with our language responses. The experimental data which we have obtained therefore afford a useful basis of comparison between the results of a number of experiments on word-association, on the one hand, and several studies of complex indicators and the expressions of the emotions, on the other. The principal results which we have described above may be briefly summarized as follows.

Speed (Slowest first)	Movement (Greatest first)	K-R Score (Lowest first)	Responses Changed (Largest percentage first)	No. Diff. Responses in K-R Tables (Largest number first)
U	U P	P	P	U
		U	U	P
P I	I	I	I	I

Further data on the relations between the number of changed responses, on the one hand, and the frequency scores of the K-R words, the speed of the responses, and the amount of movement during and after the responses, on the other, are given in Tables XI, XII, and XIII. Each of the stimulus-words used with a given S was used twice; and for each stimulus-word there were two responses, two measures of the speed of the response, and two measures of the amount of movement during and after the response. In some cases the response given on the second trial was the same as the response given on the first trial, and in other cases the second response was different from the first. In the data presented in Tables XI, XII, and XIII, we have made comparisons between the results on frequency, time, and movement when the responses on the two occasions were the same and when the responses on the two occasions were different.

TABLE XI

Relation Between the Frequency Scores of Responses to the Kent-Rosanoff Words and the Number of Changed Responses

	K-R frequency score of					
K-R frequency scores	First response			Second response		
	When second same	When second different	% change	When same as first	When different from first	% change
200–650	113	59	34.3	113	31	21.5
100–199	56	49	46.7	56	50	47.2
50– 99	50	39	43.8	50	36	41.9
25– 49	35	34	49.3	35	41	53.9
15– 24	22	17	43.6	22	28	56.0
10– 14	15	16	51.6	15	23	60.5
7– 9	8	20	71.4	8	17	68.0
5 and 6	13	13	50.0	13	14	51.9
3 and 4	11	20	64.5	11	18	62.1
1 and 2	28	55	66.3	28	49	63.6
0	36	91	73.4	36	106	74.6
No.	387	413		387	413	
Av.	113.8	76.6		131.8	55.6	
Q_1	9.8	0.9		9.8	0.5	
Q_2	75.0	11.8		75.0	10.0	
Q_3	227.5	109.2		227.5	68.6	

Table XI shows the relation between the frequency scores of responses to the K-R words and the number of responses that were changed on the second trial. We have made one calculation using the K-R frequency scores of the first of the two responses, and another calculation using the K-R frequency scores of the second of the two responses. Considering first the K-R frequency scores of the first of the two responses, for those responses

TABLE XII

Relation Between Speed of Response and the Number of Changed Responses

Time in ½ sec.	Time of First response: When second same	Time of First response: When second different	Time of First response: % change	Time of Second response: When same as first	Time of Second response: When different from first	Time of Second response: % change
15+	22	75	77.3	17	48	73.8
14	10	31	75.6	8	29	78.4
13	16	50	75.8	7	37	84.1
12	21	46	68.7	15	42	73.7
11	33	70	68.0	40	75	65.2
10	28	48	63.2	18	24	57.1
9	41	66	61.7	24	71	74.7
8	146	189	56.4	108	211	66.1
7	145	172	54.3	111	179	61.7
6	215	184	46.1	159	193	54.8
5	166	108	39.4	152	101	39.9
4	195	102	34.3	206	126	38.0
3	140	83	37.2	313	86	21.6
No.	1178	1222		1178	1222	
Av.	6.32	7.87		5.60	7.51	
Q_1	4.29	5.57		3.44	5.43	
Q_2	5.91	7.28		4.96	7.09	
Q_3	7.65	9.76		6.98	8.79	

TABLE XIII

Relation Between Amount of Movement and the Number of Changed Responses

Amount of Movement	Movement during and after First Response: When second same	Movement during and after First Response: When second different	Movement during and after First Response: % change	Movement during and after Second Response: When same as first	Movement during and after Second Response: When different from first	Movement during and after Second Response: % change
10+	16	19	54.3	15	20	57.1
9	9	16	64.0	4	8	66.7
8	12	15	55.6	9	21	70.0
7	17	34	66.7	17	20	54.1
6	39	37	48.7	29	31	51.7
5	56	51	47.7	43	58	57.4
4	62	80	56.3	53	79	59.8
3	114	139	54.9	95	120	55.8
2	230	321	58.3	263	294	52.8
1	230	232	50.2	215	231	51.8
0	73	72	49.7	90	73	44.8
No.	858	1016		833	955	
Av.	2.62	2.74		2.41	2.67	
Q_1	1.12	1.28		1.05	1.22	
Q_2	2.05	2.14		1.92	2.09	
Q_3	3.47	3.49		3.10	3.49	

that were 'individual' in the K-R tables (frequency of zero), there were 36 cases where the second response was the same as the first, and 91 cases where the second response was different from the first, or 73.4% of changed responses. For those responses that had a K-R frequency score of 200 or more, there were 113 cases where the second response was the same as the first, and 59 cases where the second response was different from the first, or 34.3% of changed responses. There is a tendency for the percentage of changed responses to increase as the K-R frequency scores decrease. The average frequency score of the same responses is 131.8, and the average frequency score of the different responses is 76.6; while the median frequency score of the same responses is 75.0, and the median frequency score of the different responses is 11.8. Similar results are obtained when the calculations are based on the K-R frequency scores of the second of the two responses. The results indicate that the K-R frequency scores are less common when the second response is different from the first than when the second response is the same as the first.

Table XII shows the relation between the speed of the response and the number of responses that were changed on the second trial. Considering the time of the first response, for those responses that had a time of 15/5 sec. or more, there were 22 cases where the second response was the same as the first, and 75 cases where the second response was different from the first, or 77.3% of changed responses. For those responses that had a time of 3/5 sec., there were 140 cases where the second response was the same as the first, and 83 cases where the second response was different from the first, or 37.2% of changed responses. There is a tendency for the percentage of changed responses to increase as the time increases. The average time of the same responses is 6.32/5 sec., and the average time of the different responses is 7.87/5 sec.; while the median time of the same responses is 5.91/5 sec., and the median time of the different responses is 7.28/5 sec. Similar results are obtained when the calculations are based on the time scores of the second of the two responses. These results indicate that the speed of the response is less, and the connections are probably weaker, when the second response is different from the first than when the second response is the same as the first.[18]

Table XIII shows the relation between the amount of movement and the number of responses that were changed on the second trial. If we consider the amount of movement that occurred during and after the first response, for those responses where the movement score was at least 10, there were 16 cases where the second response was the same as the first, and 19 cases where the second response was different from the first, or 54.3% of changed responses. The average movement score of the same responses was 2.62, and the average movement score of the different responses was 2.74, while the medians were 2.05 and 2.14, respectively. Similar results are obtained when the calculations are based on the amount of movement that occurred during and after the second of the two responses. These results indicate that the amount of movement is slightly greater when the second response is different from the first than when the second response is the same as the first.

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[18] A similar tendency was observed by M. Pappenheim, Merkfähigkeit und Assoziationsversuche, *Zscb. f. Psychol.*, 46, 1908, 161-173.

The results given in Tables X–XIII show that there was avoidance of repetitive responses in approximately one-half of the cases, and the results in Table XIII on the first and second responses to the K-R words indicate that the responses became somewhat more unusual on the second trial. These results seem to have been influenced by the *S*s' general habit of avoiding repetition in ordinary language activities; and it appears that they cannot be explained by reference to the physiological principle of refractory phase.[19] In obtaining the second responses to the stimulus-words, the *S* was asked to speak the first word that came to mind regardless of whether it was the same as, or was different from, the response that he had given on the first trial, but the attempt to avoid all factors of suggestion in these instructions may not have been entirely successful.

After one response had been obtained to each of the stimulus-words that were to be used with an individual *S*, a second response was obtained to the same list of stimulus-words, and the stimulus-words were taken in the same order as before. The results given in the left blocks of Tables XI, XII, and XIII were obtained in the first half of the experimental period and the results in the right blocks were obtained in the last half of the experimental period.

In Table XI, on the frequency scores of responses to the K-R words, the only comparison that can be made between the two halves of the experiment, *i.e.* between the first and second responses to the same stimulus-word, is when the second response was different from the first. The frequency scores based on the first and the second responses to each stimulus-word, are as follows.

	Different Responses	
	First	Second
Av.	76.6	55.6
Q_1	0.9	0.5
Q_2	11.8	10.0
Q_3	109.2	68.6

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[19] Cf. C. W. Luh, The avoidance of repetitive responses, *Psychol. Rev.*, 38, 1931, 276-9.

These results show that the frequency scores of the second responses to the K-R stimulus-words were lower (the responses less common) than the frequency scores of the first responses to the K-R stimulus-words.

The data from Table XII, on the time of the first and second responses are as follows.

	Same Responses		Different Responses	
	First	Second	First	Second
Av.	6.32	5.60	7.87	7.51
Q_1	4.29	3.44	5.57	5.43
Q_2	5.91	4.96	7.28	7.09
Q_3	7.65	6.98	9.76	8.79

These results show that the second responses to the stimulus-words were faster than the first responses to the same stimulus-words.

The data from Table XIII, on the amount of movement during and after the first and second responses are as follows.

	Same Responses		Different Responses	
	First	Second	First	Second
Av.	2.62	2.41	2.74	2.67
Q_1	1.12	1.05	1.28	1.22
Q_2	2.05	1.92	2.14	2.09
Q_3	3.47	3.10	3.49	3.49

These results show that less movement occurred during and after the second responses in the second halves of the experimental period than during and after the first responses to the same stimulus-words in the first halves of the experimental period.[20]

(6) Relation between frequency scores, time, and amount of movement. The results on the relation between the frequency scores of responses to the K-R words, the time of the responses, and the amount of movement during and after the responses are given in Tables XIV, XV, and XVI; and the construction of all three tables is the same.

In Table XIV on the relation between frequency scores and time, there were 80 cases where the frequency score of the response was at least 200 and where the time was 3/5 sec. The total number of cases is shown in the third row from the bottom and in the third column from the right. The median scores are given in the second row from the bottom and in

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[20] Some of these results on the effect of practice on word-association corroborate the findings of F. L. Wells, Practice effects in free association, this JOURNAL, 22, 1911, 1-13.

TABLE XIV

RELATION BETWEEN THE FREQUENCY SCORES OF FREE ASSOCIATION RESPONSES TO THE KENT-ROSANOFF STIMULUS-WORDS AND THE SPEED OF THE RESPONSES

K-R frequency scores	Time in ⅕ Sec.													No. cases	Time	
	3	4	5	6	7	8	9	10	11	12	13	14	15+		Med.	Av.
200–650	80	56	34	61	35	26	4	5	7	1	4		3	316	5.1	5.2
100–199	45	50	30	19	17	30	3	5	6	3	1		2	211	4.9	
50– 99	38	28	17	33	15	22	4	1	6	3	2	4	2	175	5.6	
25– 49	27	14	19	28	11	22	10	6	4		3		1	145	5.9	6.4
15– 24	10	13	14	7	15	14	5	2	2	1	2	2	2	89	6.5	
10– 14	6	13	9	14	12	6	2	1	2		3	1		69	6.0	
7– 9	2	6	6	9	9	15	1	2		1	1		1	53	6.9	
5 and 6	4	6	6	9	13	5	1	2	3	1	1		2	53	6.6	
3 and 4	6	3	6	16	8	12	1	2	1		3	1	1	60	6.4	6.6
1 and 2	19	13	14	36	18	22	8	2	6	5	9	4	4	160	6.4	
Individ.	17	28	27	40	35	46	13	8	12	17	9	4	13	269	7.1	
No. cases	254	230	182	272	188	220	52	36	49	32	38	16	31	1600		
K-R frequency scores:																
Med.	97.0	83.5	36.3	29.0	14.1	17.4	14.5	19.5	17.0	4.7	3.2	2.5	1.8			
Av.	61.4				16.4				5.8							

the second column from the right. The median frequency score for the time of 3/5 sec. is 97.0, and the median time score for the frequency of 200-650 is 5.1/5 sec. In the bottom row, the figure 61.4 is the average of the four medians, 97.0, 83.5, 36.3, and 29.0. The results in the table indicate that as the time increases the frequency scores decrease, and as the frequency scores decrease the time increases. This finding supports a result that we have previously obtained.[21]

Table XV shows the relation between the frequency scores and the amount of movement, and it is interesting to note that the correlation between the two is practically zero. The results on the relation between time and movement are given in Table XVI, and these figures likewise show that there is practically a zero correlation between the two. In Tables XIV, XV, and XVI, slightly different results would have been obtained if the averages instead of the medians had been calculated in the second row from the bottom and in the second column from the right, but all of the scores are given in the tables and the most accurate impression may be formed by a direct examination of the figures themselves.

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[21] H. and E. B. Cason, Association tendencies and learning ability, *J. Exper. Psychol.*, 8, 1925, 167-89. Cf. also M. Trautscholdt, Experimentelle Untersuchungen über die Assoziation der Vorstellungen, *Phil. Stud.*, 1, 1883, 213-250.

TABLE XV

Relation Between the Frequency Scores of Free Association Responses to the Kent-Rosanoff Stimulus-Words and the Amount of Movement During and After the Responses

K-R frequency scores	Amount of Movement											No. cases	Movement	
	0	1	2	3	4	5	6	7	8	9	10+		Med.	Av.
200–650	26	64	82	29	15	12	9	10	4	5	9	265	2.0	2.1
100–199	13	50	53	23	10	8	5	3	2	2	7	176	2.0	
50– 99	14	27	45	15	11	6	7	3	3	1	9	141	2.2	
25– 49	14	42	30	13	5	8	3		3	1	1	120	1.6	1.9
15– 24	5	17	26	10	5	1	4	4			7	79	2.2	
10– 14	8	13	14	1	7	2		1	2		1	49	1.8	
7– 9	2	13	17	3	4	2		1			1	43	1.9	
5 and 6	5	8	12	5	3	2	2				4	41	2.1	
3 and 4	1	11	15	7	3	1	2	4	1			45	2.2	2.2
1 and 2	10	28	33	16	13	6	8	2	2	1	4	123	2.2	
Individ.	15	53	66	33	15	11	7	8	5	2	12	227	2.2	
No. cases	113	326	393	155	91	59	47	36	22	12	55	1309		
K-R frequency scores:														
Med.	43.3	36.5	35.8	29.3	15.5	38.5	28.8	19.5	32.8	149.5	22.4			
Av.	38.5			27.8			50.6							

TABLE XVI

Relation Between Speed of Free Association Responses and the Amount of Movement During and After the Responses

Time in ⅕ sec.	Amount of Movement											No. cases	Movement	
	0	1	2	3	4	5	6	7	8	9	10+		Med.	Av.
15+	6	27	44	19	9	3	8	2	1	3	4	126	2.2	2.1
14	5	17	20	5	10	1	2				2	62	2.0	
13	5	21	24	13	7	6	7	3		2	4	92	2.3	
12	8	19	38	12	6	7	6	3			4	103	2.1	
11	7	44	60	21	9	12	9	10	2	1	1	176	2.1	
10	6	23	30	14	7	3	5	4	2	1	2	97	2.2	2.1
9	12	37	45	27	16	11	6	1	2	3	5	165	2.2	
8	34	131	149	61	45	30	20	14	13	6	11	514	2.1	
7	37	117	150	57	43	24	15	10	3	5	19	480	2.1	
6	47	146	186	78	36	39	19	13	11	3	27	605	2.1	2.0
5	50	109	107	54	26	27	16	11	11	5	12	428	2.0	
4	54	119	148	65	36	32	16	16	15	7	18	526	2.1	
3	51	149	141	60	39	22	20	11	9	4	22	528	2.0	
No. cases	322	959	1142	486	289	217	149	98	69	40	131	3902		
Time:														
Med.	5.6	6.2	6.4	6.3	6.7	6.2	6.7	6.3	5.5	6.7	6.0			
Av.	6.1			6.4			6.2							

SUMMARY AND CONCLUSIONS

The *S* was seated in a specially constructed chair apparatus, and the amount of gross bodily movement was measured during and for a short time after word-association responses to pleasant, indifferent, and unpleasant stimulus words. The list of stimulus-words was repeated, and various comparisons were made between speed of responses, amount of gross bodily movement, frequency scores of responses to the Kent-Rosanoff words, and number of responses that were changed on the second trial. The results of the experiment are closely related to the investigations of word-association, complex indicators, expressions of the emotions, and motor psychology. The principal conclusions are as follows.

(1) The *S*s showed a tendency towards optimism in grading the words P, I, and U.

(2) The responses to the U stimulus-words were slower than the responses to the P or I stimulus-words, but the speed of the responses to the P and I stimulus-words was approximately the same.

(3) Less gross bodily movement occurred during and after the responses to the I stimulus-words than during and after the responses to the U or P stimulus-words.

(4) The order of rank for peculiarity of the responses to the K-R words was P-U-I.

(5) The order of rank for the percentage of responses changed on the second trial was P-U-I.

(6) There was a tendency for the percentage of responses changed on the second trial to increase as the frequency scores of the responses to the K-R words decreased.

(7) There was a tendency for the percentage of responses that were changed on the second trial to increase as the time of the responses increased.

(8) More movement occurred during and after the responses when the second response was different from the first than when the second response was the same as the first.

(9) When the word-association procedure was repeated, there was avoidance of repetitive responses in approximately one-half of the cases.

(10) For the second of the two responses to the stimulus-words, *i.e.* during the second halves of the experimental periods, the frequency scores of responses to the K-R words were lower (the responses were less common), the speed of the responses was faster, and less movement occurred during and after the responses.

(11) There was a tendency for the frequency scores of responses to K-R words to decrease as the time of the responses increased.

(12) The correlation between the frequency scores of responses to the K-R words and the amount of movement which occurred during and after the responses was approximately zero.

(13) The correlation between the time of the responses and the amount of movement which occurred during and after the responses was also approximately zero.

ADAPTATION TO THE PAIN OF RADIANT HEAT

By Lawrence Joseph Stone, Columbia University, and
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The controversy regarding the essential nature of pain which has long been waged by Von Frey and Goldscheider,[1] is concerned with one of the fundamental problems in sensory physiology and psychology. Its resolution is of systematic importance to both sciences.

Von Frey holds that cutaneous pain is a separate modality with specific nervous structures—*i.e.* the free nerve-endings. Included in the evidence which he offers for this theory are his results from an experiment on pain adaptation. He found that, under continuous stimulation, a painful experience becomes steadily weaker and finally disappears without undergoing a qualitative change.[2]

Goldscheider, on the other hand, regards pain as a common feeling (*Gemeingefühl*) without specific nerve-endings. It is aroused, he thinks, through the mediation of the tactile nerve fibers and results from a summation of their excitations: weak stimulation gives touch; intense stimulation, by way of summation, gives pain. In support of this theory he too points to the results of an experiment on pain adaptation. He found, under continuous stimulation, that the experience which is at first painful not only becomes weaker but also undergoes a qualitative change;[3] it passes, he reports, into an *"unterschmerzliche Empfindung"* or *"Druck"* before its complete disappearance.

Each side of the controversy points to the phenomenon of pain adaptation as evidence for its theory; consequently the problem of adaptation has become an *experimentum crucis.*

Two studies have recently appeared upon the adaptation of cutaneous pain. Besides confirming the fact of adaptation, they attempted to resolve this controversy. In the first study, Wells and Hoisington found that "the end-quality of pure, non-pressury, cutaneous pain . . . is neither a weak pain (Von Frey) nor a pressure

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[1] For an exposition of the two points of view and a history of the controversy see Friedrich Kiesow, The problem of the conditions of arousal of the pure sensation of cutaneous pain, *J. Gen. Psychol.*, 1, 1928, 199-212.

[2] M. von Frey, Beiträge zur Physiologie der Schmerzsinnes, *Ber. ü. d. Verhandl. d. königl. sächs. Ges. d. Wiss. z. Leipzig, math.-phys. Kl.*, 46, 1894, 193; *Untertersuchungen über die Sinnesfunktionen der menschlichen Haut,* 1896, 242; Der Schmerzsinn, in M. von Frey and H. Rein's, *Physiologie der Haut,* 1929, 142 (Vol. 1, Pt. 2, of the *Handbuch der Haut- und Geschlechtskrankheiten,* 1929).

[3] A. Goldscheider, *Gesammelte Abhandlungen,* 1, 1898, 44, 397; Über Schmerz und Schmerzbehandlung, *Zsch. f. physik, und diät. Therap.*, 19, 1915, 289-309; *Das Schmerzproblem,* 1920, 20, 71; Beiträge zur Physiologie der Gemeingefühle: I. Physiologie des Schmerzes, *Zsch. f. Sinnesphysiol.*, 57, 1926, 1 ff.

(Goldscheider), but a non-pressury 'bright contact'."[4] Upon the basis of this result, they suggest that the difference between Von Frey and Goldscheider is principally terminological.

Though recognizing the difficulty and danger of equating qualitative terms, Wells and Hoisington suggest, by way of reconciling the disparate results, that Von Frey's "weak pains" and Goldscheider's *"unterschmerzliche Empfindungen"* and *"Drücke"* correspond to their "bright contacts." If such is the case—"if Von Frey's insistence that isolated pain spots give nothing but pain is . . . due to a confusion of 'weak pain' with non-painful 'bright contact'," and "if Goldscheider's contention that isolated pain spots give 'pressure' is the result of a failure to differentiate between true pressure and the non-pressury contact-like experience"—then, they conclude, "the controversy will be settled."[5] Yes, settled by the elimination of Von Frey! For Von Frey to admit "that the final phase of pain subsidence is not really painful"[6] (as Wells and Hoisington would have him do), would be to accept Goldscheider's theory, for such a statement contains the essence of the latter's theory. Wells and Hoisington's study must, therefore, be regarded as corroborating Goldscheider.

The second study, by Burns and Dallenbach,[7] also corroborates Goldscheider, for these authors found that "the pain experiences aroused by punctiform stimuli are replaced by pressury experiences before the stimulus is completely adapted."[8] Their confirmation, however, is conditional, since they recognize the possibility, which others have overlooked, that the replacement of pain by pressure or 'bright contact' might be due to "the residual effect of the pressure-pain stimulus" employed in their experiments.[9] They are of the opinion that this is the case; but, as they observed, "the only way . . . of deciding this question is to arouse pain with stimuli, for example, radiant heat, that do not give rise simultaneously to pressure experiences."[10] This statement provides the point of departure for the present study.

Our object in this study was twofold. We wished (1) to learn whether the pain aroused by radiant heat would under continuous stimulation come to complete adaptation; and (2) to discover whether pressure or pressure-like experiences were inevitable concomitants of pain at any phase in the process. We hoped to be able, in the light of our results, to contribute something definite to the broader problem with which Von Frey and Goldscheider were concerned; namely, the fundamental nature of pain: is it a separate modality, or is it merely part and parcel of a *Gemeingefühl* which includes pressure?

Radiant heat has not, as far as we know, been used in the study of pain adaptation, though it has been used as a stimulus for the arousal of pain.[11] Von Frey used it in testing Goldscheider's statement "dass die Erregung der Schmerzpunkte *unterschmerzlich beginnt und unterschmerzlich aufhört*."[12] In order to escape the concomitant pressures that are aroused when an object is brought to bear upon the skin, Von Frey aroused pain by concentrating the sun's rays upon the tissues by means of a lens. Because he was able to obtain pure, pressureless pains by this method, he concluded that Goldscheider's summation theory of pain is untenable.[13] In refutation of this experiment and conclusion, Goldscheider pointed out that the course of experience is telescoped when strong stimuli are used, and that the *unterschmerzliche Empfindungen* (*Berührungsempfindungen*) are present, though obscured by the intense pain.[14] "Ich habe," says Goldscheider, "deshalb den Verbrennungschmerz gar nicht in den Kreis meiner Untersuchungen einbezogen und es wäre besser gewesen, wenn auch mein Gegner auf dieses unzureichende Beweismaterial verzichtet hatte."[15]

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[4] E. F. Wells and L. B. Hoisington, Pain adaptation: A contribution to the Von Frey-Goldscheider controversy, *J. Gen. Psychol.*, 1, 1928, 364.

[5] *Op. cit.*, 365.

[6] *Idem.*

[7] M. Burns and K. M. Dallenbach, The adaptation of cutaneous pain, this JOURNAL, 45, 1933, 111-117.

[8] *Ibid.*, 117.

[9] *Idem.*

[10] *Ibid.*, 116.

Fortunately, in refuting Von Frey's evidence, Goldscheider lays down, by implication at least, the conditions under which the *unterschmerzliche Empfindungen* are to be perceived. They are two in number: (1) the pain aroused should not be intense; and (2) it should not rise suddenly or quickly. Though these conditions may not have been realized in Von Frey's experiments with radiant heat, they did obtain, as we shall later show, in our experiments. Consequently, if we discover that subpainful pressury experiences are not invariable accompaniments of the pain of radiant heat, then Burns and Dallenbach's suggestion—that the pressury concomitants of painful experiences are due to the pressure-pain stimulus employed—will be substantiated, and Von Frey's theory that pain is a distinct and separate modality of sensation will be confirmed.

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[11] It has frequently been used as a stimulus in studies on the localization of pure warmth. For a report of such an experiment and a review of the earlier literature, see F. L. Dimmick, On the localization of pure warmth sensations, this JOURNAL, 26, 1915, 142-150.

[12] Goldscheider, *Das Schmerzproblem*, 1920, 21.

[13] Von Frey writes of these pressureless pains as follows: "Für die vorliegende Betrachtung ist wesentlich, dass an vielen Orten Schmerz ohne begleitende andersartige Empfindung erregt werden kann, insbesondere fehlt jede Berührungsempfindung, die angeblich den Schmerz regelmässig begleiten oder ihm vorausgehen soll" (Versuche über schmerzerregende Reize, *Zsch. f. Biol.*, 76, 1922, 9).

[14] Regarding the statement, "dass eine Berührungsempfindung den Schmerz regelmässig begleiten oder ihm vorausgehen soll," with which Von Frey credits him, Goldscheider writes: "Dies gilt aber . . . nicht für Reize von solcher Stärke, dass die Erregung das Stadium der Berührungsempfindung schnell durchlaufend sofort zur schmerzhaften Höhe ansteigt. Maximale Schmerzreize sind überhaupt ganz ungeeignet, um in dieser Frage etwas zu entscheiden, da der plötzlich einbrechende starke Schmerz alle anderen Empfindungen überlagert" (Beiträge zur Physiologie der Gemeingefühle: I. Physiologie des Schmerzes, *Zsch. f. Sinnesphysiol.*, 57, 1926, 5).

[15] *Idem.*

Method and Procedure

The dorsal surfaces of both forearms were used as the experimental tissues. These areas were shaved and kept smooth during the course of the experiment, so as to avoid the pressury experiences which otherwise might have been aroused by contact of the hairs with the radiant-heat stimulator.

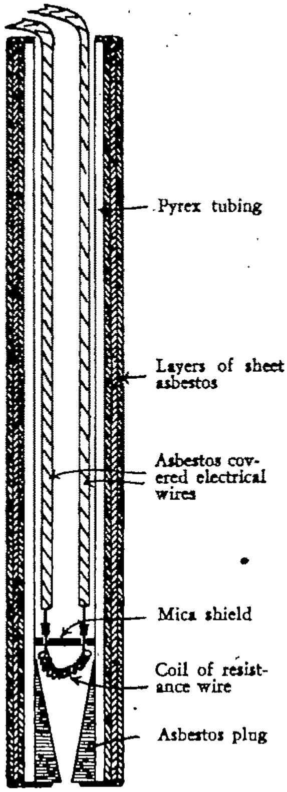

FIG. 1. DIAGRAM OF STIMULATOR

At the beginning of an experimental period, *O* seated himself in a comfortable chair placed sidewise at the end of a table. The arm (hand and forearm) next to the table was placed behind a screen, and in a plaster of Paris arm-rest that had been cast especially for it. The experimental area was thus kept relatively free from accidental movements and tremors.

The back of the forearm was roughly divided into quarters, and a given quarter was stimulated only once during an experimental period. After every two trials *O* was given a short rest, during which he walked about the room, while *E* adjusted the apparatus for the next two trials.

Apparatus. The stimulator (see Fig. 1) consisted of a coil of 20-gauge resistance wire—the heating unit—placed 2.5 cm. from the lower end of a pyrex glass tube, 15 cm. long and 1.2 cm. in diam. The diameter of the lower end of the tube was reduced to 2 mm. by an asbestos plug,[16] which also served to keep the coil at a constant distance from the lower end of the tube. The tube was wrapped in several layers of sheet asbestos, the outermost of which extended around the bottom and top edges of the tube, while the electrical wires leading through the top of the tube were covered with asbestos, and the bare ends were separated by a mica shield. The coil was heated by a current of 6.5 v. and 6.5 amp. The apparatus was held vertically in the clamp of a universal support, and was lowered into position or raised by means of a rack-and-pinion gear.

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[16] Various diameters were tried, but smaller diameters did not permit sufficient heat to pass through the openings to arouse pain in the time that was at our disposal, while larger diameters gave areal pains which were not completely adapted even with excessively long stimulation. In one experiment, with a diameter of 1.2 cm., the pain was not completely adapted after 45 min. The pain waxed and waned, frequently seemed to be on the point of disappearing, but never entirely did so. The intensive increases were accompanied by shifts in the locus of the pain. The radiations apparently penetrated deeper and deeper into the skin, and as pain adapted in one part it arose in another. The greater the area stimulated, the less likelihood would there be, then, of all the parts coming to complete adaptation at the same time. That the radiation penetrated deeply into the tissues in the experiment just cited, is shown by the fact that, though never extremely painful, it resulted in a severe burn and in the destruction of the corium. The injury scarred in healing.

Observers. The *Os* were Dr. A. D. Glanville (*G*), research fellow in psychology; Mr. W. S. Neff (*N*) and Mr. J. W. Macmillan (*M*), graduate students in psychology; Mr. H. A. Sklarsky (*S*) and Mr. R. Fleischman (*F*), undergraduates; and the authors, who observed only in the preliminary experiments.[17] Of the *Os* in the main experiments, *G, N,* and *M* knew the general nature of the problem and were experienced in cutaneous observation, while *S* and *F* had only the information given them in the instructions, and were unpracticed in psychological observation.

Instructions. The following instructions were given at the beginning of every experimental period:

> At a 'Ready,' 'Now' signal, your arm will be stimulated. Describe the temporal course of your experience. Give particular emphasis to the report of the appearance and disappearance of pain. Relax and remain as motionless as possible during the course of the experiment. Close your eyes at the signal, and do not open them until told to do so.

Procedure. The cast in which *O* placed his arm was adjusted before every experiment so that the surface stimulated was horizontal. At "ready" the stimulator was brought to a position directly above this area, and at "now" it was lowered to a point 1 mm. from it.

E recorded *O*'s oral reports, together with the times read off from a stopwatch which was started simultaneously with *E*'s "now."

After a report of the disappearance of pain, stimulation was continued for 20 sec.[18] If the pain reappeared before the 20-sec. interval had elapsed, or if it did not fade out entirely, the stimulator was removed after 4–7 min., depending upon *O*'s susceptibility to injury. Excessive injury was thereby avoided.

Every *O* had a practice series of 5 experiments to familiarize him with the apparatus, task, and routine, and a main series of 25 experiments, conducted at uniform hours twice a week during April and May, 1933.

RESULTS

Normal course of experience. The normal and typical course of experience resulting from stimulation of the skin by radiant heat is illustrated by the following reports.

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[17] We varied, during the preliminary experiments, the size of the area stimulated (see footnote 16), the intensity of the stimulus (in so far as this was affected by the voltage of the heating unit and the distance of the coil from the skin), and the temporal and spatial intervals between stimulations. The constants described under Method and Procedure seemed to give the best results, but none of the conditions could be studied systematically without departing from the object of the present experiment. Every one of the factors could profitably, however, be made the subject of a separate study.

[18] Our procedure differed in this respect from that of Burns and Dallenbach, who did not remove the stimulus "until 20 sec. after *O*'s last positive report of an experience" (*op. cit.*, 115). Pain was followed in our experiments by warmth, which, unlike the residual pressures of their experiments, did not readily reach complete adaptation. Indeed, as we discovered in the preliminary experiments, the skin was blistered and severely injured without the intrudance of pain when attempts were made to prolong stimulation until 20 sec. after the disappearance of warmth—"the last positive report of an experience."

F: [Exp. 13, right arm, area 3]: (2″) warm; (20″) slight heat; (27″) itch; (29″) burning; (35″) itch increasing; (38″) burning itch; (42″) slight pain; (1′23″) steady burning pain; (2′4″) very slight pain but very hot; (2′47″) pain increasing; (2′55″) pain decreasing; (3′8″) no pain but warm; (3′28″, when stimulus was removed) warm.[19] [Retested 10″ after removal of stimulus, pain rearoused 22″ later.]

G: [Exp. 21, right arm, area 1]: (1″) warm; (30″) hot in small area; (37″) heat gone; (49″) heat back; (55″) heat gone; (1′3″) heat back; (1′14″) stingy heat; (2′28″) sting; (2′35″) promise of pain in a small spot, but not yet painful; (3′32″) pain and itch; (4′) pain increasing slightly; (4′7″) pain decreasing; (4′11″) pain increasing again; (4′19″) pain not constant; (4′23″) pain decreasing; (4′33″) not sure whether pain or just intense heat; (4′42″) no pain; (4′55″) itch and pain; (5′) pain increasing; (5′17″) pain decreasing; (5′22″) pain gone; (5′25″) heat almost gone; (5′42″, when stimulus was removed) warm. [Retested at 10″, pain rearoused 18″ later.]

M: [Exp. 4, left arm, area 1]: (1″) warm; (7″) more intense warm; (12″) warmer; (38″) warmer; (50″) hot; (54″) a little pain; (58″) burning pain; (1′1″) stronger pain; (1′7″) intense pain; (1′17″) itchy, burning pain; (1′44″) pain still stronger; (1′47″) tingling burning pain; (2′14″) very strong burning pain; (2′30″) pain a little less; (2′47″) a little less pain; (3′2″) very hot, only a little pain; (3′31″) pain almost gone; (3′46″) pain goes and comes back in little flashes; (4′10″) very hot, but only slightly painful; (4′51″) no pain; (4′58″) a slight pain has returned; (5′21″) no pain, just heat; (5′41″, when stimulus was removed) heat. [Retested at 10″, pain experienced 30″ later.]

N: [Exp. 5, left arm, area 1]: (1″) warm; (10″) intense warmth; (18″) more intense; (25″) warmth with a fringe of pain; (29″) sharp pain; (36″) pain sharply localized; (42″) flash of cold; (55″) sharp pain; (1′17″) heat with a fringe of pain; (1′37″) diffuse, intense, burning heat; (2′8″) fluctuating pain; (2′30″) hot; (2′48″) diffuse heat of lower intensity; (3′) just heat, no pain; (3′20″, when stimulus was removed) intense warmth. [Retested at 10″, pain experienced 20″ later.]

S: [Exp. 24, right arm, area 4]: (1″) warm; (33″) warmth with slight pain; (35″) increase in pain, warmth slight; (48″) pain increased; (1′) pain and heat; (1′8″) pain decreased; (1′15″) pain increased; (1′57″) pain decreasing, no heat, only warmth; (2′16″) no pain, slight warmth; (2′33″) very slight pain; (2′59″) very slight pain and warmth; (4′25″) no pain, just warmth; (4′45″, when stimulus was removed) slight warmth. [Retested at 10″, pain experienced 15″ later.]

The experiences aroused by radiant heat begin with warmth. This quality is reported first by all *O*s in all the experiments. From this point on, experience deviates but slightly in the different experiments from a general course passing through intensive gradations of warmth to heat, and through similar gradations of heat to weak pain; the pain then grows in intensity, remains constant at a high degree for a short time, and gradually declines and disappears. The decline of pain is accompanied by the reappearance of heat and of warmth, the latter persisting after pain has disappeared, and frequently even after the removal of the stimulus.

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[19] The numbers in parentheses in this and the following reports show the time that has elapsed between the beginning of the experiment and the report they accompany.

The typical course of the experience may be divided into three phases: (1) a fore-phase, beginning with the application of the stimulus and ending with the arousal of pain; (2) a mid-phase, running from the appearance of pain, through its growth, to the beginning of its decline; and (3) an end-phase, which begins with the decline of pain and ends with the interruption of the experiment.

Arousal of pain. Pain did not appear early in any of the experiments. The average time required for its arousal varied, as Table I shows, from 40.3 ± 16.5 sec. (for *S*) to 59.4 ± 41.7 sec. (for *G*). The separate times ranged from 5 to 212 sec.

TABLE I

SHOWING THE AVERAGE TIME IN SEC. REQUIRED FOR THE AROUSAL OF PAIN, THE M.V., AND THE RANGE OF DISPERSION

O	Av.	m.v.	Range
F	51.1	26.8	14–151
G	59.4	41.7	6–212
M	44.1	19.6	7–123
N	41.9	23.6	5–161
S	40.3	16.5	5–75

Not only was pain slow in coming, but it was always mild and weak on its first appearance, as the following characterizations of the first pains, in every one of the experiments with every *O*, clearly show.

F: itch (10); slight itch (8); ticklish burn (2); slight burn (1); slight ticklish pain (1); slight pain (1); slight burning pain (1); very slight itch (1).[20]

G: slight pain (6); mild pain (3); small pain (3); stingy pain (2); hint of pain (1); sting (1); small point of pain (1); slightly stingy (1); itch (1); very slight pain (1); and 5 cases in which the first pain was not specifically characterized, though the next and subsequent reports of pain show that the first was unintense. For example, consider *G*'s reports in Exp. 15: pain appeared at 35 sec. when it was described merely as "pain;" at 40 sec., he reported that it was "increasing;" at 45 sec., "still increasing;" at 49 sec., "increasing in intensity;" at 59 sec., "stabbing pain;" at 65 sec., "still increasing;" etc. This progression can only mean that the first pain was mild or weak.

M: slight pain (13); very slight pain (7); a little pain (1); a flash of pain (1); and 3 cases in which the first pain was not characterized, but was shown by the next and subsequent reports to be weak.

N: slight pain (12); very slight pain (3); itch (1); a kind of pain (1); a point of pain (1); tingle of pain (1); fringe of pain (1); and 5 cases not characterized but shown by subsequent reports to be weak.

S: slight pain (10); very slight pain (7); extremely slight pain (1); flash of pain (1); and 6 cases not characterized, but shown by the next and following reports to be mild or weak.

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[20] The numbers in parentheses indicate the frequency of use.

These results clearly show that our stimulus was not of a kind to produce a *plötzlich einbrechender starker Schmerz.* We appear, therefore, to have met successfully the conditions laid down by Goldscheider as necessary for the perception of the *unterschmerzliche Empfindungen.*

Pressury experiences. Despite the fact that our conditions were apparently optimal for the observation of pressury experiences during the period before the arousal of pain—the period during which, according to Goldscheider's theory, the pressures should be both numerous and insistent—our *O*s reported pressury experiences in only 8 of the 125 experiments. Of these exceptions (2 with *F*, 4 with *N*, and 2 with *S*—*G* and *M* did not report pressures antecedent to pain in a single experiment), none indicates that pain arose from or because of a summation of the pressury experience.

F reported "tickle" before pain in Exp. 2 and 4; *N* reported a "pressury sensation" in Exp. 1, "tickle" in Exp. 7 and 20, and "tingle" in Exp. 15; and *S* reported a "dull pressure" in Exp. 13, and a "slight pressure or contact" in Exp. 17. Their reports in these experiments, from the stimulation to the arousal of the first pain, were as follows.

F: [Exp. 2, left arm, area 3]: (3") warm; (15") hot; (25") hot and tickle; (36") slight ticklish pain; (43") increasing pain. [Exp. 4, right arm, area 4]: (10") warm; (28") hot and tickle; (38") ticklish burn; (43") same plus pain.

N: [Exp. 1, left arm, area 4]: (5") warm; (50") peculiar pressury sensation; (1'10") kind of pain as though skin were being drawn up, a pulling sensation; (1'43") a sharp pain is now merged with this. [Exp. 7, left arm, area 2]: (3") warm; (45") tickle; (47") itch; (49") pain. [Exp. 15, left arm, area 3]: (3") warm; (41") warm increasing; (1'12" tingle of pain; (1'14") burning pain. [Exp. 20, right arm, area 1]: (1") warm; (20") tickle; (25") pain; (29") pain increasing; (31") increasing; (38") still increasing; (40") sharp pain.

S: [Exp. 13, left arm, area 4]: (1") warm; (3") heat; (18") dull pressure; (44") slight pain. [Exp. 17, left arm, area 4]: (3") warm; (28") slight pressure or contact; (48") slight pain.

"Tickle," "draw," "tingle," and "contact" are beyond doubt pressury experiences, but there is no evidence for the belief that they are related to pain, or that pain is in any way dependent upon them. Pain was aroused in 117 of the 125 experiments without the intrusion of pressure. Since pure, pressureless pains occurred so frequently (in 94% of our experiments), and since our results corroborate those obtained by Von Frey, we feel justified in concluding with him that Goldscheider's summation theory of pain is untenable.

This conclusion is supported by the results of the second and third phases of the experiments. During the second, the pain-phase, 2 of our

*O*s reported pressures in 12 experiments (9 by *N*, and 3 by *S*) after the appearance of pain or together with it. These instances are to be sure not numerous; but if pain is, as Goldscheider claims, the summation of pressures, we should not expect the sum to appear before the parts, nor the sum and the parts to co-exist. Such reports, as are illustrated by the following excerpts, are inconsistent, therefore, with Goldscheider's theory.

> *N*: [Exp. 6, left arm, area 4]: (2′20″) a little pain mixed with pressure and warmth. [Exp. 9, left arm, area 3]: (38″) very sharp pain mixed up with burning heat; (64″) addition of drawing pressure quality. [Exp. 15, left arm, area 3]: (1′12″) tingling pain; (1′14″) burning pain with lots of heat in it; (1′40″) now pressure in it; [Exp. 19, left arm, area 3]; (7″) slight warm, (15″) pain; (20″) pain increasing; (28″) intense pain; (35″) increasing; (53″) very sharp pain; (1′28″) very lively pain; (1′42″) a lot of drawing pressure in it.
>
> *S*: [Exp. 11, right arm, area 3]: (1″) warm; (11″) pain and warmth; (35″) pain and heat; (42″) pressure added; (1′5″) still pressure, pain, and heat. [Exp. 12, left arm, area 2]: (3″) warm; (32″) very slight pain; (45″) pain more definite but still slight; (1′5″) pressure came in; (1′20″) pressure, pain, and heat. [Exp. 22, left arm, area 3]: (4″) warm; (55″) slight pain; (60″) pain increasing; (1′8″) the slight pain increasing; (1′18″) pain remaining constant; (1′30″) I think there is pressure with it now.

During the third, the end- or adaptation-phase of the experiments, pressury experiences were reported 11 times by 3 *O*s (4 by *G*, 5 by *N*, and 2 by *S*); but only 5 of these (1 by *G*, 3 by *N*, and 1 by *S*) were reported after the disappearance of pain. If Goldscheider's theory of pain were correct, we should expect pain always to be followed by pressury experiences. As we shall return to this point presently, it will suffice here merely to note the inconsistency between fact and theory.

The pressury experiences noted in these experiments offer no support for the summation theory of pain. Whence do the pressures come, and how and by what are they aroused? We have no knowledge which will enable us definitely to answer these questions, but we believe that they are incident to heat. Not only were the pressury attributions chiefly used by our *O*s in connection with "hot," "heat," and "burn," but the analyses of heat that have been made reveal that it is itself "an astringent quality that lies in the qualitative continuum between pressure and pain."[21] Moreover, our stimulus may not have been altogether pressure-free. "It is possible," as Dimmick has pointed out, "that the radiant heat causes the skin to contract, and thus produces a direct stimulation of the organs of pressure."[22]

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[21] N. C. Burnett and K. M. Dallenbach, The experience of heat, this JOURNAL, 38, 1927, 422. Cf. also F. L. Dimmick, On the localization of pure warmth sensations, *ibid.*, 26, 1915, 145; F. Cutolo, A preliminary study of the psychology of heat, *ibid.*, 29, 1918, 445 f.; J. H. Alston, The spatial condition of the fusion of warmth and cold in heat, *ibid.*, 31, 1920, 311; S. C. Ferrall and K. M. Dallenbach, The analysis and synthesis of burning heat, *ibid.*, 42, 1930, 72-82.

Adaptation of pain. According to the criterion of adaptation which we accepted for the purposes of this study—namely, the disappearance of pain for an interval of 20 sec.—complete adaptation occurred, as Table II shows, in 76 (60%) of our experiments—in 19 for *F*, 10 for *G*, 15 for *M*, 11 for *N*, and 21 for *S*. We chose this rather rigorous criterion (1) so as to bring our results into line with those of Burns and Dallenbach, who employed a similar criterion; and (2) so as to obviate all questions regarding accuracy of observation, the reason for their original choice, since disappearances for 20 sec. can hardly be explained away by imagination or distraction.

TABLE II

SHOWING THE NUMBER OF EXPERIMENTS IN WHICH PAIN DISAPPEARED AND THE NUMBER OF TIMES PAIN DISAPPEARED FOR 20 SEC. OR FOR SHORTER INTERVALS

O	No. experiments		Duration		Total no. cases
	disappeared	did not	20 sec.	less than 20 sec.	
F	22 (88%)	3 (12%)	19	26	45
G	21 (84%)	4 (16%)	10	25	35
M	23 (92%)	2 (8%	15	29	44
N	17 (68%)	8 (32%)	11	32	43
S	21 (84%)	4 (16%)	21	4	25
Total	104 (83%)	21 (17%)	76	116	192

Had we chosen a less exacting criterion, more cases of adaptation would have been recorded; for pain disappeared, as Table II also shows, 116 times for intervals of less than 20 sec. (26 for *F*, 25 for *G*, 29 for *M*, 32 for *N*, and 4 for *S*). The duration of these cases, examples of which are given in the excerpts below, varied from 2-19 sec., the average duration being about 10 sec.

F: [Exp. 20, right arm, area 1]: (2′54″) pain; (3′10″) no pain; (3′29″) pain; (3′50″) no pain; (4′5″) pain; (4′27″) pain decreasing; (4′52″) no pain; (5′6″) pain; (5′25″) no pain; (5′45″ stimulus removed). [Retested at 5″, pain reëxperienced 12″ later.]

G: [Exp. 4, right arm, area 2]: (3′36″) pain increasing; (3′45″) just barely there; (3′50″) it's gone; (3′53″) back again; (4′7″) gone; (4′11″) back; (4′15″) it surges up and down, appears and disappears; (5′5″) slight increase, fairly intense; (5′15″) gradually fading; (5′23″) increasing; (5′33″) going gradually; (5′40″) almost gone; (5′52″) back again; (6′1″) fading; (6′5″) pain gone; (6′25″ stimulus removed). [Retested at 10″, pain reëxperienced 15″ later.]

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[22] F. L. Dimmick, On the localization of pure warmth sensations, this JOURNAL, 26, 1915, 145.

M: [Exp. 18, left arm, area 1]: (2′) very strong pain; (2′8″) less pain; (2′14″) less pain; (2′22″) dull pain; (2′28″) still decreasing; (2′45″) just slight pain; (2′55″) increasing again; (3′2″) decreasing; (3′9″) pain gone; (3′13″) back, very slight; (3′25″) pain gone; (3′35″) slight pain, comes and goes; (3′48″) pain gone; (4′7″) slight pain; (4′15″) increasing; (4′20″) decreasing; (4′26″) pain increasing; (4′36″) still increasing; (4′45″) pain intense; (5′ stimulus removed).

N: [Exp. 23, right arm, area 4]: (4′2″) pain and heat; (4′10″) very slight thrilling pain; (4′22″) pain gone; (4′33″) back; (5′3″) intermittent slight pain; (5′47″) gone; (5′58″) pain; (6′3″) gone; (6′10″) slight pain; (6′16″) gone; (6′26″) pain again; (6′37″) gone; (6′50″) slight needle of pain that comes and goes; (7′15″) pain fading, alternates with heat; (8′5″) steady and increasing pain; (8′30″) definite pain; (8′40″) increasing; (9′ stimulus removed).

S: [Exp. 19, right arm, area 4]: (1′15″) pain increasing; (1′57″) pain decreasing, no heat only warmth; (2′16″) no pain, slight warmth; (2′33″) very slight pain and warmth; (4′25″) no pain, just warmth; (4′45″ stimulus removed) warmth. [Retested at 10″, pain reëxperienced 15″ later.]

Our experiments, it will be recalled, were not of a fixed length. The stimulus was continued until a 20-sec. period of adaptation was obtained, or until it appeared that the skin was about to be grievously injured. Whether disappearances of 20 sec. would have been obtained in all experiments if stimulation had been prolonged, we seriously doubt, since the effect of radiant heat, unlike that of punctiform stimuli, is not confined to a definite point. The heat rays, penetrating deeper and deeper into the tissues and dispersing more and more widely through the skin, must stimulate an increasing number of nerve endings. Thus it could be only by chance that an interval of 20 sec. would elapse between the adaptation of pain at one nerve ending and its arousal at another. The experiments in which the disappearance of pain was not reported may, therefore, be interpreted as instances in which new nerve endings were stimulated before adaptation had run its course. Regarded in this light, the number and percentage of cases of complete adaptation we obtained is extremely high. In any case, it is high enough to warrant the conclusion that the pain of radiant heat is adaptable.

TABLE III

Showing for the Experiments in which Complete Adaptation of 20 Sec. Occurred, the Average Time in Sec. Required for the Adaptation of Pain after Its First Arousal, the M.V., and the Range of Dispersion

O	No. cases	Av.	m.v.	Range
F	19	46.3	36.9	8–146
G	10	104.1	83.0	2–241
M	15	135.9	63.1	38–319
N	11	122.5	56.1	10–212
S	21	97.0	54.3	1–187

The time from the first arousal of pain to its complete adaptation varies considerably, as Table III shows, from 8-146 sec. for *F*; 2-241 sec. for

G; 38-319 sec. for *M*; 10-212 sec. for *N*; and 1-187 sec. for *S*. The average adaptation-times show large individual differences, varying from 46.3 sec. (for *F*) to 135.9 sec. (for *M*). These deviations may be due to differences in the resistance of *O*'s skin to radiant heat, but the size of the m.v. and range of dispersion point rather to chance differences in depth of stimulation.

It must be remembered that complete adaptation is but one of several causes of the disappearance of pain, other possible causes are nerve block, destruction of the nerve ending, or distraction. As we have assumed above, distraction was eliminated by the adoption of the 20-sec. criterion, but there still remains the possibility of nerve block or destruction of the nerve endings—particularly the latter, as in spite of utmost precautions, the skin was frequently blistered and burned. In order to test these possibilities, we restimulated the points that had yielded complete adaptation (as the excerpts reported on pp. 234, 238f. show) at intervals ranging from 5 to 20 sec. after the conclusion of the principal experiment. ***In every case pain was reëxperienced;*** thus, the 20-sec. disappearances reported in this study are true cases of adaptation.

As Table IV shows, the time from restimulation to the rearousal of pain varied considerably, ranging from 16 sec. (for *M*) to 43.3 sec. (for *G*). A comparison of the values in this tables with those in Table I—which gives the data for the first arousal of pain—reveals that the stimulation-times of the rearousals are on the average from 9.7 sec. (for *N*) to 28.1 sec. (for *M*) shorter than the first arousals (see last column, Table IV). These results are due, we believe, to a "softening effect" of the first stimulations. We venture this suggestion, first, because the correlations between the times of arousal and rearousal are positive, though low, for all our *O*s. As computed by Spearman's Foot-Rule, $R = 1 — [6\Sigma g/(n^2 — 1)]$, the coefficients are 0.21 for *F*, 0.04 for *G*, 0.20 for *M*, 0.29

TABLE IV

Showing for the Experiments in which Complete Adaptation of 20 Sec. was Obtained, the Average Time in Sec. Required for the Rearousal, the M.V., and Range of Dispersion, together with the Difference between the Time of Arousal and Rearousal

O	No. cases	Av.	m.v.	Range	Difference between arousal and rearousal
F	19	28.3	15.2	5–96	22.8
G	10	43.3	23.7	15–113	16.1
M	15	16.0	3.0	7–47	28.1
N	11	32.2	15.4	12–70	9.7
S	21	27.8	14.7	5–90	12.5

for *N*, and 0.29 for *S*. Secondly, as the m.v. and the range of dispersion show, the lower average time of the rearousals was obtained chiefly through the shortening of the upper range of dispersion. By comparing Tables I and IV we find that the upper range of the times of rearousal is considerably shorter for 4 *O*s, while the lower range is approximately unchanged, being identical for *M* and *S*, slightly larger for *G* and *N*, and slightly smaller for *F*. Since the "softening effect" would be proportional to the length of the first stimulation, these results—a low positive correlation, a smaller m.v., and a shortening of the upper range of dispersion—are the very results we should expect. Long primary stimulations would have great effect, short ones hardly any at all.

The normal and most frequent course of adaptation was, as we observed above, a gradual decline in intensity and a disappearance of pain. The decline was accompanied by the reëmergence of heat and of warmth, the latter persisting after the disappearance of pain and usually until the removal of the stimulus.[28] Pressury experiences, which should according to Goldscheider inevitably follow pain in the course of adaptation, were reported in but 5 experiments. These 5 cases—as well as all the other instances of complete adaptation—were, however, closed by reports of warmth. The pressures reported were mild and weak, as were also many of the warmths. Our experiments, therefore, are not subject to the criticism that the intensity of the end-phase masked or obscured the "*unterschmerzliche Empfindungen*." Had these experiences been present, they would have been reported more frequently by our *O*s. That they were reported in only 5 out of 76 experiments showing complete adaptation, while warmth was reported in all of them, is further evidence, we believe, for the untenability of Goldscheider's pressure-theory of pain.

The results of this study might lend support to a new theory that pain is a summation of *unterschmerzliche Wärmeempfindungen;* but we prefer the conclusion that the accompaniments of pain are functions of the type of stimulus employed in its arousal. When a pressure-pain stimulus is used, residual pressure-effects are noted. When a warmth-heat-pain stimulus is used, residual warmth-effects are noted. It is our belief, furthermore, that if a cold-pain stimulus were employed, residual cold-effects would be noted. However that may be, sufficient facts are at hand to justify us in concluding with Von Frey, that pain is a separate modality of experience which may, under proper conditions, be completely adapted without undergoing any qualitative changes.

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[28] These results, it is interesting to note, stand in direct opposition to those reported by Heinbecker, Bishop, and O'Leary (Analysis of sensation in terms of the nerve impulse, *Arch. Neur. & Psychiat.*, 31, 1934, 34-53). These authors state that "burning pain from a hot object lost its quality of being hot before that of being painful, and finally only touch was recognized" (p. 42). They do not, however, tell us anything about the conditions under which their observation was made. Lacking these essential items of knowledge, we can do no more than to note the discrepancy, and to point out that their results fall in line with Goldscheider's. We should also point out that their use of psychological terms is both naïve and ambiguous.

The conclusion that pain is a separate modality with nerve fibers of its own is corroborated by the results of recent experiments in the field of neurophysiology. Gasser and Erlanger,[24] and more recently Waterston,[25] have demonstrated that pain is subserved by specific nerve fibers. Though these authors are not in complete agreement regarding the details of the correlation between fiber and function, they accord in the view that separate nerve fibers "convey the sensations of pain."

SUMMARY

Under the conditions of this experiment, which successfully meet Goldscheider's strictures against the use of radiant heat as a stimulus of pain, we find:

(1) That pure, pressureless pains are aroused by radiant heat.

(2) That these pains disappear without undergoing qualitative changes.

(3) That the disappearances, as the restimulation and rearousal showed, are real cases of sensory adaptation.

(4) That warmth is an invariable accompaniment in the fore- and end-phase of the pain aroused by radiant heat.

(5) That pressury experiences are not inherent in pain as they were neither the invariable nor constant accompaniment of the pain aroused by radiant heat.

In view of these results we feel justified in concluding:

(1) That Goldscheider's theory of pain is untenable.

(2) That the accompaniments of pain—pressure, as in the earlier experiments, and warmth, as in this experiment—are functions of the type of stimulus employed. A warmth-heat-pain stimulus has a residual warmth effect when pain is adapted. A pressure-pain stimulus has similarly a pressure effect.

(3) That pain is, as Von Frey contends, a separate modality of experience which may, under proper conditions, be completely adapted without undergoing any qualitative changes.

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[24] H. S. Gasser and Joseph Erlanger, The rôle of fiber size in the establishment of a nerve block by pressure or cocaine, *Amer. J. Physiol.*, 88, 1929, 581-591.

[25] David Waterston, Observations on sensation: The sensory functions of the skin for touch and pain, *J. Physiol.*, 77, 1933, 251-257.

AN EMPIRICAL COMPARISON OF AUDITION, VISION, AND TOUCH IN THE DISCRIMINATION OF SHORT INTERVALS OF TIME

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The purpose of this research was to make an investigation of time discrimination in the three main sense modalities: vision, audition, and touch. Much research has been done in the field of audition, and some in vision and touch, but practically no work has been done toward a comparative study of the three sense modalities. Miss Gridley,[1] using the Seashore phonograph record for the measurement of the "sense of time," instrumentally communicated to the *O* by the teletactor, compared hearing and touch and found that her *O*s made 5% higher scores in the auditory situation than in the tactual. Most research has followed the plan of varying the length of the intervals studied under constant conditions. In this research we have decided to use one length of interval (1 sec.), and to study it under varying conditions, using different techniques, and the three sense modalities.

Apparatus

Observer's room. All the observations were made in a small room approximately 6 x 9 x 9 ft. The room was well ventilated and the temperature kept about 68°. The room was painted pale green and experiments were done under reduced illumination. The window shade was raised 18" from the sill; when experiments were done at night, a 5 Watt lamp furnished illumination.

The stimuli were of 3 main types, auditory, visual, and tactual. For the source of auditory stimuli, a Western Electric receiver unit (56 ohm impedance) was mounted on the wall about 4′ in front of *O*. The regular stimuli were short "clicks" which were quite loud.[2] By introducing resistance into the line, the intensity could be reduced to the limit of audibility. A 4-watt commercial neon lamp (110 volt) supplied stimuli for vision. The neon lamp was inside a 4-in. tube, the open end of which was covered with several layers of tissue paper. This served to dim the visual stimuli and prevent definite fixation such as on the filament of the lamp. The stimuli

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* Accepted for publication September 6, 1932. The author is indebted to Professor R. H. Gault of Northwestern University for proposing this problem and for many helpful suggestions during its course.

[1] Pearl Gridley, The discrimination of short intervals of time by finger-tip and by ear, this Journal, 44, 1932, 18-43.

[2] A rough estimate of the intensity can be made by an experiment in which the receiver was placed outside and away from reflecting walls. It was audible to a person with normal hearing within a distance of 80 ft.

appeared as an illuminated surface. A teletactor unit furnished stimuli for touch.[3] It made some sound which of course had to be eliminated. Stopping *O*s ears made him unconscious of any auditory stimuli, but there was still the possibility of subliminal stimuli which might influence his judgment. Consequently we secured two concrete boxes, one inside the other. The outside box (32 in. cube) contain an inside box (24 in. cube) which rested on springs and was lined with 2 in. of cotton. The teletactor unit was suspended on springs from the top of the inside box. The wires through which the teletactor unit was activated were laid in the concrete before it was set. Between the two boxes and between the teletactor unit and the inside boxes the wires were carefully coiled so as to prevent the conduction of any sound out over the wires. The *O*, in order to feel the teletactor, thrust his arm through a hole 3½ in. in diam., which was padded on both sides of each box by a collar of rubber sponge. This precluded all possibilities of auditory clues from the teletactor, as evidenced by a test of subliminal stimulation which will be reported later.

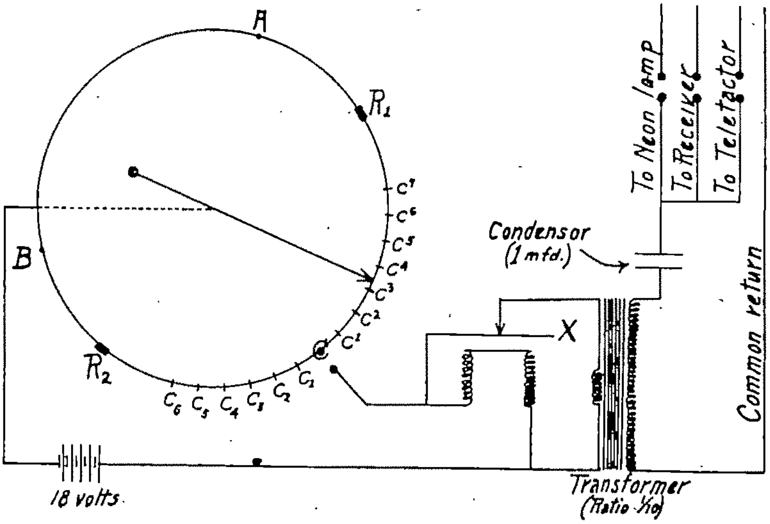

FIG. 1. WIRING DIAGRAM OF APPARATUS.

The receiver unit, neon lamp, and the teletactor unit were all activated by short electrical impulses which could be spaced at any desirable interval between 700 and 1300 σ by *E* in another part of the laboratory. In order to reduce the duration of the stimulus to a minimum an automatic circuit breaker was inserted between the distributing board and *O*. Thus as the batteries were connected, the magnet was energized and contact (Fig. 1) was immediately broken. Furthermore, a step-up transformer (ratio 1-10) increased the potential, and also prevented any continuous flow of current. A one microfarad condenser in the line eliminated everything but the peak of the wave. This impulse gave very clear sharply defined stimuli, the intensity of which could be varied by means of a rheostat.

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[3] The teletactor unit is a powerful low impedance magnetic vibrator which is used by Dr. Gault in connection with the tactual interpretation of speech.

As a variation, the apparatus was designed so that continuous stimulation could be given and the electric impulses served to break the circuit for an instant. For example, in the visual field, the neon lamp could be lighted steadily, and the intervals of time marked off by very brief flickers. This was accomplished by connecting a 110 volt A.C. line, through a 25 watt lamp to the neon lamp. The electric impulse distributor was connected across the line, so that it served to short-circuit the neon lamp for an instant, thus causing a flicker and marking off the desired interval of time.

A variation in tactual stimuli was accomplished by energizing the teletactor through the teletactor amplifier. As this is an A.C. amplifier, there is a slight steady hum in the teletactor unit. The impulses stood out plainly on this background of vibration. This variation in tactual stimuli was made with the thought that possibly the results from the 3 sense modalities might be more nearly comparable if a background of vibrations were provided in the tactual sphere, since in both the auditory and visual fields we had a background of sound and light respectively.

All instructions were given to *O* in writing. This was accomplished by a board 9 in. by 14 in. containing 10 small electric lamps on the left margin beside which instruction cards were placed. *E* indicated by a light the particular set of instructions that he wished *O* to follow. *O* held a small block on his lap which contained two keys by which he made all his responses. Telephone connection was available if needed.

Experimenter's room. *E* worked in a small room in another part of the laboratory. There were three walls between *O*'s room and *E*'s so that the operation of *E*'s apparatus was not audible to *O*.

The chief equipment was an electric impulse distributor which marked off intervals of time (the term 'interval of time' is used here to designate the period between two stimuli) varying from 700 to 1300 σ in steps of 20 σ. The distributor consisted of a brush 21 in. long which was rotated by a synchronous motor (1800 R.P.M.) geared down to 20 R.P.M. Thus the brush made a complete revolution every 3 sec. The circumference being about 132 in., the brush moved 0.0421 in. or approximately 1/24 in. in 1 σ. The spacing of the posts was accurate to 1/32 in. so that it may be safely said that the instrument was accurate to ± 1 σ. The only possibility of error beside the placing of the contact posts was in the speed of the motor. Being a synchronous motor, its speed depended upon frequency of the current which, during the past year, has not varied more than 0.08%. Contact posts were placed at various positions on the circumference and each one was connected to a switch by which that particular post could be connected to the receiving apparatus. The brush was grounded to the frame of the motor which was in turn grounded. Since current was supplied by a central battery system in which the negative pole was grounded, this served as the negative side of the line. The receiving apparatus was connected to the positive side of the battery and to the electric impulse distributor. In the first part of the experiment, which employed the method of Right and Wrong Cases, the *O*s were asked to compare 2 intervals marked off by 3 stimuli (clicks, flashes, and punches as the case might be). That is, the question for *O* to decide was this: "Was the interval between the first and second stimulus longer or shorter than the interval between the second and third?"

The standard interval in this experiment was 1 sec. and the other interval varied from 1 to 1.3 sec. In this part of the experiment the standard interval was first half of the time and last the other half of the time. To accomplish this, a double-throw switch connected either R_1 or R_2 (Fig. 1) to a relay switch which would make or break the circuit to the observation room. For example, if the switch was thrown connecting R_1 to the relay, the relay was tripped when the brush touched it, thus connecting the distributor to the observation room. The brush then touched contact A which sent the first impulse, then, a second later, contact B, and finally contact C superscript (the particular one depending upon the length of the interval desired). When the brush came to R_1 again the line to the observation room was broken and the brush continued to rotate without sending any impulses until it struck R_1 again. Thus there was a little more than 3 sec. between each series of impulses. Now if *E* desired the standard interval to be last instead of first he could merely throw the double-throw switch, making R_2 energize the relay. This also connected the series of posts labeled C subscript to the line. By another series of switches *E* determined which of the C subscript posts was to be connected to the line. When the brush struck R_2, the relay connected the distributor to the observation apparatus. Then the C post which was connected gave the first impulse and A the second—an interval of more than 1 sec. The third impulse came from B, making the second interval the standard. As the brush struck R_2, the line was disconnected until the brush struck it the third time, or *E* turned the switch to R_1.

O responded by pressing one of two keys, depending on which interval he estimated to be the longer. These keys were wired in series with *E*'s switch which determined which interval was longer, so that if *O* pressed the proper key, it was recorded on an electric impulse counter. Another impulse counter recorded the total number of trials given. In the latter part of the research the electric-impulse counter refused to work accurately, so two signal lights were substituted, from which *E* recorded *O*'s responses.

In the part of this research in which the method of Average Error was used, some additional apparatus was necessary. Here *O* was presented with the two stimuli marking off the standard interval and was asked to produce the third by pressing a key at the proper time. The key operated a relay which produced a stimulus (click, flash, or punch) exactly like the regular stimulus. When *O* pressed the key, a contact was made and an impulse just like the impulse which came from the distributing board was sent into the line.

The key circuit was also connected to a series of small lamps which was connected to the various contact posts on the distributing board. *O* should respond as the brush passed contact post C. By watching the lights, *E* could tell when *O* actually pressed the key, and read directly the error made.

Preliminary Experiments

Auditory stimuli. In the description of the apparatus the exact circuit and electrical measurements have been described so that the experimental conditions can be reproduced exactly. As a means of explaining the approximate intensity of the click, we connected the receiver to an extension cord, took it outside away from the building, and measured the greatest distance a person with normal hearing could be from the receiver and still hear the clicks. This experiment was done at midnight when all was quiet. It was found audible up to a distance of 80 ft. A less intense click was produced by inserting 2000 ohms resistance into the line. This could be heard at a distance of 35 ft. Both clicks were clearly above the threshold in the experimental situation as the receiver was approximately 4 feet from *O*. A study using 5 *O*s was made to determine the effect of the different intensities on accuracy of discrimination.

Three different levels of discrimination were examined: one set of intervals in which the difference in length was well above *O*'s differential threshold, one set at the threshold, and a third set below the threshold. *O* was given the following instructions:

> You will hear 3 clicks marking off 2 intervals of time. One interval will always be 1 sec., and the other will be a little longer. If the second interval is longer, press the key marked 'L,' if shorter, press the key marked 'S.'

Then he was presented with sets of clicks of the maximum intensity, followed by 10 of the less intensity, and so on until he had made 120 judgments. It was found that the difference in intensity made no significant difference in the result: 2 *O*s showed better discrimination with the loud click, 1 showed no difference due to intensity, while the other 2 showed keener discrimination when the less intense clicks were used.

Visual stimuli. The source of the visual stimuli, as we have explained, was a small neon tube. We found that the flash from the neon tube was not always from the same part of the tube. Furthermore, it left a definite after-image. Therefore we mounted the tube inside a tissue paper covered frame 4 in. in diam. Then the stimulus appeared as a short faint glow of the tissue paper surface.

The question occurred to us as to whether a flash was the best type of visual stimulus, since our problem was the study of time discrimination in response to visual stimuli in general rather than to flashes; flashes of light being only one form of many possible visual stimuli. As an alternative visual stimulus, we connected the neon lamp so that it was constantly illuminated and the intervals were marked off by a just-discernible flicker. Thresholds were computed by three psychophysical methods; namely, Right and Wrong Cases, Average Errors, and Just Noticeable Differences. Eight *O*s served for this experiment and the differential threshold values showed no significant difference in the two forms of visual stimuli. Because it was more convenient, the flash was used throughout this research.

The next problem was to decide whether in the visual part of the experiment it was best to work in the dark or the light. Both lag and persistence of sensation are involved. Persistence of sensation is at a minimum with low intensities and light adaptation. However, lag is probably tending toward a maximum with light adaptation. The problem is extremely complicated and, according to Ferree,[4] depends on the nature and composition of the light, intensity, light or dark adaptation, and the individual *O*. We did not attempt to consider all the factors involved, being content to reduce the intensity to a minimum and use the conditions which gave the finest differential sensitivity. Eight *O*s served for this experiment and were given the following instructions:

> You will see 3 flashes of light marking off 2 intervals of time. One interval will always be 1 sec., and the other will be a little longer. If the second interval is longer, press the key marked 'L'; if shorter, press the key marked 'S.'

As in sound, 3 degrees of difficulty were used: one set of intervals in which the difference was above *O*'s differential threshold, one in which the difference was approximately at the threshold, and one in which it was below the threshold. The *O*s would be allowed to make about 20 judgments under light adaptation and then the room was darkened. While adaptation was taking place, *O* worked on another experiment. After about 5 min., we returned to the light-dark adaptation study, and gave *O* 20 trials under dark adaptation. This process was continued, at each change allowing time for adaptation, until each *O* had made 120 judgments. The same experiment was performed using the flicker instead of the flash. The results showed that for the flashes, discriminations were a trifle keener under light adaptation than under dark adaptation, but for the flicker, the difference was negligible. Consequently, all the experiments were done under light adaptation, although the illumination was slightly subdued.

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[4] C. E. Ferree in a personal letter.

Tactual stimuli. The tactual stimuli may be considered for all practical purposes to consist of a single movement of the diaphragm of the teletactor, because the chokes and condensors in the line, as described in the account of the apparatus, eliminated everything but the peak of the wave. At first we thought that possibly the punches ("punches" is adopted to refer to tactual stimuli felt over the teletactor) should be presented in a background of vibrations in order that the tactual situation might be more nearly comparable to the visual and auditory situation in which there was naturally a background of light and sound respectively. To supply this background the teletactor receiver was activated through the amplifier which gave a steady low-pitched hum. In this arrangement, the punches stood out clearly from the background of vibration. However, after trying it with 3 *O*s we concluded that the type of tactual stimuli made no apparent difference in accuracy of discrimination. Consequently, for the remainder of the experiment, we used only the discrete impulse hook-up, as it was more convenient.

One more investigation was necessary in connection with the tactual part of the experiment; namely, to ascertain whether the *O*s were receiving any auditory clue. Although they were not conscious of any sound coming from the soundproof box containing the teletactor, it was conceivable that the *O*s might be influenced by some subliminal auditory stimuli. To test this proposition, we set up a simple experiment. Four *O*s served in this test, and were given the following instructions:

> Put your arm into the box as you do in the experiment, but do not touch the diaphragm of the teletactor. Every 5 sec. the light will flash. Half of the flashes will be accompanied by stimuli over the teletactor. You are to guess each time the light flashes whether or not the teletactor has been activated.

None of the 4 *O*s was able to guess correctly more times than chance would allow. Therefore we were convinced that no subliminal auditory clues were operative in the tactual situation.

TECHNIQUE

The first attempt to measure differential sensitivity was by the Method of Right and Wrong Cases. The *O*s were presented with 3 stimuli marking off 2 intervals of time, and each one was asked to decide whether the second interval was longer or shorter than the first. The data to be presented later are based on approximately 14,000 judgments by this method.

Another approach was afforded by the Method of Just Noticeable Differences. Two intervals of time in which the second was considerably longer were presented to *O*, and the pair was repeated, each time decreasing the length of the second interval until the *O* reported they were equal. The second was further decreased until *O* reported a noticeable difference. The same process was repeated with an ascending series of values, until sufficient judgments had been made to compute a threshold. Over 1000 judgments were collected by this method.

The Method of Reproduction (Average Error) was employed in a set-up in which only the first intervals were determined by the impulse distributor, and *O* reproduced the second interval. The apparatus was so designed that the stimuli which *O* produced were quantitatively and qualitatively exactly like the regular stimuli. The motor response required of *O* to reproduce the interval was the depression of a key. By means of a chronoscope, *E* could read directly the error made by *O*.

Right and Wrong Cases. The first problem in connection with this technique was the determination of the effect of position of the interval in the pair being judged. In preliminary experiments, we found that out of 2385 judgments, 13% more correct judgments were made when the second interval was longer than the standard interval (1 sec.) than when it was shorter. This should be expected, as Woodrow pointed out,[5] because all of the intervals used here were above the indifference point and therefore gave the "absolute impression of long," even though they were relatively short. This would tend to minimize the difference when the second interval was shorter and exaggerate it when the second interval was longer. During the experiment, the second interval was shorter half the time and longer half the time, so that any error due to position was constant.

The problem of disposing of "equal" judgments has long been a topic of dispute. There are three possibilities: (1) to disregard them entirely in the final results; (2) to divide them equally between right and wrong judgments; and (3) to exclude them, limiting the *O*s to two categories of report.

The first solution (namely, to discard them from the results) is certainly unscientific. It is a selection of the data. By such a procedure, we should eliminate from ⅓ to ½ of the *O*'s judgments, as shown by a short experiment in which equal judgments were admitted. These *O*s had made several thousand judgments previous to this experiment and were consequently well trained. Furthermore, they were persons who were interested; they volunteered for these extra sittings.

The second method of disposing of the "equal" judgments; namely, to divide them equally between the correct and incorrect judgments, has some vogue. However, it can be argued that it is better to allow *O* to divide them between the two categories according to slight or even subliminal clues than for *E* to divide them equally.

An experiment in which equal judgments were allowed was repeated using 3 *O*s. Of the 1695 judgments, 42% were "equal." Thresholds were computed both by disregarding these "equal" judgments and by dividing them equally between the right and wrong responses. Table I shows considerable variation depending upon the method. It would be difficult to judge which figure is the nearest to the true threshold.

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[5] H. Woodrow, Behavior with respect to short temporal stimulus-forms, *J. Exper. Psychol.*, 11, 1928, 167-198.

TABLE I

Showing the Different Threshold: Obtained by Each of the Three Methods of Dealing with "Equal" Judgments

Equal judgments	Audition	Vision	Touch
(1) Not counted in final results	.074	.124	.087
(2) Divided between right and wrong	.078	.126	.088
(3) Not allowed	.059	.118	.094

As Woodworth says, there are practically no cases in which *O* cannot find some difference in two stimuli if he is required to search for it.[6] Therefore there can be little justification for the admission of the equality judgment. The number of equal judgments made always depends upon the standard of clearness or confidence *O* sets up as boundary between equal and different. An *O* who is cautious and insists on a clear difference before he makes the judgment longer or shorter will show a broad middle zone, where he is unable to decide the direction of the difference and so classifies the stimuli as equal. The same *O* by following every little clue and slight impression will greatly reduce that broad zone of equal judgments. Consequently the threshold will be reduced.

In view of the arguments and experimental evidence presented above we decided to use only the two-category plan in the remainder of our research. Some provision for doubtful cases was provided in that *O* was not compelled to respond every time, but was allowed a second presentation of an interval-difference if he was entirely unable to decide at the first presentation. However, this was not encouraged and the majority of *O*s used it only occasionally.

It will be recalled that the Method of Right and Wrong Cases was only one-third of this research, and since we had to determine psychometric constants for 3 sense modalities, some cut in the time required of volunteer *O*s had to be made. The only alternative possible would have been the use of one or two *O*s who might have been persuaded to give the necessary 30 hours for the experiment. Since the principal object of all this research was a comparison of the 3 different modalities, a larger number of *O*s seemed absolutely indispensable. In view of this fact, we decided to reduce by more than half the time required of our *O*s in this part of the experiment, by working with only 2 intervals rather than 5 or 7 as is customary in psychophysical techniques. During the training period (which was considerable) we estimated the threshold value. Then by using time values which would include the estimated threshold, we proceeded to take judgments on those time intervals. In a few cases, we found that the intervals did not include the threshold and therefore we had to add a third interval. The thresholds were computed by the formula: $\text{D.L.} = [D_a\ (B - 50) - D_b\ (50 - A)]\ /\ (B - A)$ where B represents the percentage of correct judgments at the interval D_b and A the percentage of correct judgments at the interval D_a. Of course this assumes that the curve rises at a uniform rate from D_b to D_a, which is not correct; but as the intervals D_b and D_a were quite close (20 σ apart) the error incurred by not considering the shape of the curve is slight. These data do not make it possible to compute *h*, the constant of precision, which would be an interesting bit of knowledge, but it can hardly be considered essential for our purposes. Some measure of the relative spread of the judgment is afforded by the slope (M) of the curve when the responses are plotted against the stimulus-values which is given by the formula: $M = dy/dx$. Table II shows the threshold values and the slope of the curve for each *O* in each modality.

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[6] R. S. Woodworth, The psychological researches of James McKeen Cattell, *Arch. Psychol.*, 4, 1914, (no. 30), 60-74.

TABLE II

Showing Thresholds and the Slopes of Curves for Data Obtained from the Method of Right and Wrong Cases

O	Auditory		Visual		Tactual	
	D.L.	M	D.L.	M	D.L.	M
1	.071	6.5	.215	4.5	.102	6
2	.053	5	.093	3	.073	3
3	.113	5.5	.130	5	.260	5
4	.091	6	.138	4.1	.177	2.7
5	.047	8	.091	8	.069	4
6	079	5.5	.167	6.5	.148	7
7	.033	5.5	.114	3	.104	5
8	.092	4	.159	6	.134	6
9	.096	8	.134	8.5	.096	9
10	.046	10.5	.095	2	.080	3
11	.074	5	.201	7	.163	4.5
12	.084	6	.118	3.5	.100	8.5
13	.037	5	.102	4	.069	0.5
14	.061	5	.106	5	.066	4.0
15	.064	2	.164	4.5	.115	6.2
16	.071	2	.130	5	.127	2
17	.093	3.5	.195	5	.094	3.5
18	.053	7.5	.108	9	.053	7
Av.	.070	5.5	.137	5.2	.100	4.8

Just Noticeable Differences. Starting with the second interval unmistakably longer than the first, it was gradually reduced until *O* reported that the intervals had become equal. The midpoint between the smallest interval perceived as longer and the longest interval perceived as equal was taken as the upper threshold (R_{ud}) for the descending series. The second interval continued to grow shorter by imperceptible decrements until *O* reported that it was shorter. The midpoint between the smallest interval perceived as equal and the largest perceived as shorter was the lower threshold (R_{ld}) for the descending series. The interval was made still shorter so that the difference was clearly perceptible, and gradually increased until *O* reported equal. A point halfway between the largest interval perceived as shorter and the smallest interval perceived as equal gave us the lower threshold (R_{la}) for the ascending series, which when averaged with the R_{ld} (lower threshold for descending series) gave a value which we considered the lower threshold (R_l). The interval was increased farther until *O* reported that the second interval was longer, and an upper threshold computed for the ascending series which, averaged with the value obtained in the descending series, gave the upper threshold (R_u). The two limens were computed 5 times (in several cases 10 and even 15 times) for each *O*, and the values given here are the average thresholds.

The value R_u above which an interval is perceived as longer, and the value R_l below which an interval is perceived as shorter, than the standard 1-sec. interval determine the boundary of the period of uncertainty.

As has already been point out, all of the intervals used in this study are long "absolutely;" *i.e.* they are all above the indifference point (approximately 0.6 sec.) so that they give the "absolute impression of long." Therefore, when the variable interval becomes less than the standard, it is overestimated, because it is long "absolutely," although short compared with the standard. As a result, R_l is farther below standard than R_u is above it. Therefore, probably the R_u would be a truer threshold than the average of R_u and R_l. The opposite point of view is just about as tenable. We will present all the data and let the reader take his choice.

The estimation value of the stimulus (R_e) is another psychophysical constant which will be used later in comparison with the average R obtained by the method of reproduction. The value, designated by R_e, is merely the average of all the 8 values obtained in computing the thresholds. Since thresholds were computed 5 times, each value (R_e) represents the average of 40 values.

Table III shows the upper and lower threshold values, average threshold value, and probable errors for each *O* as computed by the Method of Just Noticeable Differences.

Method of Reproduction. Our third approach to a comparative study of the 3 different modalities was by the Method of Average Errors, or more appropriately called in this research, the Method of Reproduction. *O* was given 2 stimuli marking off the standard 1-sec. interval, and asked to produce the third stimulus so as to make the first and second intervals equal. The third stimulus which *O* produced was exactly the same qualitatively and quantitatively as the first two produced by the electric impulse distributor. For each response *E* recorded the error and its direction. In this experiment, the only motor response required by *O* was depressing a key about 1/16 in. After our *O*s had practiced placing the third stimulus in the proper place, they were instructed to put their attention on the intervals which they produced rather than upon the actual pressing of the key. Thus we hoped to reduce the interference which the motor adjustment made with the placement of the final stimulus.

However, this method of reproduction is hardly comparable with the previous method, since here we are measuring not *O*'s discrimination but the precision with which he can reproduce an interval. There are certain interesting constants which can be calculated from these data. First of all, there is the length of the reproduced interval which is generally designated by R, and is given by the formula: $R = 1.00 - \Sigma E/N$; in which E represents the actual error made on each trial, and N the number of trials. From this value, we can compute C, the constant error, by the formula: $C = R - 1$.

A more significant factor is the average error, A.E., which is merely $A.E. = \Sigma E/N$ without regard for signs. Such a constant is not worth much without some measure of reliability, so we have included the probable error of the average for each *O*, which is $P.E._{av} = P.E./(N)^{1/2}$.

The average errors contain, in addition to a constant error (C) a variable error (V.E.) which may be computed by $V.E. = (E - C)/N$. The 5 constants just described have been computed for each sense modality and are given in Table IV.

TABLE III

Showing Psychometric Constants Obtained for Each Modality by the Method of Just Noticeable Difference

O	Audition				Vision				Touch			
	R_u	R_l	D.L.	P.E.	R_u	R_l	D.L.	P.E.	R_u	R_l	D.L.	P.E.
1	.040	.068	.054	.002	.052	.056	.054	.001	.052	.068	.060	.002
2			.036				.034				.033	
3	.100	.036	.068	.005	.110	.116	.113	.003	.100	.078	.089	.004
4	.092	.098	.095	.005	.116	.120	.113	.003	.096	.128	.114	.002
5	.032	.014	.023	.001	.180	.036	.108	.008	.100	.020	.060	.005
6	.034	.052	.043	.001	.124	.084	.104	.004	.044	.052	.048	.002
7	.100	.060	.080	.003	.252	.092	.172	.008	.260	.100	.180	.008
8	.100	.055	.077	.003	.090	.100	.095	.002	.085	.120	.102	.002
9	.080	.023	.076	.003	.134	.102	.118	.003	.106	.123	.114	.002
10	.040	.034	.037	.001	.066	.056	.061	.002	.056	.043	.049	.002
11	.126	.060	.093	.003	.166	.153	.159	.003	.126	.113	.119	.002
12	.036	.068	.052	.003	.084	.120	.102	.003	.084	.068	.076	.002
13	.028	.034	.031	.002	.067	.082	.074	.002	.059	.060	.054	.002
14	.084	.028	.056	.003	.120	.072	.096	.005	.100	.036	.068	.003
15	.068	.060	.064	.004	.060	.076	.068	.003	.048	.076	.062	.002
16	.124	.044	.084	.004	.188	.084	.136	.004	.148	.044	.096	.005
17	.076	.068	.072	.003	.148	.156	.152	.003	.132	.108	.120	.004
18	.036	.052	.044	.002	.100	.076	.088	.002	.060	.060	.060	.002
Av.	.070	.053	.061	.003	.121	.093	.103	.004	.097	.076	.084	.003

TABLE IV

Showing Psychometric Constants Obtained for Each Modality by the Method of Reproduction

O	Audition				Vision				Touch			
	A.E.	P.E.	V.E.	C.E.	A.E.	P.E.	V.E.	C.E.	A.E.	P.E.	V.E.	C.E.
1	.160	.001	—.100	.090	.161	.0024	.125	—.036	.114	.004	.86	.028
2	.064				.078				.078			
3	.071	.003	—.066	.005	.096	.0049	.045	.051	.096	.005	.060	.036
4	.070	.004	.060	.010	.115	.0062	.064	.051	.117	.004	.087	—.030
5	.044	.003	.035	.009	.057	.004	.022	.035	.094	.005	.021	.073
6	.068	.003	—.019	.049	.081	.004	.043	.038	.059	.004	.045	.014
7	.063	.002	.006	.057	.074	.006	.047	.027	.068	.004	.083	.003
8	.180	.009	—.098	.082	.256	.005	.000	—.128	.120	.005	.000	—.120
9	.083	.004	.062	.021	.111	.005	.052	.059	.089	.005	.040	.049
10	.091	.005	.074	.017	.104	.005	.100	.004	.093	.004	.047	.046
11	.102	.005	—.101	.001	.152	.004	.0000	—.152	.181	.000	.000	.181
12	.076	.003	—.035	.041	.117	.004	.069	—.048	.142	.004	.030	—.112
13	.045	.003	.038	.007	.117	.004	.017	.100	.049	.004	.023	.026
14	.051	.004	—.049	.002	.083	.005	.025	—.058	.079	.002	.004	—.075
15	.115	.005	—.032	.082	.118	.005	.031	—.087	.090	.003	.053	—.037
16	.129	.003	—.103	.026	.108	.004	.074	.034	.114	.004	.010	—.094
17	.125	.004	—.124	.001	.119	.005	.031	—.088	.140	.005	.003	—.137
18	.042	.002	.006	.036	.126	.003	.000	—.126	.074	.004	.010	—.064
Av.	.070	.004	—.026	.027	.107	.004	.044	—.023	.100	.004	.036	—.034

Comparison of the Psychological Methods Employed

The problem of the validity of the various psychophysical methods is not new in the history of experimental psychology. Kellogg,[7] in 1929, made an elaborate investigation using all three methods. He found that the Method of Reproduction was the finest measure of differential sensitivity since it gave the smallest variable error. The Method of Constant Stimuli gave the most reliable results, *i.e.* the smallest variation from sitting to sitting.

It is difficult to determine the relative validity of the three techniques used in this research because of the lack of adequate criteria. However, we can compare the results obtained from the different psycho-physical methods, and in that way infer something about their relative validity.

In the first place, all three methods give the same results in that they showed hearing to have the lowest differential threshold, and vision the highest. Table V shows the average results obtained from each method.

Another method of comparing the technique is by means of correlation coefficients which, because of the small number of cases, were computed by the Rank-Difference Method.

TABLE V

Showing Differential Threshold Obtained from Each Method for Each Modality

Method	Audition	Vision	Touch
Right and Wrong Cases	.070	.137	.100
Just Noticeable Differences	.061	.103	.084
Reproduction	.080	.107	.100
Av.	.070	.115	.095

TABLE VI

Coefficients of Correlations Showing the Relationship of the Different Technique

	R. & W. with J. N. D.	R. & W. with Reproduction	J. N. D. with Reproduction
Audition	.51 ± .12	.46 ± .11	.55 ± .12
Vision	.16 ± .15	.62 ± .10	−.08 ± .16
Touch	.38 ± .13	.53 ± .11	.73 ± .09

It will be observed that in audition, the three correlations are all sufficiently larger than their probable errors. In vision, however, a very different situation exists. The Methods of Reproduction and Just Noticeable Difference are correlated to the extent of —0.08, so obviously, both cannot be a reliable measure of differential sensitivity. We find that the Method of Right and Wrong Cases is correlated to a greater extent with the Method of Reproduction (0.62) but not with the Method of Just Noticeable Difference (0.16). This seems to cast doubt on the validity of the latter method as a measure of differential sensitivity in the field of vision.

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[7] W. N. Kellogg, An experimental comparison of psychophysical methods, *Arch. Psychol.*, 17, 1929, (no. 106), 1-86.

The Rank-Difference Method of correlation conceals much of the relationship because of the proximity of the threshold values, but, with only 18 cases, it was the only method possible. A truer picture of the real relationship can be had from an inspection of Table VII.

TABLE VII

Showing Differential Thresholds for Each Modality as Computed by Each of the Three Psychophysical Methods

O	R. & W. Cases			J. N. D.			Reproduction		
	A.	V.	T.	A.	V.	T.	A.	V.	T.
1	.071	.215	.102	.054	.054	.065	.117	.161	.114
2	.053	.093	.073	.036	.034	.035	.064	.078	.078
3	.113	.130	.099	.068	.113	.089	.071	.096	.096
4	.091	.138	.117	.095	.118	.112	.098	.107	.117
5	.047	.091	.069	.023	.108	.060	.044	.057	.094
6	.097	.167	.148	.043	.104	.048	.068	.081	.059
7	.033	.114	.104	.080	.174	.180	.063	.074	.086
8	.092	.159	.134	.077	.095	.102	.079	.256	.240
9	.096	.134	.096	.076	.118	.113	.083	.111	.089
10	.046	.095	.080	.037	.061	.049	.091	.104	.093
11	.074	.201	.163	.093	.159	.120	.102	.152	.181
12	.084	.118	.100	.052	.102	.076	.054	.096	.117
13	.037	.102	.069	.031	.074	.055	.045	.117	.049
14	.061	.106	.066	.056	.096	.068	.051	.083	.079
15	.064	.164	.115	.064	.069	.058	.113	.118	.090
16	.071	.136	.127	.084	.136	.096	.129	.108	.128
17	.093	.195	.094	.072	.152	.120	.125	.119	.140
18	.053	.108	.053	.044	.088	.060	.042	.126	.074
Av.	.070	.137	.100	.061	.103	.084	.080	.107	.100
A.D.	.018	.034	.029	.019	.033	.029	.024	.027	.030

A final method of comparing the reliability of the techniques is a study of the changes in the results obtained for 6 *O*s from two sittings a month or more apart, and is shown in Table VIII.

TABLE VIII

Showing the Average Changes in Differential Sensitivity Over a Period of a Month

Modality	R. & W.	J.N.D.	Reproduction
Audition	.016	.002	.059
Vision	.039	.012	.052
Touch	.037	.018	.064
Av.	.031	.011	.058

COMPARISON OF DIFFERENTIAL SENSITIVITY IN THE THREE MODALITIES

As has already been observed (Table V), all three techniques show audition to have the keenest differential sensitivity and vision the poorest, with touch lying midway between audition and vision.

Another comparison is afforded by a comparison of the variability of the *O*s for the different sense modalities. Just as the measures of central tendency show the order of differential sensitivity to be audition, touch, and vision, so the following table shows the amount of variability as measured by the average deviation from the mean to be in the same order.

TABLE IX

SHOWING THE AVERAGE DEVIATION FROM THE MEAN FOR EACH MODALITY AS MEASURED BY THE DIFFERENT METHODS

Method	Audition	Vision	Touch
Right and Wrong Cases	.018	.034	.024
Just Noticeable Differences	.019	.033	.027
Reproduction	.024	.029	.030
Av.	.020	.032	.027

It is interesting that keen differential sensitivity is accompanied by less variability, so that we can say that the greatest sensitivity and the least variability are found in audition, while the very opposite is true of vision. Touch lies midway between audition and vision on both scores—sensitivity and variability. The correlation coefficients in Table VI show that the degree of correlation among the different methods is quite constant for audition and quite variable for vision. As usual, touch stands midway between audition and vision. This would lead us to place most confidence in the measurement of auditory differential sensitivity and least in the visual field.

If we now turn to the individual *O*s, we can make a comparison of the variability in the different sense modalities. Here too, we find that the responses to the auditory stimuli are most reliable (as measured by the probable error of the average) and those to the visual stimuli least. The reliability of responses in the tactual situation is between that for vision and audition. Tables III and IV show the probable error of the average for each *O* for the Methods of Just Noticeable Differences and of Reproduction.

TABLE X

SHOWING THE P.E.av. OF THE AVERAGE FOR THE DIFFERENTIAL THRESHOLDS AS OBTAINED FOR EACH MODALITY BY THE METHODS OF REPRODUCTION AND JUST NOTICEABLE DIFFERENCES

(The P.E.av. for each will be found in Tables III and IV)

Method	Audition	Vision	Touch
Reproduction	.0041	.0047	.0042
Just Noticeable Differences	.0028	.0035	.0030

To summarize, we can say that on the average the differential threshold for vision is 65% higher and for touch 34% higher than the threshold for audition.

TABLE XI

Showing the Relative Value of Differential Thresholds for Each Modality as Obtained by the Different Techniques

Method	Audition	Vision	Touch
Right and Wrong Cases	1	1.98	1.44
Just Noticeable Differences	1	1.65	1.33
Reproduction	1	1.34	1.25
Av.	1	1.65	1.34

In comparing the variability and the reliability of the three modalities, we found the same order to hold. The following table shows the relative variability of the different sense modalities.

TABLE XII

Showing the Relative Average Deviations for Each Modality

Method	Audition	Vision	Touch
Right and Wrong Cases	1	1.88	1.33
Just Noticeable Differences	1	1.72	1.45
Reproduction	1	1.11	1.25
Av.	1	1.57	1.34

In brief, audition gives the keenest differential sensitivity, is least variable, and most reliable. Vision gives exactly opposite results while touch lies between audition and vision on all three scores.

PROBLEM-SOLVING IN DIFFERENT FIELDS OF ENDEAVOR

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The term reasoning, like many other terms in psychology, has been used with a variety of meanings. It will be the purpose of the first part of this paper to discover the essentials of reasoning by reviewing the literature on the subject and then to conclude with a definition. The second part will give the data and conclusions of experiments dealing with "problem solving" in eight different academic fields.

The belief in mental faculties as separate entities promoted by the phrenologists and their followers is gradually giving way to the belief in general mental functions. That is, memory, for example, is a general function of the whole brain rather than any limited part of it. This belief is supported by the experiments of such men as Franz[1] and Lashley.[2] Lashley maintains, as a result of his experiments on rats, that the brain operates as a whole, and a habit which has been thoroughly established may be taken over by other parts of the brain, and perhaps, by the brain stem. He also concludes from his experiments that with any general or complex type of behavior the amount lost in the behavior by the removal of parts of the brain is in direct proportion to the amount of the brain removed without reference to the location. Such experiments tend to refute the idea of highly localized functional areas for the higher and more complex functions. Thus, we may conclude from a study of such experimental evidence that the various functions of the brain are very general functions involving probably the whole cortical portion if not the brain stem.

This conception of the brain and its functions does not imply, however, that the different functions such as memory, imagination, and reasoning may not vary greatly in their relative efficiency in the same individual.

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* Accepted for publication August 12, 1932. This experiment has been carried through three different times: once at the University of Michigan in 1929, the results of which were submitted as a dissertation in partial fulfillment of the requirements for the degree of Doctor of Philosophy, and the other two times at the Western Kentucky State Teachers College in 1930 and 1931.

The author wishes to acknowledge his indebtedness to Professors W. B. Pillsbury and Carl Spearman and others who aided in the experiments and in the preparation of the manuscript.

[1] S. I. Franz and K. S. Lashley, The retention of habits by the rat after destruction of the frontal portion of the cerebrum, *Psychobiol.*, 1, 1917, 3-18.

[2] K. S. Lashley, *Brain Mechanism and Intelligence*, 1929, summary, 175-6.

That the brain functions as a whole does not oppose the idea that a given brain may vary markedly in its capacities to receive, to retain, or to relate. A certain brain may receive without retaining, or it may retain and recall without being able to relate; that is, to use in solving problems; but, if it retains well in one academic field, as this is a general function, it ought to retain well in any other, other things being equal. In other words, retention does not differentiate in favor of any field of material. Likewise, the brain that is apt in relating in one field may be relatively as efficient in all other fields, other things being equal. Therefore, the writer launched these experiments to test the thesis, "That a person will be as good a reasoner in one academic field as another providing he has an equal background."

ESSENTIALS OF REASONING

In order to secure a definition of reasoning consistent with the various authorities and to discover the conditions of reasoning, the steps in reasoning, and the essentials or nature of the reasoning process, the literature on reasoning has been reviewed.[3] As a result of this review we may make the following summary.

(1) The essential conditions for reasoning are: first, a problem; secondly, sufficient experiences with which to solve the problem; and thirdly, the individual must possess sufficient ability to rearrange those experiences in new relations to meet the new situation.

(2) The formal steps in reasoning as found in the literature are: a problem; an understanding of the problem, or judgment; the solution of the problem, or inference; a conclusion; a belief in the conclusion; verification; proof; and application. From the psychological point of view, the last three are not parts of the real reasoning process; but are follow-up processes for the purpose of testing or checking up on the conclusion, which really ends the reasoning process. Verification is but supplementary proof; application is but practical proof; and induction and deduction are the logician's method of proof.

(3) Thus, we may conclude that the essentials of reasoning may be reduced to: a problem, an understanding of the problem, and the solution of the problem, which presupposes essential experiences and ability.

(4) Finally, we may define reasoning as the solving of a practical or theoretical problem or difficulty by the use of or through the relating of past experiences, the end result of which is believed, no matter how fallacious or inconsistent it may seem to others. One accepts it as true because it is consistent with what one knows. (Frequently, our *S*s who failed completely in solving the problems were very positive of their results.)

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[3] A fuller treatment of this subject may be obtained from the University of Michigan in the original thesis.

EXPERIMENTS IN PROBLEM-SOLVING

Object. The following experiments were planned to test the assumption that problem-solving, which involves the mental relating of experiences, is a general function and constant within a given individual, other things being equal.

To do this 146 *Ss* in groups of 50, 46, and 50 were ranked according to their ability to solve problems in eight academic fields: geometry, arithmetic, physics, mechanics, economics, sociology, geography, and history. In order to test ability and not past experiences, all of the materials necessary for solving the problems in each of the fields were thoroughly taught. Then the ability to solve problems with these materials was tested according to the usual classroom procedure. The *Ss* were allowed to use only the materials taught for solving the problems. If other materials were used, the solutions were not counted.

MATERIALS AND TECHNIQUE

Material. The following are fair samples of the various kinds of materials used:

Geometry. If a line from the vertex of an isosceles triangle is perpendicular to the base, prove that it bisects the base and the vertex angle.

Prove that the bisector of the exterior angle at the vertex of an isosceles triangle is parallel to the base.

At noon, a flag pole casts a shadow 56 ft. along the ground. How tall is the pole? The only instrument that you are allowed to use is a yard stick. When the yard stick is stood up along side and parallel to the pole it casts a shadow 16 in. long. The ground is level and at right angle to the base of the pole.

Arithmetic. A steer dresses away 35%. If it weighs 877.5 lb. after it is dressed what did it weigh alive?

What should be the selling price per pound for a blended coffee blended in the following way: 20% of 50¢ coffee, 33⅓% of 60¢ coffee, and 46⅔% of 30¢ coffee?

The freezing and boiling points on a Centigrade thermometer are 0° and 100° respectively. The same points on a Fahrenheit thermometer are 32° and 212° respectively. What will be the reading on the Centigrade thermometer when the reading on the Fahrenheit thermometer is 72°?

Physics. What effect will a double concave lens have on the direction of propagation of two parallel rays of light? Why is this so?

A locomotive passes an observer. The pitch of the whistle changes as it goes away. Does the pitch rise or lower? Why?

A steamboat has a water displacement of 1000 cubic feet; the boat itself weighs 5 tons; how many tons of cargo can the boat support?

Mechanics. In the transmission of an automobile are three gears arranged on the drive shaft from the engine from front to back: an 8 in. gear, a 6 in., and a 4 in.; and on the drive shaft leading to the differential in the same manner: a 4 in. gear, a 6 in., and an 8 in. (In the original the mechanism of the transmission and the differential were drawn.)

(a) Which two gears would you mesh to get the greatest power at the wheel?
(b) Which two gears would you mesh to get the greatest speed of the wheel?
(c) With the same gears meshed, which would give the greatest speed along the road, a 26 in. wheel or a 32 in. wheel?

(d) With the same load, which size wheel will take the most power to drive, the 26 in. or the 32 in.?

(e) Suppose that your car is standing on an incline headed down hill. You are going to start your engine by allowing your car to coast down the incline. Which gears will you mesh in order to have your engine start the easiest?

Economics. "Internal commerce does not increase the wealth of a nation since it only transfers goods from one person to another." Is this sound? Why?

"If the wheat crop of the world should fall off one half next year, a rise in price would be of a great social advantage." Is this sound? Why?

Why is a monetary system based on a bi-metallic standard unsatisfactory?

Sociology. Why is the rule of the Clergy in Civil Society objectionable?

Why is organized and synchronized work in education and industry apt to cause many human failures?

Why is competition (struggle) in society, on the whole, a beneficial process?

Geography. Why is the earth cold at the poles and hot at the equator? (This is not due to the poles being farther from the sun).

What would be the most evident effect if the axis of the earth was not inclined to its plane of revolution?

What is happening in our meteorological conditions when the winds shift in the following way: N.E. to N. to N.W.?

History. Why did the New England states become manufacturing states and the Southern states agricultural states?

How could Whitney's invention of the cotton-gin in 1792 be given as one of the causes of the Civil War?

What effect did the Emancipation Proclamation have on England's behavior toward the South? Why?[4]

Procedure. (1) Instructions. Printed instructions were placed in the hands of the *S*s before every test. To insure uniformity of understanding, *E* read them aloud to the *S*s and explained such details as were necessary. The instructions were as follows:

> Please remember that your value as a subject in this experiment depends upon your faithfully carrying out the instructions given you. The purpose of this experiment is to obtain reliable data for whatever conclusions it may warrant. Your contribution to the results will depend upon your putting forth each time your best efforts. (1) You will be presented with some material which you will try your best to master. (2) You will be given some questions to answer to test how well you digested the material. (3) You will be given some problems to solve based upon this material. After the experiment has been finished, the aim and results will be given you.
>
> Avoid worrying while working on the material. You will not be expected to get all of the problems; that is, not all of you. What is expected of you is that you do your very best. Consistent effort throughout the experiment is what will give the best results rather than solving every problem.
>
> If any of the problems are so familiar to you that you do not have to solve them, that is, you do not have to think them through but you can put down the answer from memory, indicate this in writing in the margin beside the problems. It may be that you have had something similar before, but if enough time has elapsed so that you actually have to work the problems through again to solve them, then they are the same as new. Only in the cases that you know the answers without having to work the problems through will you indicate that you did not have to solve them.

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[4] An attempt was made to avoid as far as possible an overlapping of subject matter, such as arithmetical computations in geometry, physics, economics, etc.

You will accept the material taught as true for the solving of the problems. We wish to test your ability to use the material as given.

You will use *only* the materials taught for the solution of these problems. The reason for this is that we want all of you to work with the *same* materials and we wish to see how well you can do with materials as taught.

Special instructions for the problems in practical mechanics.

When working the problems in practical mechanics do not motion out the solutions with the hands and do not use pencil and paper excepting as indicated. Make a complete solution of a problem in your head before you make a mark; and then do just what is asked of you and no more. Avoid erasing if possible. Be sure that you have your solution and then put it down. Further instructions will be given later.

(2) Individual information. The *S*s were required to fill out blanks after every test period, giving the following information about themselves: name; sex; time consumed in solving the problem; year in college; previous preparation in the subject tested in semester hours in high school and in college.

(3) Routine of work. The material for each subject was thoroughly taught on Monday afternoon from 4–6 o'clock, or longer if needed. The *S*s were held until they thoroughly understood everything that had been presented. Much of the material was simple and only a review for most of the *S*s; but nothing was taken for granted. An outline of the material presented in each subject was given to each *S* so that the work might be reviewed before the test and vague points cleared up.

On Wednesday afternoon of the same week, from 4–6 o'clock, the tests were given. For the first 15 min., or more if needed, the *S*s answered a true-false test on the subject matter to determine whether or not they had mastered the information presented. Most of the *S*s were through with this in 5 min. If any points were unclear, they were explained and then the *S*s were given the problems with these final instructions.

You will have but one hour to work on the problems, but do not worry about the amount you are getting done, just work as rapidly as is consistent with accurate work; you may work them in any order that you please; and you are to use for your solutions nothing but the material taught on Monday. Any fact which you have forgotten or of which you are not sure that is essential in the solution of a problem will be supplied you. You are to put down on your test paper all of your work; just answers do not count. Put down each step in your solution, for it is the solution in which we are interested and not merely the answer. Do not quit until the hour is up unless the problems have all been solved. Keep on trying.

(4) Evaluating the solutions. The steps necessary in the solution of each problem were decided upon by *E* in collaboration with two professors in each of the subjects tested. Two methods of evaluating the solutions given by the *S*s were tried: an 'all-or-none' method (*i.e.* all right or all wrong), and a partial value method (*i.e.* giving part credit). Since it made little difference in the final outcome which method was used we evaluated the solutions by taking each step into account. If all the steps were correct, the solution was given full credit even though the final answer was wrong. Our interest was in the solution and not in the answer. Frequently answers would be far wrong because of errors in simple calculations while the principles of the solutions were entirely correct.

The relative difficulty of the problems and the values assigned them were determined as follows. If a problem was solved by all of the *S*s it was given the lowest rank, 0. If 90% of the *S*s solved it, it was ranked as 10; if 80% solved it, it was ranked as 20; if 50%, as 50, and so on throughout the entire list of problems. After the problems had been ranked, arbitrary values were assigned them so as to bring the total number of points to 120. This was done, in lieu of standardized tests, in order to make them as objective as possible.[6]

(5) Attitude of the Ss. The general attitude of the Ss was good. A few who did not take the work seriously were excused after the second meeting; the rest entered into the spirit of the experiment and worked hard.

(6) Subjects. Of those completing the tests, the first group was made up of 30 men and 20 women, the second group of 24 men and 22 women, and the last group of 18 men and 32 women—146 Ss in all. They were selected almost entirely from the classes in psychology in the University of Michigan and the Western Kentucky State Teachers College. There were 12 seniors, 34 juniors, 58 sophomores, and 42 freshmen.

RESULTS

(a) Subject matter. The subject matter or information taught was well mastered. From 23 to 36 'true-false' questions were used to test the S's knowledge of each of the subjects taught. An average of only 2.22 questions was missed on all of the tests.

(b) Problem-solving. The tables below give the correlations between every two of the eight academic subjects used in this study. It will be noted (1) that these correlations are moderately high and constant, and (2) that the inter-correlations between the science and the humanitarian groups are about as high and as constant as the intra-correlations within each of these groups.

Some of the correlations were lowered by the following factors over which we had no control.

(1) The correlations were most affected by the fact that many of the Ss were taking college courses in the subjects employed in this study; for example, of the first group of Ss, 13 were taking courses in economics, 10 in sociology, 10 in history, 4 in physics, 3 in geography, and none in geometry or mechanics. The other groups of Ss gave similar results. In order to demonstrate the effect of simultaneous study upon the correlations, the Ss taking economics and sociology were omitted, and a few of the correlations were recomputed. The correlation between geometry and economics rose from 0.55 to 0.69; between arithmetic and economics, from 0.60 to 0.67; between mechanics and economics, from 0.65 to 0.74; and between geometry and sociology, from 0.41 to 0.69. These values are given in parentheses in Table I.

(2) Another factor which affected geometry slightly was the inaccuracy in calculations; *e.g.* in some cases errors were made in calculating the degrees of the angles not given in the problems, which made the solutions of the problems impossible with

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[6] We had hoped to use standardized tests in these experiments, but we were unable to find any that were suitable. Few tests are concerned with problem-solving, most are based on memory—even in mechanics and physics, the task usually set is to identify or label parts or wholes.

the material given. Had these angles been correctly calculated, the solutions would have been comparatively easy.

TABLE I

CORRELATIONS FOR FIRST GROUP OF Ss

	Geom.	Arith.	Physics	Mech.	Econ.	Soci.	Geog.
Arithmetic	.69						
Physics	.66	.77					
Mechanics	.65	.67	.77				
Economics	.55 (.69)	.60 (.67)	.69	.65 (.74)			
Sociology	.41 (.69)	.59	.65	.65	.72		
Geography	.48	.58	.68	.60	.57	.62	
History	.58	.60	.63	.65	.69	.78	.56

TABLE II

CORRELATIONS FOR THE SECOND GROUP OF Ss

	Geom.	Arith.	Physics	Mech.	Econ.	Soci.	Geog.
Arithmetic	.78						
Physics	.73	.79					
Mechanics	.47	.57	.66				
Economics	.69	.74	.77	.54			
Sociology	.67	.74	.78	.57	.90		
Geography	.61	.72	.84	.68	.77	.81	
History	.60	.70	.81	.61	.80	.89	.86

TABLE III

CORRELATIONS OF VALUES FOR ALL THREE GROUPS TOGETHER
(Raw correlations without corrections)

	Geom.	Arith.	Physics	Mech.	Econ.	Soci.	Geog.
Arithmetic	.76						
Physics	.78	.75					
Mechanics	.61	.63	.77				
Economics	.70	.66	.78	.59			
Sociology	.64	.69	.78	.65	.86		
Geography	.59	.68	.77	.69	.71	.75	
History	.53	.65	.63	.56	.66	.75	.68

(3) A third factor which affected the results slightly was the fact that two of the *S*s did not follow the instructions. For example, one *S* who was antagonistic to the social principles taught spent the most of the hour attacking those principles rather than trying to solve the problems. Thus, this *S* ranked high in everything but sociology. In an interview later, he had no difficulty in solving the problems.

In the opinion of the writer these correlations are very significant considering all of the factors contributing to lower them. Besides those mentioned above, the following could not be entirely eliminated in a laboratory experiment: *(4)* earlier established attitudes; *(5)* short periods for instruction, and *(6)* the specializing of some of the *S*s in some of the academic subjects.

(c) Coefficient of reliability of the problem-solving tests. Spearman has recommended, when a test cannot be repeated that the odd problems be correlated against the evens and then applied to the formula: $r_x = 2rh/(1 + rh)$. This has been done, and the following table gives the raw correlations and the coefficients of reliability for each of the tests according to this formula.

(d) Correction of coefficients of correlations for attenuation. The coefficients of correlation for problem-solving given in Table III have been connected for attenuation according to Spearman's formula and are given in Table V.

TABLE IV

COEFFICIENTS OF RELIABILITY FOR THE PROBLEM-SOLVING TESTS

Subject	Raw correlation	Coefficient of reliability
Geometry	.85	.918
Arithmetic	.81	.895
Physics	.67	.802
Mechanics	.82	.901
Economics	.78	.876
Sociology	.74	.850
Geography	.75	.856
History	.52	.684

(e) Individual rankings. In order that the details of the experiment may be presented more clearly, the data have been analyzed further by giving the ranking of 10 *Ss*, selected haphazardly from the first group, in each of the eight academic subjects. Number 1 represents the highest rank and 50 the lowest.

TABLE V

CORRELATIONS OF TABLE III, CORRECTED FOR ATTENUATION

	Geom.	Arith.	Physics	Mech.	Econ.	Soci.	Geog.
Arithmetic	.83						
Physics	.90	.88					
Mechanics	.67	.70	.90				
Economics	.77	.74	.92	.66			
Sociology	.72	.79	.95	.74	1.00		
Geography	.65	.77	.92	.78	.82	.88	
History	.79	.83	.86	.72	.85	.98	.88

TABLE VI

RANKING OF 10 Ss OF THE FIRST GROUP

S	Geom.	Arith.	Physics	Mech.	Econ.	Soci.	Geog.	Hist.
2	49*	47*	40	44	40	44	36	35
4	43	48	47	48*	47	36†	41	42
6	46	50	50	49	40*	37	49	34
12	42	43*	49	43	46	42	29	46
21	50	43*	41	48*	44	40	44	50
31	49*	47*	46	50	50	50	50	49
49	1	1	4	6	3	31‡	3	2
25	7	2	3	2	25§	1	1	3
26	3	10	19	10	11	13	5	11
28	12	13	1	5	1	4	2	4

* The reason for 2 Ss getting the same rank is that they obtained the same score in these tests.

† This S was taking sociology at the time of this experiment.

‡ This *S* is the one who attacked the principles of sociology instead of solving the problems.

§ This S received a grade of 70 in economics, which was a fair score; but it gave him a low rank because of the large number that were taking economics at the time and were crowded toward the top.

In only a very few cases did an *S* who ranked in the first quartile of one subject get into the fourth quartile of another and vice versa.

(f) Information and problem-solving. The following correlations were obtained between rank in information and in problem-solving: in geometry, 0.37; arithmetic, 0.35; physics, 0.59; mechanics, 0.49; economics, 0.47; sociology, 0.45; geography, 0.51; and history, 0.38. These correlations are lower on the whole than those obtained between the various problems.

An analysis of the data shows that the *Ss* who were able to solve the problems had the required information, but not all the *Ss* who had the information were able to solve the problems. The ability to solve the problems does not vary directly with the amount of information. As one *S* (a woman) reported, "I can learn all that you teach in a few minutes but I do not seem to be able to apply it in solving the problems"; and her results bore out the truth of this statement. She ranked among the highest in information, but only among the average in problem-solving. Furthermore, some of the *Ss*, who made a poor showing in the information tests, did as well and in some cases better in problem-solving than other *Ss* who stood high in information. Some *Ss* cannot use what they know, while others use the knowledge they have to good advantage. One *S*, for example, of the first group was among the 6 lowest in information and the 12 highest in problem-solving.

(g) Effect of sex. Table VII shows that on the average the men ranked but a little higher in information and general intelligence, as measured by the Army Alpha Test, than the women but, in problem-solving, their average score was 48.7% above that of the women.

TABLE VII

A COMPARISON OF THE SCORES MADE BY THE MEN AND WOMEN

Sex	Av. score information	Av. score prob.-solving	Av. Army Alpha	Av. I.Q.
Women	84.9	39.6	127.68	108
Men	90.2	58.9	141.76	114
Diff.	5.3	19.3	14.8	6
% diff.	6.2	48.7	11.0	5.5

(h) Correlations between problem-solving and the Army Alpha Test. The following coefficients show that there is a higher correlation between the problem-solving in these eight fields and the Army Alpha test than between the information tests in the eight fields and the Army Alpha.

(a) Coefficients of correlation between problem-solving in the eight fields and the Army Alpha Test are: geometry, 0.51; arithmetic, 0.58; physics, 0.55; mechanics, 0.59; economics, 0.42; sociology, 0.53; geography, 0.50; and history, 0.54.

(b) Coefficients of correlation between the information tests in the eight fields and the Army Alpha Test are: geometry, 0.31; arithmetic, 0.34; physics, 0.40; mechanics, 0.53; economics, 0.41; sociology, 0.19; geography, 0.39; and history, 0.31. This is probably due to the fact that 120 of the 212 points of the Army Test are scores for reasoning or seeing relations. The rest of the points are for information.

We find higher correlations between these 120 points and problem-solving of the eight academic subjects than with the information tests. Also, we find them higher than with the Army Test as a whole. (1) Coefficients of correlation between the 120 points of the Army Test and the problem-solving of the eight academic subjects are: geometry, 0.61; arithmetic, 0.68; physics, 0.69; mechanics, 0.64; economics, 0.59; sociology, 0.58; geography, 0.50; and history, 0.57. (2) The coefficients of correlation between the 120 points of the Army Test and the information test of these eight subjects are: geometry, 0.43; arithmetic, 0.32; physics, 0.43; mechanics, 0.52; economics, 0.34; sociology, 0.10; geography, 0.32; and history, 0.33.

(i) Correlations with previous preparation. In order to meet the criticism that the solutions of these problems might have been unduly influenced by previous training, correlations have been worked out between the solving of the problems and the previous preparation of the *S*s in semester hours in each of the subjects, except mechanics. Mechanics was excepted because of shop work, factory work, and home work which could not be reduced to semester hours. The average coefficient of correlation for the remaining subjects is 0.26 ranging from —0.17 to 0.53 as follows: geometry, 0.43; arithmetic, 0.50; physics, 0.53; economics, 0.30; sociology, —0.17; geography, 0.17; and history, 0.09.

The preparation in mathematics of various individuals varied from 12 to 36 semester hours, in physics from 0 to 15, in economics from 0 to 9, in sociology from 0 to 18, in geography and related subjects such as geology and physiography from 0 to 12, and in history from 6 to 36.

To show how little previous training influenced the results of these experiments, under the conditions given, the following 5 cases are cited. One *S* who had had no previous preparation in sociology received the highest score, and two others who had had no sociology tied for second and third places in problem-solving. Also, 2 *S*s who had had no previous preparation in geography made the highest scores in this subject. An exception to this general rule must be made in the cases of mathematics and physics. There is a higher correlation between these subjects and previous training, which may be due to the fact that students who are good in problem-solving elect these courses, since they are usually taught by the problem method and hence would be attractive to them.

(j) Correlations with honor points. There is a relatively low correlation between the average number of honor points earned in the University of Michigan and the scores made in solving the problems in these eight fields. The following are the coefficients of correlation: geometry, 0.34; arithmetic, 0.38; physics, 0.42; mechanics, 0.32; economics, 0.20; sociology, 0.42; geography, 0.42; and history, 0.32.

These results also show that those who received high honor points generally solved the problems, but the contrary is not true, not all of those able to solve the problems received honor points. This may be due to a number of factors of which the following three are but suggestions. (1) It may be due to the fact that many of the undergraduate courses are fact gathering courses; (2) it may be due to personal prejudices; or (3) it may be due to the fact that some of these students were not applying themselves to their university work.

The coefficient of correlation between the average scores made on the information tests in these eight subjects and the average honor points earned in the University of Michigan is 0.60. The coefficient of correlation between the average number of honor points and the Army Alpha scores is 0.50.

(k) Criterion of the tetrad equation. The correlations between the marks of school subjects, etc., tended toward an arrangement, which can be expressed in Spearman's tetrad equation.[8] He says:

> Whenever the tetrad equation holds throughout any table of correlations, and only when it does so, then every individual measurement of every ability can be divided into two independent parts which possess the following momentous properties. The one has been called the "general factor" and denoted by the letter *g*; it is so named because, although varying freely from individual to individual, it remains the same for any one individual in respect to all the correlated abilities. The second part has been called the "specific factor" and denoted by the letter *s*. It not only varies from individual to individual, but even for any individual from each ability to another.

Since these correlations meet the criterion of Spearman's "tetrad equation" it may be that problem-solving represents *g*, which Spearman says may be eventually appropriately called "intelligence."

SUMMARY OF CONCLUSIONS

This experiment has been repeated three times with consistent results during the last three years with three different groups of *S*s. Though we have no wish to be dogmatic, the following conclusions and generalizations seem nevertheless to be warranted.

(1) Given a common background, there is a rather high correlation between the ability to solve problems in one subject and the ability to solve problems in another. The average of the 29 correlations is 0.67.

(2) The correlations between some of the problem-solving tests were lowered by various factors which could not be eliminated in these experiments; the main one of which was that many of the *S*s were taking courses in similar subjects at the time of the experiments, especially economics, sociology, and history.

(3) These experiments show a lower correlation between information and the ability to solve problems in the same subject than between the ability to solve problems in different subjects. The average of these cor-

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[8] C. E. Spearman, *The Abilities of Man,* 1927, 73 f.

relations is 0.45. However, those who solved the problems knew the material, but not all of those who knew the material could solve the problems.

(4) These correlations, as a whole, show very little relation between the ability to solve problems and previous preparation unless the material has been studied within a year. This is probably due not so much to extra information as to the similarity of the problems. When problems are similar and past solutions are fresh in mind, the right association or Gestalt is much more apt to come.

(5) The data herein given would indicate that the best fact gatherers receive the greatest number of honor points.

(6) Although the women ranked nearly on a par with the men in the Army Alpha and information tests, they were unable to maintain that equality in problem-solving. The men scored 48.7% higher than the women.

(7) These correlations meet the criterion of the tetrad equation.

The results of this study seem to substantiate the view that "problem-solving" is a general ability, "factor," or function. Thus, an individual, other things being equal, should function in this capacity relatively as efficiently in one field as another.

Although the functionalists and Gestaltists[7] differ in their general theories, these facts would accord with either of them if left to them to interpret. According to the first of these schools of psychology, problem-solving or the relating of past experiences to overcome a difficulty is a general function of the whole brain and depends upon the original mobility of the nervous system; while according to the second, especially to Köhler,[8] these inner workings are explained by internal dynamics of the nervous system, which depend upon the original dynamics for functional unity and organization. Both of these schools of psychology would explain problem-solving on the basis of some inherent disposition of the nervous system; one by trial and error or sudden unexplained association and the other by the physiology of the nervous system or inner dynamics. By either theory, however, the solution of a problem will be immediate and will depend upon past experiences either in the form of associations or functional units called patterns or configurations. As Wertheimer says,[9] problem-solving or creative thinking is the successful transposition of a member of one configuration to another. These different configurations are identified as belonging to a common configuration. Thus, these results and these theories would seem to make problem-solving a general function dependent upon the innate capacity or dynamics of the nervous system.

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[7] Gestalt appears to be only a new interpretation or revised theory of association. Association is explained in functional wholes, configurations, or patterns as opposed to the serial. a-b-c, type of association.

[8] W. Köhler, *Gestalt Psychology,* 1929, 103-149.

[9] M. Wertheimer, *Über Schlussprozesse im produktiven Denken,* 1920, 1-22; *Drei Abhandlungen zu Gestalttheorie,* 1925, 1-84.

It is possible to explain the other mental processes in a similar manner; that is, they are general functions or manifestations and depend upon the original disposition of the physical organism. This theory makes it easy to explain why the same individual may differ so widely in his relative ability in the different functions. A "low grade" may have an excellent rote memory without being able to relate his experiences into a construct or to transfer them from one Gestalt to another. Each is a different function or manifestation of the brain. A piece of lead may receive impressions and retain them, but it is incapable of relating those impressions into anything different. Likewise, with these different functions of the nervous system, impressions may be received and retained without being related or organized. Of course, relating cannot be carried on without something to relate. Although memory may be independent of reasoning or problem-solving, problem-solving is more or less dependent upon a good memory.

Since memory is the more general function and much more easily measured than reasoning, it is easy to understand how our schools might unconsciously drift into the habit of exercising and testing this function. This combined with the bad habits of study acquired by many of our bright students may explain why the students good in memory receive the most honor points.

Finally, let us inquire as to what we mean by intelligence. What is it that differentiates the lower forms from the higher, the more intelligent humans from the less intelligent, the genius from the average, if it is not the ability to meet "unprecedented situations"? Such men as Socrates and Edison had fewer opportunities to acquire information than we; yet they rose head and shoulders above us. Why? Not because of their phenomenal memories but because of their ability to solve problems with what they knew. Is it not, therefore, this ability to solve problems which the makers of intelligence tests are trying to measure? If it is, then they shall have to revise materially their tests.

It is evident that these makers of tests are not agreed; for example, one group—Otis, Pressey, and those who helped to make the Army Alpha Test—excluded memory tests; another group—Binet, Terman, and Thorndike—include them. Yerkes, in one of his tests, has 6 components out of 20 given to testing the imagination; and the Army Alpha Test allows

THE EFFECT OF A CONSTANT VISUAL ANGLE UPON THE BINOCULAR-DISCRIMINATION OF DEPTH DIFFERENCES

By LLOYD S. WOODBURNE, University of Michigan

If we set up two pencils a little distance away and close first one eye and then the other, the perceived distance between the pencils will not be the same. This illustrates *parallactic difference* or *binocular parallax*. Other terms which have about the same meaning are *binocular disparity* and *stereoscopic acuity*. In the following pages the term *visual angle* has been reserved for the size of the retinal image.

History of the Problem. Although the physiology of binocular vision has had a long history, and the acuity of binocular depth-discrimination has been carefully studied, little attention has been paid to the effect of the changes in size of the visual angle of the objects.

Pulfrich[1] (1899) was the first to take this factor into account. He repeated Helmholtz's pin-experiment, using, instead of pins, stereoscopic pictures. In this way he kept the size of the retinal image of the objects constant. He determined that normal eyes could discriminate differences in depth when the parallactic difference between two objects was as low as 10″. Since Pulfrich gives little detail, one cannot be sure just what conditions did exist, and, therefore no definite conclusion can be drawn from his investigation.

Heine[2] (1900) used three rods as test-objects, their ends covered by a diaphragm. The middle rod alone was displaced, the outside rods remaining 5 meters from *S*. *Ss* with normal visual acuity were able to obtain a limen as low as 12″ in parallactic difference between the two objects; while those having sharp vision could still discriminate at 6″. Heine seems unaware of Pulfrich's problem.

Bourdon[3] (1900) made a careful study of stereoscopic acuity. He constructed a much more delicate apparatus which allowed the image of three pins to fall on the foveal region. The head was held firmly in position. When the stationary rods were two meters distant, he was able to appreciate a displacement of 1.5 mm. This is equal to a parallactic difference of 5″, which Bourdon found was the smallest value that he could obtain by the vernier method in monocular acuity. Bourdon does not mention the possibility of changes such as those in which we are interested.

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[1] C. Pulfrich, Über den von der Firma Carl Zeiss in Jena hergestellten stereosco-
[2] L. Heine, Sehschärfe und Tiefenwahrnehmung, *Arch. f. Ophthal.*, 51, 1900, 146.
pischen Entfernungsmesser, *Phys. Zsch.*, 1, 1899, 98.
51, 1900, 146.
[3] B. Bourdon, *La perception visuelle de l'espace*, 1902, 1-442.

The next important work was done (1919) by Howard.[4] He used only two rods, the ends covered and both movable, so that there was no constant point of orientation. The rods were placed at 6 meters and separated 6 cm. from each other. The images of both fell on the fovea. Using a questionable method, he found a 75% limen as low as 1.8″ for a few of his best *Ss*. These results were obtained only with *Ss* of high visual acuity and perfect balance of eye-muscles. He failed to take into account our factor of size of angle.

Andersen and Weymouth[5] (1923) redetermined the threshold of binocular depth-discrimination with normal vision. Threads were used as test-objects, the middle one movable. The threads were 2 meters from *S*, and were exposed for 1.5 sec. The 2-meter distance was alleged to reduce accommodation to a minimum. Since the movement of the middle thread is limited to 6 mm., the change in its apparent diameter was said to be too slight to be used as a cue. This latter claim is not substantiated. The limina lie at the points where the 50%-line crosses the curves of the distribution of the near- and the far-judgments; *i.e.* the 50%-line indicates the position where judgments of near and far equal half the total number of judgments at any position. This range from the point of 50% far- to 50% near-judgments was then divided by two in order to get the average single displacement. The authors who averaged their interpupillary distance before they derived any individual values. While there is no objection to deriving average values, this should be done after the final individual values have been calculated. They have assumed in their calculations, by dividing the above range by two, that the curves of the far- and near-judgments are identical in character and slope. As will be pointed out later in this article, this is not necessarily true; nor is there any constant relationship between the subjective and the instrumental zero. The curves are drawn from the summed results of all thread-lengths in each series. Their results on a thread-length of 2 cm. in series B have been used for comparison with our own. They were plotted and the values calculated in a manner identical with ours. The value obtained in this way is a j. n.d. of 2.08″ or a distance of 0.152 μ on the retina.

Langlands'[6] (1929) test-objects were a maltese cross, a square, and a circle. These objects were placed before and behind the plane of a ship's mast and funnels which served as the plane of reference. *L*'s method of deriving his 50%-threshold was the same as Howard's and is subject to the same criticism, for the correctness of the reports has definite reference to the instrumental zero. By the use of lighted circular tubes and an exposure of 4 sec. *L* obtained a liminal value of 2.7″ parallactic difference. He finds that binocular resolution increases with the intensity of the illumination and with interpupillary displacement. From this fact he infers that the size of the retinal image, the effect of diffusion-circles, and the differences of perspective in direct and indirect vision are all negligible. He also attempted to control the factor of size of retinal image, but was unsuccessful in finding a mechanism which would keep the visual angle constant.

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[4] H. J. Howard, A test for judgment of distance, *Amer. J. Ophthal.*, 11, 1919, 656.

[5] E. E. Andersen and F. W. Weymouth, Visual perception and retinal mosaic, *Amer. J. Physiol.*, 64, 1923, 561.

[6] N. M. S. Langlands, Experiments on binocular vision, *Med. Res. Council, Privy Council*, Spec. Rep. Ser. No. 133, 1929.

The Problem

Thus far we have considered only those changes in the visual angle of an object which take place when the object recedes from or approaches the eye. A second change lies in the perspective effect. Thus parallel lines or objects seem, as they recede, to come closer together and this change also gives a cue to the distance of the object. The effect of these two sources of change is the chief problem of this investigation.

An accessory problem concerns the ability to discriminate depth-differences when the eyes are not fixated upon the median plane in the primary position. The question is simply one of relative torsion, assuming that torsion of the eyeball does really occur with change of position. The answer to this question should be instructive, since, if comparable values are found, they would indicate that this type of discrimination is not dependent upon any fixed alignment or arrangement of the retinal elements.

Apparatus

Andersen and Weymouth used a thread-length of 2 cm. They found empirically that this length gave the best results, but they did not vary the diameter of the thread. In a preliminary experiment I determined that slightly better results could be obtained with a thread-diameter of 0.3 mm. instead of the 0.5 mm. which they used. I then designed slits, to be illuminated from behind, that would have minimal dimensions of 2 cm. by 0.3 mm. and whose proportions would remain constant whatever the actual size. The slits were designed to eliminate the factor of visual angle in all the respects mentioned above. As the distance from *S* increased, the actual size of the slit also increased, so that the visual angle remained the same at all positions. In Fig. 1, which illustrates the exposure-slits in detail, the mechanism is shown for the automatic adjustment of the size of the slits. The heavy line A is the length of the slit that was illuminated. Its extent is from the overlapping flange above A to the cross-piece just below. The outside edges of each slit and the inside edges of each frame were milled to an angle of 26′ to allow for proportional changes of the slit-width as the height increased. Small springs extending between the two halves of each slit at the top and bottom held them firmly against the sides of the frame and insured the full width that corresponded with a particular height.

A wedge B is the means of pushing the slit A upward and so increasing the actual size. The movement of this wedge is controlled by that of the attached wheel C on the track D. This track was an inverted brass T-beam, whose distance from the tracks (G) on which the slits moved decreased the greater the distance from *S*. The rear end of this brass T-track was, then, 3 cm. higher than the front position, and consequently the slits were 3 cm. longer at that farthest position. At this rear position the brass T-track was hinged to allow adjustment of the slant when desired. Consequently the farther back the slits were pushed, the greater would be the size of the slit in both directions, because of the upward push of the wheel-wedge mechanism C-B rolling on the sloping track D. The sides of the slits were held firmly down on the wedge by the set of springs indicated by F. The position of the vernier attachment is at E. The distance between the slits was 38 mm. when the absolute distance from *S* was 207 cm.

The effect of perspective was eliminated by having the tracks on which the slits moved diverge sufficiently, as they receded, to make the slits appear equidistant from each other whatever the position. *S*'s head was held in position by a mouthpiece calibrated in degrees to give accurate and quick adjustment of angular changes in the horizontal direction. The end of the mouthpiece was coated with sealing wax,

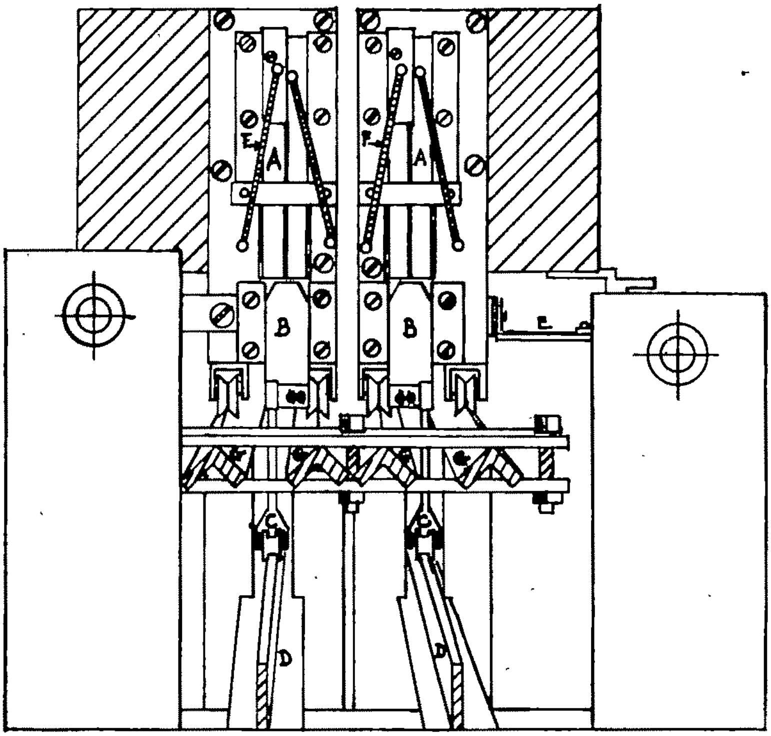

FIG. 1. DETAIL OF EXPOSURE SLITS

and *S* was told to bite into it while it was soft, making an impression of his teeth. This impression when hard held the head immovably. The mouthpiece was attached to a universal joint to allow adjustment in all directions and was clamped to an arm attached to the front support of the apparatus.

The light source was a neon tube behind each slit, the intensity of which was reduced and better diffusion obtained by gluing a piece of ground glass and two thicknesses of white paper just behind each slit. These neon tubes were coiled into a square and attached to a frame fitting into the tin boxes, to which the slits were attached. The electrical connections from the electrodes led out of the back of these tin boxes and thence either to the other box or to the transformer. The exposure apparatus was an adaptation of a Meumann time-sense apparatus to give contacts for illuminating the slits and warning signals at known intervals. The warning light allowed *S* to relax in the chair between exposures.

Although the experiment took place in a dark room *S* was kept light-adapted by a screen illuminated and tinted as nearly as possible the color and intensity of the slits. This was to avoid any possibility of a different light-adaptation for white and for colored lights. One *S*, who was practicing before the screen was tinted, reported that it was much easier to judge the depth-difference after the color was added. The intensity on this screen was controlled by means of a rheostat in series with the screen lights. A second screen of dull black finish was placed just in front of the slits to cut out any reflection from the tin boxes. Both screens had holes in them through which *S* examined the slits.

The readings were taken from a 3-meter steel tape calibrated in mm. and stretched from one end of the track to the other. The left slit was the one varied in position. It had a vernier attached which slid over the tape allowing for readings in either direction as fine as 0.1 mm.

Every effort was made to have everything except the apparatus comparable with the work of Andersen and Weymouth. The exposure-time was the same (1.5 sec.), as was also the statistical method. The factor of irradiation was virtually eliminated by altering the intensity of the light on the adaptation-screen. The interpupillary distance was measured by a mirror type of interpupillary distance-gauge, and each *S* was measured twice at different times by this method.

METHOD

A practice-period of 5–6 hrs. was given each *S*, in order to acquaint him with the process of judging depth-difference, and also to bring the practice-effect to a minimum. During this practice-period trials were made by the method of minimal changes away from and toward zero in either direction. Thus the general position of subjective zero was found and used as a point of reference for the placing of positive and negative intervals. After the practice-period the method used was that of constant stimuli, the first interval being some distance on either side of the subjective zero. Twenty-five readings were taken at each position, with the magnitude of the interval decreasing to zero. When *S* gave a majority of equal judgments, the interval was increased and 25 readings were taken at the same positions as before, but with an increasing size-interval. In this way the errors of habituation were counterbalanced. The order of presentation, arranged in advance, permitted 200 judgments without repetition.

The *Ss* were instructed to record a + for a judgment of farther, = for indecision, and — for nearer. A maximum of 25 judgments was made at a sitting. (With 40 readings between rest-periods, the number of errors in the second 20 increased by as much as 50% over those of the first half.)

The exposure-timing apparatus was going continuously; and all changes of position were made in the dark with the aid of a flashlight. While these changes were being made a small hinged screen was flapped up in front of the second (black) screen to prevent the *S* from seeing the nature of the change.

The formula used for the derivation of the parallactic difference is that found in Helmholtz,[7] Hofmann,[8] and Howard.[9] It is: $p = 2a\delta/E^2$, where p = parallactic value in radians, $2a$ = interpupillary distance, δ = depth difference, and E = the mean distance from the subject to the exposure slits. The derivation of this formula can be found in any of the references mentioned. It is understood, of course, that this formula holds only under certain conditions and is not always applicable. The conditions under which it can be used are: (1) the objects lie near the median plane of *S*, (2) the difference in distance between the objects is small, and (3) the distance from *S* to the objects is relatively great.

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[7] H. Helmholtz, *Physiological Optics*, (J. P. C. Southall, trans.), 3, 1924, Note on Sec. 30 by Von Kries.

[8] F. B. Hofmann, *Physiologische Optik*, 1925, 418.

[9] H. J. Howard, *loc. cit.*

In order to change from a parallactic value in radians to one in angular seconds, the value in radians is multiplied by $180/\pi$, and that product in turn by 3600. Conversion from angular measure to linear distance on the retina is through multiplication of the tangent of the angle by the distance from the nodal point to the retina (15.5 mm.).

TABLE I

FREQUENCY OF THE THREE TYPES OF JUDGMENT AT THE VARIOUS INTERVALS

E.G.-VIII: SERIES A

Age: 38
V.A.: R 20/15
L 20/15
I.P.D.: 64 mm.
M.B. Exo-1½

Interval (mm.)	Judgments		
	near	equal	far
12		3	47
10	5	5	40
9	6	6	38
8	9	10	31
6	26	17	7
5	38	6	6
4	41	4	5
2	49	1	
Frequency Curve	Far limen:		.80″
	Near limen:		.67″
Parallactic Values			
Angular	Far limen:		2.23″
	Near limen:		2.03″
Linear	Far limen:		.167 μ
	Near limen:		.154 μ

It was decided to use the graphic statistical method of drawing a smooth curve through the points representing the frequency of a certain type of judgment. The point of crossing of the near- and far-curves was taken as the null position or the subjective zero. The limen was defined as the 50% point on the near- and far-curves. This point was derived by averaging the intervals corresponding to the 50%-point, the 75%-point, and the 25%-point. The figure (interval) represented by this average value was then computed from the null position to get the liminal value. In some cases the value of the mean of the judgments, as represented by $\bar{T}$ in the formula for the normal curve, was found. It showed, however, considerable variation and was not calculated for all cases. Several reasons for the use of the frequency-curve instead of the calculated mean are evident. In the first place, the central tendency of the judgments was what was desired, since the absolute numerical values were of no significance. The values derived were to be used only in comparison with the results under similar conditions when the size of the retinal image was not controlled. We have taken a table on a thread-length of 2 cm. from the results of Andersen and Weymouth, and placed it with series A, as the most comparable with our own results. A more cogent reason for not using the calculated mean value is the fact that the individual curves show no regular progression, which is required for the use of such methods. In the sample curve which follows it is noteworthy that the tails of the curves, which express a low frequency of that type of judgment, do not come down immediately to the base line after passing the null position. This is indicative of a certain amount of oscillation of judgment, and is partly due, I am sure, to the attitude of *S* just prior to the exposure. If *S* is anticipating a far-judgment, the object will look far, and the converse likewise, no matter what the relative position of the slits. More than this, there is a certain amount of natural fluctuation which has been found to occur when small differences were under consideration.

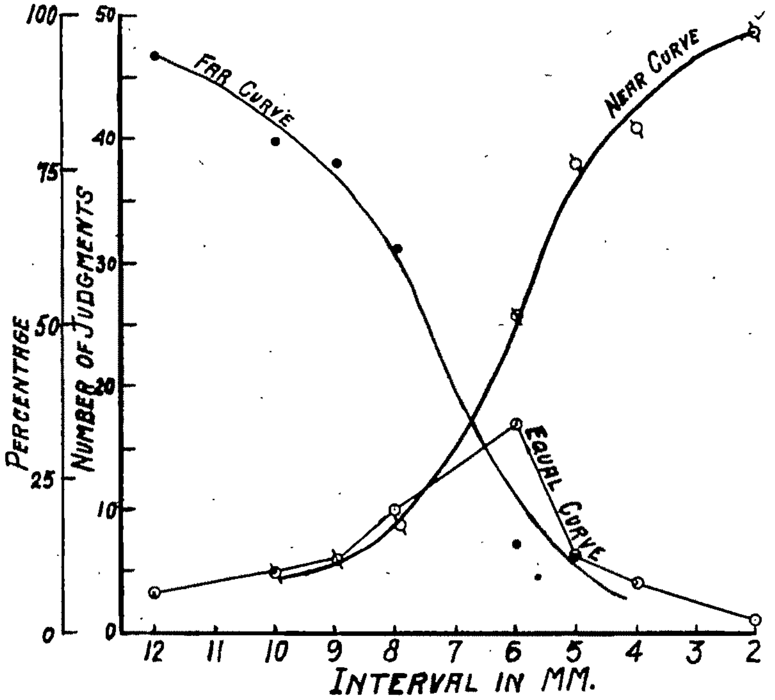

FIG. 2. CURVES OF E.G.-VIII, SERIES A

Far Limen = 0.8 mm.; Near limen = 0.67 mm.; Null position = 6.8 mm.

A sample table and the curve which was derived from it are reproduced. Table I gives the frequency of the three types of judgments at the various intervals. The curve (Fig. 2) is merely a graphic representation of this same frequency, with the interval plotted against the frequency of a certain kind of judgment. Individual tables and curves were made out for each *S* and for each series. Then these individual values were averaged in the summary-tables which are given here.

In the tables, I.P.D. = interpupillary distance, V.A. = visual acuity, F = far-limen, and N = near-limen, M.B. = muscle balance.

RESULTS

Series A. The slits were placed 207 cm. from *S.* Fifty readings were taken at each of 8 positions with the eyes in the primary position. The results of each *S* have been plotted in a curve for each series, and thereafter and only thereafter have summary tables been made out. It is not necessary to give all the individual tables and curves, since the sample given above illustrates very well all the factors which we shall discuss.

Below is given the summary of this series, with a summary of the results of Andersen and Weymouth on a thread-length of 2 cm. (Series B of their investigation).

TABLE II

SUMMARY OF SERIES A

Subject	No.	Age	I.P.D. (mm.)	V.A.	Limen (mm.)	Parallactic value	
						angular	linear
K.D.	I	20	64.5	20/15	F: .59	1.82″	.136μ
				20/15	N: .47	1.45″	.110μ
S.E.	III	19	58	20/15	F: .64	1.81″	.135μ
				20/15	N: .66	1.83″	.139μ
F.R.	V	20	64	20/15	F: .56	1.72″	.129μ
				20/15	N: .40	1.23″	.092μ
V.G.	VI	18	61	20/15	F: .50	1.46″	.110μ
				20/15	N: .60	1.75″	.131μ
J.M.	VII	20	70	20/30	F: .73	2.42″	.183μ
				20/30	N: .57	1.91″	.144μ
E.G.	VIII	38	64	20/15	F: .80	2.23″	.167μ
				20/15	N: .67	2.05″	.154μ
H.W.	X	43	64	20/15	F: 1.03	3.21″	.242μ
				20/15	N: 1.50	4.10″	.340μ
					Av.:	2.12″	.159μ
Andersen and Weymouth: Series B (Length of thread: 2 cm. Separation of threads: 4 cm.)			64 mm.	20/15	F: .67	2.16″	.162μ
				20/15	N: .617	2.00″	.150μ
					Av.:	2.08″	.156μ

From Table II it can be seen that the average results of the group of seven *S*s was 2.12″ or 0.159 μ. There is a certain amount of variability in the individual results which does not show in the average. The averaging of the individual results is possible when these values are given in terms of angular seconds, since, at that stage, all individual differences have been already taken into account.

By reference to Table II it can be seen that the average of the results of Andersen and Weymouth give a value of 2.08″ or 0.156 μ distance on the retina. The difference in magnitude between our results and theirs is sufficiently small to be ignored when the exactness of the two methods is taken into account. One would expect to find some difference between the two sets of results on the basis of the difference in apparatus and technique. We have, however, very definitely found values of the same general magnitude, and can draw comparisons quite adequately and justifiably. We may say, then, that, from the above comparison of results found when the visual angle was and was not under control, such changes are not a necessary cue to fine depth-discrimination; but when present they are likely used as an accessory means.

Series B-1. This is the control-group for the observations with the eyes in secondary positions. There were eight *S*s, each of whom gave 50 judgments at each of the 8 positions. Since this was a new group of *S*s, a five-hour practice-period preceded the recording of the judgments. The distance from the *S* to the slits was, in this case, 414 cm.

TABLE III

Summary of Series B-1

Subject	No.	Age	I.P.D. (mm.)	V.A.	Limen (mm.)	Parallactic values	
						angular	linear
M.K.	XI	22	60	20/20	F: 2.8	2.1″	.158μ
				20/20	N: 2.8	2.1″	.158μ
J.M.	XII	20	60	20/20	F: 2.5	1.81″	.136μ
				20/20	N: 2.9	2.13″	.160μ
M.E.	XIII	20	62	20/30	F: 3.7	2.78″	.208μ
				20/30	N: 2.9	2.13″	.160μ
H.H.	XIV	21	63	20/15	F: 3.6	2.72″	.204μ
				20/15	N: 4.3	3.23″	.242μ
R.P.	XV	24	66	20/15	F: 1.1	0.87″	.065μ
				20/15	N: 1.0	0.80″	.060μ
E.H.	XVI	19	60	20/15	F: 1.6	1.16″	.087μ
				20/15	N: 2.4	1.73″	.130μ
E.K.	XVII	20	58	20/15	F: 4.5	3.12″	.234μ
				20/15	N: 3.1	2.16″	.162μ
					Av.:	2.11″	.158μ

Table III shows an average value of 2.11″, which corresponds to a retinal distance of 0.158 μ. These results are derived from the first seven *S*s. The eighth *S* was, on her own admission, discriminating on the basis of "real near" and "not so near." This indicates clearly that her range had not as yet been found at all. In Series B-2 and B-3 her range was discovered and small consistent results were obtained.

Series B-2. The same *S*s were used as in B-1; the slits were in the same position as before, and the number of judgments continued at 50 per station.

There was, however, one important change which must be kept in mind. The mouthpiece, which held *S*'s head, was turned 15 degrees to one side or the other. Approximately half of the *S*s had their heads turned to the right and the rest to the left. This meant, of course, that the head turned about a point which was forward of the plane through the two nodal points. The axis of this rotation was 41 mm. When the head was in this position the eyes were turned back to look directly at the slits as before. If the head had been turned about a point directly under the center of the plane through the two nodal points, the eyes would have been turned exactly 15 degrees from the 'primary' position. Since this turning is done with the head at the end of a radius of 41 mm. we must calculate what is the actual turn of the eyes in order to face the slits as before. The radius of this distance moves the head to the side 1 cm., which causes a reduction in the angle of the eyes from the primary position of 8′. Thus the eyes now turn 14° 52′ instead of the full 15°. To find the decrease of the interpupillary distance which corresponds to such turning of the eyes we must take the cosine of 14° 52′ times the original interpupillary distance. The cosine of this angle is 0.9665. I have, however, used only the first two places of this figure in the following computations.

Since the time-allotment was short, it was impossible to give each *S* the length of practice-period that was desirable in this new situation. Consequently the number of mistakes in the early series of readings was probably larger than it would otherwise have been. The parallactic values derived show this when compared with Series B-3.

TABLE IV

SUMMARY OF SERIES B-2

Subject	No.	Age	I.P.D. (mm.)	V.A.	Limen (mm.)	Parallactic values	
						angular	linear
M.E.	XIII	20	62	20/30	F: 4.6	3.28″	.246μ
				20/30	N: 3.8	2.70″	.208μ
H.H.	XIV	21	63	20/15	F: 4.7	3.39″	.254μ
				20/15	N: 4.4	3.14″	.234μ
R.P.	XV	24	66	20/15	F: 2.9	2.30″	.173μ
				20/15	N: 2.1	1.60″	.130μ
E.H.	XVI	19	60	20/15	F: 2.6	1.80″	.134μ
				20/15	N: 4.2	2.98″	.223μ
E.K.	XVII	20	58	20/15	F: 4.5	3.00″	.224μ
				20/15	N: 4.2	2.80″	.210μ
F.L.	XVIII	17	61	20/20	F: 2.3	1.69″	.122μ
				20/20	N: 2.5	1.86″	.132μ
					Av.:	2.54″	.190μ

The figures in Table IV give the average result for 6 *S*s. This average was found better for purposes of comparison than the individual readings since there is a certain amount of variability which cannot be eradicated in the number of judgments used. In the computation of the average values given for this series the results of M.K.-XI and J.M.-XII were not used. The reason in the case of M.K.-XI was that he consistently refused to give any equal judgments no matter how small the interval. It was learned in the case of J.M.-XII that after the first series a guess would be made one way or the other when it was impossible to tell the relative position.

TABLE V

SUMMARY OF SERIES B-3

Subject	No.	Age	I.P.D. (mm.)	V.A.	Limen (mm.)	Parallactic values angular	Parallactic values linear
M.E.	XIII	20	62	20/30	F: 3.3	2.32″	.175 μ
				20/30	N: 4.1	2.90″	.217 μ
H.H.	XIV	21	63	20/15	F: 2.7	1.96″	.147 μ
				20/15	N: 2.3	1.67″	.124 μ
R.P.	XV	24	66	20/15	F: 1.7	1.30″	.097 μ
				20/15	N: 2.2	1.67″	.125 μ
E.H.	XVI	19	60	20/15	F: 1.8	1.25″	.095 μ
				20/15	N: 3.2	2.22″	.166 μ
F.L.	XVIII	17	61	20/20	F: 3.1	2.17″	.163 μ
				20/20	N: 3.0	2.11″	.158 μ
					Av.:	1.96″	.147 μ

We find that the average value for this series in angular seconds is 2.54″, which corresponds to the 0.190 μ linear distance to the retina. These values are slightly larger than those found for Series B-1. Some increase would be expected because of the lack of the full training period. Otherwise the figures should coincide, since the shortened interpupillary distance has already been taken into account in calculating the size of the parallactic angle.

The question which instantly arises is: Can the increase in the parallactic values be explained on the basis of lack of practice or is there some other factor to consider? A second possibility would be the factor of torsion, which is supposed to be present when the eyes are turned to the side. It is very clear that we have here a confusion due to the presence of two factors. Let us look for the answer to this question in the results of Series B-3, where we find merely the addition of a certain amount of torsion.

Series B-3. In this final series of our experiment we have the same situation found in B-2 with a slight addition.

The addition is the elevation by 15 degrees of the rear end of the apparatus. This, it is clear, increases the torsion considerably when the head is kept turned

15° to the side. The approximate amount of torsion of the eyes in this final position is equal to 1° 59′. An additional factor in this series is the fact that when the rear of the apparatus is elevated the absolute distance to the slits decreases slightly. The reason for this is that the mouthpiece is so situated that the eyes are 17 cm. above the front support of the apparatus and remain stationary in position. Thus the mouthpiece does not move when the apparatus is elevated, resulting in a slight decrease in distance from *S* to the slits. This decrease amounted to 4 cm., making the distance 410 cm. The difference in the parallactic angles due to this difference is almost negligible, amounting to 0.02″ or 0.00155 µ.

Here also the results of M.K.-XI and J.M.-XII were eliminated before the averages were calculated for the same reasons as stated in Series B-2. The subject E.K.-XVII did not finish this series and no results are included for him.

In this summary of this final series we have results which are slightly lower than those found in Series B-2. The average for B-2 was 2.41″, corresponding to 0.180 µ, while in B-3 we have obtained an average value of 2.30″ which corresponds to a linear distance on the retina of 0.173 µ. The decrease in the values for this last series is due, as we assume, to the loss from the lack of practice already mentioned in B-2. Since there is no increase in B-3 over B-2, we may safely conclude that in human adults any torsion that may be present does not reduce the fineness of binocular perception of depth differences.

CONCLUSIONS

As we have already suggested in the discussion of Series A, the values derived when the factor of the visual angle was kept constant are very little different from those calculated from the results of Andersen and Weymouth. These results are given at the end of the first series. The difference between our average value and theirs is 0.04″ or 0.003 µ. This is too small a value to be significant in a study of this sort. We may safely conclude, then, that normal changes in the visual angle of an object are not necessary cues in the binocular discrimination of depth-differences, although they are probably used as accessory cues when present. It would be expected that there would be some difference between the two sets of results due to the fact that in our investigation a different light-source and slightly different test-objects were used.

The comparison of the values derived from the three Series, B-1, B-2, and B-3, shows the relationship between the results with the eyes in the 'primary' position and those found when they were in 'secondary' positions. By reference to the summaries of these three series we can compare directly the results found under the various conditions. By means of such a comparison we find that in B-1, when the eyes were directed straight ahead, the average value of all subjects was 2.11″ which corresponds to a linear distance on the retina of 0.158 μ. As has been already stated there was an insufficient practice-period before the readings were taken on B-2 with the eyes 15° to the side. The average values found for this series were 2.54″ or a retinal distance of 0.190 μ. This is an increase of 0.43″ and (0.032 μ) over B-1. As we have noted above, there is a confusion in the results of this series on account of the effect of the lack of practice and the possible effect of torsion in the eye-movement. In Series B-3 we have only an increase of this possible factor of torsion. The values for this last series, when the apparatus was elevated, are 1.96″ and 0.147 μ. If the results of the second series are compared with the last, it will be evident that there is a slight decrease in the values obtained. This decrease, which was found with a considerable increase in torsion (if real torsion does occur) would seem to show that the changing of the position of the eyes to this extent does not increase the limen more than would be expected on account of the decrease of the interpupillary distance. If there is real torsion of the eyeball in moving from one position to another, it does not affect the fineness of binocular depth-discrimination. It is evident, moreover, that there is no necessary relationship between this type of discrimination and any fixed alignment or arrangement of the retinal elements.

Supplementary Note. It will be noticed, if the reader turns to Table I and curve appended (Fig. 2), that there is a shift in the position of the equal judgments away from the instrumental zero. In 5 of the *Ss* this shift was discovered, and in some cases to a considerable extent. When such a shift does occur it is impossible to use the correctness of the judgment as the basis for deriving the limen. Unfortunately this principle of correctness has been used by some of the experimental workers, notably Howard and Langlands, as mentioned above. It is, however, quite conceivable that the poor results recorded for Howard's *Ss*, who had somewhat anomalous vision, might have been due to this reference to the instrumental zero. Such a method as he uses starts with an unproved assumption, instead of basing the derivation entirely upon *Ss*' reports. A reasonable explana-

Subject	Shift	No.	V.A.	Muscle-balance
E.G.	backwards	VIII	20/15 20/15	Exo—1½, Hyperphoria—½ Abduction—6, Adduction—8
M.E.	forwards	XIII	20/30 20/30	Abduction—6 Adduction—0
E.H.	backwards	XVI	20/15 20/15	Eso—2, Abduction—6 Adduction—12.
H.H.	forwards	XIV	20/15 20/15	Accommod. Exo—2, Abduction—6 Adduction—10
F.L.	backwards	XVIII	20/20 20/20	O.K. O.K.

tion of such cases may be found in an examination of the refraction-records of those *Ss* whose results show such a shift. The tabulation above gives in detail the visual acuities and the muscle-balance of these *Ss*.

It will be promptly noted that there is very little asymmetry in the acuity of the two eyes. When we look at the muscle-balance of each subject we do find some discrepancy. It seems that in all but one case (F.L.) there is imbalance of the eye-muscles with a shift in either direction. We may suggest, then, that a lack of balance of the eye-muscles is often accompanied by such a shift in the position of equal judgments in front of or behind the instrumental zero.[10]

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[10] It may be questioned how well the apparatus described above controlled the size of the visual angle. One *S*, whose results are not included in Series A, worked for 30 hours without being able to decrease the number of errors below 8 or 10 out of 20 readings, with a depth interval of 3 mm. When the work was practically done and he was asked what he had used as his basis of discrimination, he reported that he had tried to judge on the basis of the size of the slit. The nearer slit should be larger, he thought. It seems to the writer that this is a fair proof that *accurate* discrimination on the basis of the apparent size of the slit was impossible.

THE STATISTICS OF THE METHOD OF CORRECT MATCHINGS

By DWIGHT W. CHAPMAN, Harvard University

Several recent studies[1] have employed what may be termed the *method of correct matchings.* Allport and Vernon, for example, report experiments whose purpose was to determine whether certain aspects of personality are conveyed in handwriting. The method is to require Ss to attempt to match character-sketches of a group of persons with specimens of the handwriting of those same persons. The number of correct matchings thus achieved is then compared with the number to be expected from chance alone, and any significant excess of the former over the latter constitutes evidence that real similarities between sketches and handwriting must govern the Ss' performance.

This experimental method is at once simple and direct, and capable of extension to the solution of many analogous problems. However, its whole effectiveness rests obviously upon a correct understanding of what is to be expected from chance matchings; and certain phases of this statistical problem have, so far as I know, not yet been discussed for the psychologist. Mathematical mistakes in the use of the method have already occurred, as we shall see; and in critical cases they may easily lead to a quite false assessment of the data. This paper will therefore treat the following questions in order:

(1) Given two series of t elements each, and defining a correct matching as the correct pairing of one element in the first series with its companion in the second series, what is the probability that a single chance arrangement of the two series will result in *exactly* s correct matchings?

(2) What is the probability that a single chance arrangement of the two series will result in *at least* s correct matchings?

(3) If n independent chance arrangements of the two series are made, what is the probability that the mean number of correct matchings for the n

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[1] G. W. Allport and P. E. Vernon, *Studies in Expressive Movement,* 1933, 212–242; F. H. Allport, Written composition and characteristics of personality, in *Outline of an Inquiry Now Being Made at Syracuse University, etc.,* 1933, 33–42; H. Cantril and H. A. Rand, Additional study of the determination of personal interests by psychological and graphological methods, to appear in *Character and Personality,* 2, 1934.

arrangements will be at least $\bar{s}$? (That is, how good may we expect the average of the chance performances of n subjects to be?)

(1) *The probability that a single arrangement of two series of t elements will give exactly s correct matchings.* Let a series of t x-elements, $x_1, x_2, \cdots, x_t$, be held in some given order, and consider all the possible ways in which a series of t y-elements, $y_1, y_2, \cdots, y_t$, can be arranged against it. In what proportion of these ways will there result exactly s correct matchings between members of the two series?

Since we wish the general expression for the number of permutations of t elements which will give us exactly s correct matchings, we begin by putting s of the y elements in the same position as the corresponding s x-elements, and we shall hold them in this position throughout the following discussion. This leaves $(t-s)$ elements, which can be arranged in $(t-s)!$ permutations. Therefore our first term in the solution is

$$(t-s)! \ldots\ldots\ldots\ldots\ldots\ldots [\text{i}]$$

But this number is too large; for some of these $(t-s)!$ permutations are such that at least one of the elements in them is correctly matched with an element in the x-series. We must therefore subtract the number of such cases, since we wish only the original s elements to match. The number to be subtracted can be determined by making one of the $(t-s)$ elements match, which leaves $(t-s-1)$ elements, capable of $(t-s-1)!$ permutations. Since, however, the one matching element can be chosen in $(t-s)$ ways from amongst the $(t-s)$, and since we have $(t-s-1)!$ permutations of the remaining elements for each such choice, the number of permutations of $(t-s)$ elements such that at least one element matches is the product of these two expressions. We therefore subtract $(t-s-1)!\ (t-s)=(t-s)!$ from (i) above, giving as the first two terms of the answer

$$(t-s)!-(t-s)! \ldots\ldots\ldots\ldots\ldots\ldots [\text{ii}]$$

This number is too small, for the following reason: Suppose the element held constant in the above operation were y_i. Then the number of permutations of the remaining $(t-s-1)$ elements includes permutations in which also the jth (let us say) element matches. But we have just subtracted such a permutation as this twice over—once for the matching of y_i and once again for the matching of y_j. To correct this error, we must restore all permutations of the $(t-s)$ elements in which at least two elements match. Therefore we now hold two of the $(t-s)$ elements matched, which can be done in $(t-s)!/(t-s-2)!2!$ ways, and for each way there are $(t-s-2)!$ permutations of the remaining elements. Consequently we add to (ii) the product $[(t-s)!/(t-s-2)!2!]\ (t-s-2)!=(t-s)!/2!$, giving as the first three terms of the answer

$$(t-s)! - (t-s)! + [(t-s)!/2!] \ldots\ldots\ldots\ldots [\text{iii}]$$

By analogy with the last step, we must now subtract the number of permutations containing at least three matchings, since this last term has restored the number of such cases twice over. If we continue these corrections, alternately adding and subtracting a term, we shall get the alternating convergent series

$$\frac{(t-s)!}{0!} - \frac{(t-s)!}{1!} + \frac{(t-s)!}{2!} - \frac{(t-s)!}{3!} + \frac{(t-s)!}{4!} \cdots,$$

which is the first terms of the expansion of $1/e$. The last term will be $(-1)^{t-s}(t-s)!/(t-s)!$, where the factor $(-1)^{t-s}$ indicates that the sign of the term will be positive if $(t-s)$ is even and negative if $(t-s)$ is odd.

But this whole series must, finally, be multiplied by $t!/(t-s)!s!$, which is the number of ways in which we might have chosen the original s elements which we have held constant throughout the discussion. The *number* of permutations of t elements in which exactly s of the elements match s elements in a standard series is therefore given by

$$\frac{t!}{(t-s)!s!}\left[\frac{(t-s)!}{0!} - \frac{(t-s)!}{1!} + \frac{(t-s)!}{2!} - \cdots + (-1)^{t-s}\frac{(t-s)!}{(t-s)!}\right],$$

which reduces to[2]

$$\frac{t!}{s!}\left[\frac{1}{0!} - \frac{1}{1!} + \frac{1}{2!} - \cdots + (-1)^{t-s}\frac{1}{(t-s)!}\right] \ldots\ldots\ldots [1]$$

If (as is generally more to the point) we wish to know the *proportion* of arrangements which will give exactly s matchings—in other words, the probability that a single arrangement will give s matchings—the expression (1) must be divided by the total possible number of arrangements of t things, which is $t!$ and which gives

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[2] I have been shown by Dr. Hassler Whitney of the Harvard Department of Mathematics that the expression following can be directly derived from an expansion in symbolic logic. The present demonstration is probably clearer to the non-mathematician, though inductively less satisfactory.

$$\frac{1}{s!}\left[\frac{1}{0!}-\frac{1}{1!}+\frac{1}{2!}-\cdots+(-1)^{t-s}\frac{1}{(t-s)!}\right]\ldots\ldots\ldots [2]$$

I have evaluated this last expression to four decimal places for all values of t and s; the results are given in Table I. The probabilities associated with any value of s converge so rapidly that actual calculation beyond $t=9$ is unnecessary.

TABLE I

THE PROBABILITY OF OBTAINING EXACTLY s CORRECT MATCHINGS WHEN t THINGS ARE ARRANGED

t	$s \rightarrow 0$	1	2	3	4	5	6	7	8	9	10
1	.0000	1.0000									
2	.5000	.0000	.5000								
3	.3333	.5000	.0000	.1667							
4	.3750	.3333	.2500	.0000	.0417						
5	.3667	.3750	.1667	.0833	.0000	.0083					
6	.3681	.3667	.1875	.0556	.0208	.0000	.0014				
7	.3679	.3681	.1833	.0625	.0139	.0042	.0000	.0002		All further	
8	.3679	.3679	.1840	.0611	.0156	.0028	.0007	.0000	.0000	terms approx-	
$t \geqq 9$	.3679	.3679	.1839	.0613	.0153	.0031	.0005	.0001	.0000	imate .0000	

(2) *The probability that a single arrangement of t elements will give s or more correct matchings.* We cannot argue directly from the probabilities in Table I to the significance of an obtained number of correct matchings; for the real test of the significance of an obtained s is the probability of obtaining by chance a value of s as great *or greater*. Now, if P_s, $P_{(s+1)}, \cdots$ are the probabilities respectively of obtaining exactly s, $(s+1), \cdots$ correct matchings, the probability of obtaining *s or more* correct matchings is given by

$$P_{(s)}+P_{(s+1)}+\cdots+P_{(t)}.$$

Hence, if Table I is summed from right to left, we have Table II, which shows the probability of obtaining *s or more* correct matchings from a single arrangement of t elements.

As an example of the use of this table, let us consider an actual experimental case. In Power's study of handwriting (reported by Allport and Vernon), one graphologist correctly matched 5 out of 10 scripts with an equal number of character sketches. What is the significance of this performance? From Table II, opposite $t=10$ and $s=5$, we find that as good a result as this or better should be expected by chance only 37 times in 10,000.

(3) *The significance of the mean number of correct matchings as the result of n independent trials.*[3] Suppose that each of n Ss attempts to arrange a

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[3] I wish to thank Professor E. V. Huntington of the Harvard Department of Mathematics for a number of very helpful suggestions on problems involved in this section.

series of t elements against a second such series. The first S produces s_1 correct matchings, the second S produces s_2 correct matchings, etc. If we let $\bar{s}$ represent the mean number of correct matchings for the n Ss,

$$\bar{s} = \frac{s_1 + s_2 + \cdots + s_n}{n},$$

what will be the probability of obtaining a given value of $\bar{s}$? We can answer this question if we can describe the distribution of the means of samples made up of n s's each, taken from a universe where the probability of drawing an s of a given value is the probability shown in Table I opposite that s and the proper t.

TABLE II

THE PROBABILITY OF OBTAINING s OR MORE CORRECT MATCHINGS WHEN t THINGS ARE ARRANGED

t \ $s\to$ 0	1	2	3	4	5	6	7	8	9	10
1	1.0000	1.0000								
2	1.0000	.5000	.5000							
3	1.0000	.6667	.1667	.1667						
4	1.0000	.6250	.2917	.0417	.0417					
5	1.0000	.6333	.2583	.0916	.0083	.0083				
6	1.0000	.6319	.2652	.0777	.0221	.0014	.0014			
7	1.0000	.6321	.2641	.0808	.0183	.0044	.0002	.0002		All further
8	1.0000	.6321	.2642	.0802	.0191	.0035	.0007	.0000	.0000	terms approx-
$t \geqq 9$	1.0000	.6321	.2642	.0803	.0190	.0037	.0006	.0001	.0000	imate .0000

Let us consider first the distribution for $t = 5$, and then generalize from it. If we tabulate this distribution in terms of Equation [2], we have the equations of Table III.

Now the mean of any distribution is given by the sum of the products of its items by their relative frequencies. Hence, multiplying the values of s (Table III) by their relative frequencies, and remembering that $1/1! = 1/0!$, $2/2! = 1/1!$, or in general, $a/a! = 1/(a-1)!$, we have the terms shown in Table IV. Consider the sum of these terms as the sum of the diagonals which they form, from lower left to upper right. Excepting only the diagonal containing the single term $1/0!0!$ $(=1)$, every diagonal is of the general form

$$\frac{1}{m!0!} - \frac{1}{(m-1)!1!} + \frac{1}{(m-2)!2!} - \cdots + (-1)^{m-1}\frac{1}{1!(m-1)!} + (-1)^m \frac{1}{0!m!} \ldots\ldots\ldots [a]$$

TABLE III

EQUATIONS FOR DISTRIBUTION OF CORRECT MATCHINGS WHEN $t=5$

s	Relative frequency
0	$\frac{1}{0!}\left[\frac{1}{0!}-\frac{1}{1!}+\frac{1}{2!}-\frac{1}{3!}+\frac{1}{4!}-\frac{1}{5!}\right]=\frac{1}{0!0!}-\frac{1}{0!1!}+\frac{1}{0!2!}-\frac{1}{0!3!}+\frac{1}{0!4!}-\frac{1}{0!5!}$
1	$\frac{1}{1!}\left[\frac{1}{0!}-\frac{1}{1!}+\frac{1}{2!}-\frac{1}{3!}+\frac{1}{4!}\right]=\frac{1}{1!0!}-\frac{1}{1!1!}+\frac{1}{1!2!}-\frac{1}{1!3!}+\frac{1}{1!4!}$
2	$\frac{1}{2!}\left[\frac{1}{0!}-\frac{1}{1!}+\frac{1}{2!}-\frac{1}{3!}\right]=\frac{1}{2!0!}-\frac{1}{2!1!}+\frac{1}{2!2!}-\frac{1}{2!3!}$
3	$\frac{1}{3!}\left[\frac{1}{0!}-\frac{1}{1!}+\frac{1}{2!}\right]=\frac{1}{3!0!}-\frac{1}{3!1!}+\frac{1}{3!2!}$
4	$\frac{1}{4!}\left[\frac{1}{0!}-\frac{1}{1!}\right]=\frac{1}{4!0!}-\frac{1}{4!1!}$
5	$\frac{1}{5!}\left[\frac{1}{0!}\right]=\frac{1}{5!0!}$

TABLE IV

MEAN OF DISTRIBUTION OF CORRECT MATCHINGS WHEN $t=5$

s	$s\times$(relative frequency)
0	—— —— —— —— —— ——
1	$\frac{1}{0!0!}-\frac{1}{0!1!}+\frac{1}{0!2!}-\frac{1}{0!3!}+\frac{1}{0!4!}$
2	$\frac{1}{1!0!}-\frac{1}{1!1!}+\frac{1}{1!2!}-\frac{1}{1!3!}$
3	$\frac{1}{2!0!}-\frac{1}{2!1!}+\frac{1}{2!2!}$
4	$\frac{1}{3!0!}-\frac{1}{3!1!}$
5	$\frac{1}{4!0!}$

But the sum of the terms in this typical series can be shown to equal 0; for, consider the binomial expansion of $(a-b)^m$:

$$(a-b)^m = {}_mC_0a^m - {}_mC_1a^{m-1}b + {}_mC_2a^{m-2}b^2 - \cdots + (-1)^{m-1}{}_mC_{m-1}ab^{m-1} + (-1)^m{}_mC_mb^m.$$

Setting $a = 1$ and $b = 1$, and writing the coefficients in terms of factorials, we have

$$(1-1) = \frac{m!}{m!0!} - \frac{m!}{(m-1)!1!} + \frac{m!}{(m-2)!2!} - \cdots + (-1)^{m-1}\frac{m!}{1!(m-1)!} + (-1)^m \frac{m!}{0!m!}.$$

But $(1-1)^m = 0^m = 0$. Therefore

$$m!\left[\frac{1}{m!0!} - \frac{1}{(m-1)!1!} + \frac{1}{(m-2)!2!} - \cdots + (-1)^{m-1}\frac{1}{1!(m-1)!} + (-1)^m \frac{1}{0!m!}\right] = 0,$$

and therefore the expression in brackets, which is the same as [a] above, equals zero. Consequently every diagonal but the first in our tabulation has the value 0, and only the first diagonal, whose value is 1, contributes to the mean. The mean of the distribution is therefore 1.

It will be seen at once that this holds true not only when $t = 5$, as in our example, but for any value of t greater than 1; for the effect of increasing or diminishing t is simply to add or subtract a number of diagonals, each of which equals 0.

Analogously, we can determine the moments of the distribution of s's, namely μ_2, μ_3, and μ_4, by investigating the sums of the products: $s^2 \times$(relative frequency), $s^3 \times$(relative frequency), and $s^4 \times$(relative frequency), and correcting these sums for the mean as an origin. The demonstration that diagonals of certain orders in these sums are equal to 0 is more complicated than the demonstration in the case of the mean. But by subtracting from them series like [a] above, and performing certain obvious reductions, the proof can be performed.

From these moments, the measures of skewness and kurtosis, β_1 and β_2, can be calculated, since $\beta_1 = \mu_3^2/\mu_2^3$, and $\beta_2 = \mu_4/\mu_2^2$. The results are shown, for the various values of t, in Table V.

We have, then, the convenient discovery that these constants become invariable at $t = 4$. Since it seems more than likely that values of 4 and higher for t contain all the experimentally useful cases, we shall confine our attention to that range.

We have, therefore, to predict the distribution of mean values of s ($\bar{s}$) for

TABLE V

CONSTANTS OF THE DISTRIBUTION OF s FOR GIVEN VALUES OF t IN TABLE II.

	Constant					
t	Mean	μ_2	μ_3	μ_4	β_1	β_2
1	1	0	0	0	—	—
2	1	1	0	1	0	1
3	1	1	1	3	1	3
$t \geqq 4$	1	1	1	4	1	4

samples of size n drawn from the parent population having the constants in the last row of Table V.

Now it is known that the following relations hold between the constants of a parent distribution (irrespective of its abnormality) and the constants of the distribution of means of all possible samples of size n drawn from the parent population:[4]

$$\text{(a)} \qquad M_{\text{means}} = M_{\text{parent}},$$

$$\text{(b)} \qquad \sigma_{\text{means}} = \frac{\sigma_{\text{parent}}}{\sqrt{n}},$$

$$\text{(c)} \qquad \beta_{1\,\text{means}} = \frac{\beta_{1\,\text{parent}}}{n},$$

$$\text{(d)} \qquad \beta_{2\,\text{means}} = 3 + \frac{\beta_{2\,\text{parent}} - 3}{n}.$$

Applying the values opposite $t \geqq 4$ in Table III to these equations, we have Table VI. The distribution of $\bar{s}$ for any value of n is therefore skewed

TABLE VI

CONSTANTS OF THE DISTRIBUTION OF MEANS ($\bar{s}$) OF SAMPLES OF SIZE n, WHEN $t \geqq 4$.

$$M = 1 \qquad \sigma = \frac{1}{\sqrt{n}} \qquad \beta_1 = \frac{1}{n} \qquad \beta_2 = 3 + \frac{1}{n}$$

($\beta_1 < 1$) and leptokurtic ($\beta_2 > 3$), although these two abnormalities decrease as n becomes infinite.[5] Furthermore, since for any value of n, Pearson's criterion κ is negative, the distribution of $\bar{s}$ can be best represented by his Type I curve. We could therefore determine very closely the significance of any obtained value of $\bar{s}$ by calculating the ordinates of a Type I distribution having the moments given in Table VI (for the appropriate value of n), and by then summing the ordinates at $\bar{s}$ and at all higher values which the mean can take, as a method of approximate quadrature.

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[4] Cf. B. H. Camp, *The Mathematical Part of Elementary Statistics*, 1931, 245, Theorem II. Camp's $\alpha_3 = \sqrt{\beta_1}$, and his $\alpha_4 = \beta_2$. The useful equations (c) and (d) are curiously infrequent in the common statistical texts.

[5] Hence the probabilities for average results in the table on p. 230 of Allport and Vernon, *op. cit.*, are erroneous. I am unable to deduce the procedure used in that table. The probabilities (P) appear to be the result of interpolation amongst such values as occur in our Table I. If that is so, the method is wrong on three counts: first, such an interpolation gives the probability of obtaining *exactly* s, not s or more, correct matchings, and is therefore of no use as a measure of significance; secondly, such values do not apply anyway to the significance of the mean performance of a group of judges (for this is the problem we are now treating); and,

But this solution of the problem is in practice inconvenient, since the calculation of a Type I curve is at best irksome, and would have to be repeated for all the values of n involved in any actual problem. Very happily, however, a Type III distribution curve gives an extremely close approximation to the Type I curve within the limits of skewness with which we are concerned;[6] and for the Type III curve we have Salvosa's excellent tables of areas for different deviations from the mean and for different degrees of skewness.[7]

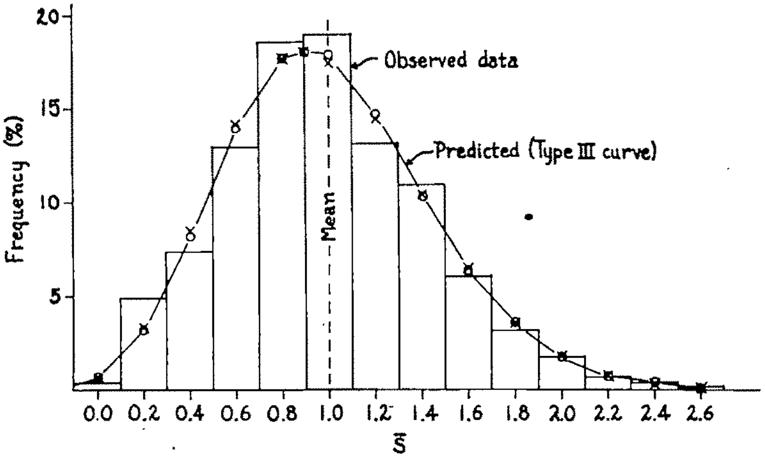

FIG. 1. SHOWING PREDICTED AND OBSERVED FREQUENCIES

Fig. 1 shows the ordinates of the Type I curve (crosses) and of the Type III curve (circles) for the case that $n=5$; and it shows, for comparison, a histogram of the results of the following empirical example: Ten cards, numbered 1 to 10, were shuffled and laid out in a series, and the number of cards matching their position in the series was recorded. This procedure was repeated, by the members of my class in statistics, in all 4,500 times. These 4,500 empirical values

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thirdly, the columns following P in Allport and Vernon's table give areas and critical ratios evidently taken from the normal curve, whereas we have just seen that the values of $\bar{s}$ are not normally distributed.

[6] On the fitting of Type I and III curves, see D. C. Jones, A *First Course in Statistics*, 1929, chap. 16.

[7] L. R. Salvosa, Tables of Pearson's Type III function, *Ann. Math. Statist.*, 1, 1930, 191–198. Salvosa's t (the argument) $=x/\sigma$, and the degrees of skewness are values of $\sqrt{\beta_1}$.

of s were then sampled by successive fives, the mean of which was computed, thus affording 900 values of $\bar{s}$. It will be seen that both the Type I and the Type III curves give a reasonable description of the empirical data; and, more important, that the two types are practically identical.

Finally, since Salvosa's Tables may be inaccessible to some, and for the sake of convenience, I present Table VII, which gives the probability (p) of obtaining a mean number of correct matchings as great as $\bar{s}$ or greater from n independent trials (*e.g.* from n judges or Ss) when the number of things to be matched (t) in each series is four or more. The table was calculated by interpolation in Salvosa's Tables. The values of n run by units from 2 to 15, and thence by fives to 30. The argument, $\bar{s}$, can, of course, take only certain values (since the total number of correct matchings for n judges must be an integer); and I have given all these possible values of $\bar{s}$, from 1 (which is the mean chance performance) up to whatever value gives $p = 0.000$ to three places.

TABLE VII

Showing the Probability (p) that the Mean Number of Correct Matchings Resulting from n Independent Trials will be as Great as $\bar{s}$ or Greater, when $t \geqq 4$

$n=2$		$n=3$		$n=4$		$n=5$	
$\bar{s}$	p	$\bar{s}$	p	$\bar{s}$	p	$\bar{s}$	p
1.000	.597	1.000	.580	1.000	.568	1.000	.559
1.500	.325	1.333	.354	1.250	.371	1.200	.386
2.000	.143	1.667	.186	1.500	.215	1.400	.134
2.500	.054	2.000	.085	1.750	.111	1.600	.068
3.000	.018	2.333	.035	2.000	.052	1.800	.033
3.500	.005	2.667	.013	2.250	.022	2.000	.015
4.000	.002	3.000	.004	2.500	.009	2.200	.006
4.500	.000	3.333	.001	2.750	.003	2.400	.002
		3.667	.000	3.000	.001	2.600	.001
				3.250	.000	2.800	.000

$n=6$		$n=7$		$n=8$		$n=9$	
$\bar{s}$	p	$\bar{s}$	p	$\bar{s}$	p	$\bar{s}$	p
1.000	.553	1.000	.551	1.000	.549	1.000	.546
1.167	.395	1.143	.401	1.125	.408	1.111	.412
1.333	.257	1.286	.270	1.250	.284	1.222	.294
1.500	.153	1.429	.171	1.375	.185	1.333	.198
1.667	.085	1.571	.099	1.500	.112	1.444	.124
1.833	.043	1.714	.054	1.625	.064	1.556	.074
2.000	.021	1.857	.028	1.750	.035	1.667	.042
2.167	.010	2.000	.013	1.875	.018	1.778	.032
2.333	.004	2.143	.006	2.000	.009	1.889	.011
2.500	.001	2.286	.003	2.125	.004	2.000	.006
2.667	.000	2.429	.001	2.250	.002	2.111	.003
		2.571	.000	2.375	.001	2.222	.001+
				2.500	.000	2.333	.001−
						2.444	.000

TABLE VII (CONTINUED)

$n=10$		$n=11$		$n=12$		$n=13$	
$\bar{s}$	p	$\bar{s}$	p	$\bar{s}$	p	$\bar{s}$	p
1.000	.543	1.000	.540	1.000	.537	1.000	.536
1.100	.416	1.091	.421	1.083	.426	1.077	.427
1.200	.304	1.182	.312	1.167	.419	1.154	.323
1.300	.209	1.273	.220	1.250	.228	1.231	.237
1.400	.135	1.364	.145	1.333	.156	1.308	.165
1.500	.084	1.455	.092	1.416	.101	1.385	.110
1.600	.049	1.545	.056	1.500	.063	1.462	.070
1.700	.027	1.636	.033	1.583	.038	1.538	.043
1.800	.015	1.727	.018	1.667	.022	1.615	.026
1.900	.008	1.818	.010	1.750	.012	1.692	.014
2.000	.004	1.909	.005	1.833	.006	1.769	.008
2.100	.002	2.000	.002	1.916	.003	1.846	.004
2.200	.001	2.091	.001+	2.000	.002	1.923	.002
2.300	.000	2.128	.001−	2.083	.001	2.000	.001+
		2.273	.000	2.167	.000	2.077	.001−
						2.154	.000

$n=14$		$n=15$		$n=20$		$n=25$		$n=30$	
$\bar{s}$	p	$\bar{s}$	p	$\bar{s}$	p	$\bar{s}$	p	$\bar{s}$	p
1.000	.533	1.000	.534	1.000	.529	1.000	.527	1.000	.524
1.071	.431	1.067	.432	1.050	.445	1.040	.447	1.033	.452
1.143	.331	1.133	.335	1.100	.355	1.080	.371	1.067	.384
1.214	.243	1.200	.251	1.150	.279	1.120	.300	1.100	.314
1.286	.173	1.267	.181	1.200	.213	1.160	.237	1.133	.255
1.357	.118	1.333	.125	1.250	.156	1.200	.182	1.167	.203
1.429	.077	1.400	.083	1.300	.113	1.240	.137	1.200	.158
1.500	.048	1.467	.053	1.350	.079	1.280	.101	1.233	.119
1.571	.029	1.533	.037	1.400	.052	1.320	.072	1.267	.089
1.643	.017	1.600	.020	1.450	.035	1.360	.050	1.300	.065
1.714	.010	1.667	.011	1.500	.022	1.400	.034	1.333	.047
1.786	.005	1.733	.006	1.550	.014	1.440	.023	1.367	.032
1.857	.003	1.800	.003	1.600	.008	1.480	.015	1.400	.022
1.929	.002	1.867	.002	1.650	.005	1.520	.009	1.433	.015
2.000	.001	1.933	.001+	1.700	.003	1.560	.006	1.467	.010
2.071	.000	2.000	.001−	1.750	.002	1.600	.004	1.500	.006
		2.067	.000	1.800	.001	1.640	.002	1.533	.004
				1.850	.000	1.680	.001+	1.567	.002+
						1.720	.001−	1.600	.002−
						1.760	.000	1.633	.001+
								1.667	.001−
								1.700	.000

The following examples will illustrate the use of Table VII.

(1) Eleven judges match six samples of handwriting with six character-sketches. The mean number of correct matchings for the group is 1.727. Under $n=11$, opposite 1.727, we find that so good a performance may be expected by chance only 18 times in 1,000.

(2) Fifteen judges achieve a mean performance of 2.2 correct matchings. Entering the column for $n=15$, we find that $\bar{s}=2.2$ is beyond the range of the table. Therefore this result may be expected less than once in 1,000 chance trials.

(3) Cantril and Rand's group of 26 laymen matched six scripts and six sketches with an average of 1.1 correct matchings. Interpolating between the values of p for $\bar{s}=1.08$ and

$\bar{s} = 1.12$ in column ($n = 25$), and then interpolating between this result and the value of p for $\bar{s} = 1.1$ in column ($n = 30$), we arrive at $p = 0.332$, approximately.

(4) In Power's study, 185 judges achieved a mean matching of 1.834. From Table IV, using $n = 185$, we find for the distribution of such means, $\sigma = .0735$, $\beta_1 = .0054$, and the skewness, $\sqrt{\beta_1}, = 0.0735$. The next lower possible value of the mean than 1.834 is 1.827; and the lower boundary of the class interval whose midpoint is 1.834 is therefore halfway between these values, or 1.838. In units of σ, this represents a deviation from the mean of the means (1.0) amounting to 11.4. Entering Salvosa's tables with $t = 11.4$ and skewness $= 0.07$, we find that we are greatly beyond the range of his six-place tabulation. The probability of 1.834 as the mean achievement of 185 judges by chance is therefore far below 1 in 1,000,000 (as against the erroneous value of 0.208 in Allport and Vernon's table)!

JOST'S LAW AND RETROACTIVE INHIBITION

By STEUART H. BRITT, Yale University, and MARION E. BUNCH, Washington University

A series of experiments in the psychology of learning was conducted by Jost in 1897.[1] On the basis of the results he formulated a statement of the relationship between the efficacy of practice and the age of associations, when the associative connections involved are regarded as being equal in functional strength just prior to the introduction of the new practice period. Warren and Carmichael state Jost's conclusions as twofold.[2] "When two associative connections are originally of equal strength but of unequal age, new repetition increases the strength of the earlier more than of the later association." Similarly, the older of the two associations "fades less rapidly than does the newer." These two generalizations, known as Jost's Law, appear fairly frequently in the literature. The limiting conditions of this law, its relation to the problems involved in the economy of distributed as compared with massed effort, and to the problem of retention, have received scant attention.

The experimental work in regard to retroactive inhibition indicates that the amount of retroaction varies with a number of factors; *e.g.* the similarity between the original and the interpolated problem, the temporal position of interpolation, the degree of learning, the affective character of the material learned, and the conditions under which learning takes place. If the effectiveness of a given repetition upon learning is to some extent a function of the age of the associative connections, then, from the standpoint of retention, it is probable that the relative susceptibility of these associations to the detrimental effect of subsequent learning is to some extent also a function of the age variable. The question arises, then, as to which of two associations of the same strength but of different age is the more susceptible to retroactive inhibition. If, for example, maze habits are established in two instances so that they are of the same functional strength, as indicated by the level of efficiency in performance, but of unequal age, will the older or the younger maze solution be more detrimentally affected by the introduc-

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* Accepted for publication December 12, 1932. The experimental data upon which this report is based were collected at Washington University, St. Louis, during 1931-1932.

[1] Adolf Jost, Die Assoziationsfestigkeit in ihrer Abhängigkeit von der Verteilung der Wiederholungen, *Zsch. f. Psychol.*, 14, 1897, 436-472.

[2] H. C. Warren and L. Carmichael, *Elements of Human Psychology*, 1930, 211 ff.

tion of another maze problem before the retention test? The present experiment was conducted to answer this question.

METHOD OF PROCEDURE

It was necessary to establish for each of two groups of *Ss* certain associations of equal strength, the two groups differing from each other only in that the associations for one group had been established for a longer period than for the other. It was also necessary to introduce an interpolated problem during the interval before the retention test in one-half of the cases in each of the two 'age'-groups, in order that comparisons might be made as to the relative amount of retroactive inhibition. The amount of retroaction when the associations in the original problem have been established for a longer period can then be compared with the amount found when the associations have been more recently formed.

For the two learning problems two stylus mazes were used, which will be designated as Maze I and Maze II. Maze I in every case was used for the original problem, and Maze II was employed in every case for the interpolated problem. Maze I was a brass maze of the same pattern as that used by Carr in his experiment on visual guidance,[3] while Maze II was made of fiber-board and was not as difficult as Maze I. Diagrams of the maze patterns have been presented in another study.[4]

The *Ss* for the experiment were students from the Elementary Psychology classes of Washington University. There were 120 *Ss* (68 men and 52 women), none of whom had had previous maze experience.

The 120 *Ss* were divided into two groups of 60 each on the basis of the age of the associative connections, two ages being employed. They may be referred to as the older and younger 'age'-groups. In order to determine the relative amount of retroactive inhibition in the two cases, each age-group of 60 *Ss* was subdivided into two groups on the basis of the conditions of interpolation: one sub-group learned a second maze during the interval before the test for the retention of the original problem, and the other did not. Within each age-group, then, a comparison of the former sub-group with the latter on the basis of the efficiency of retention indicates the amount of retroactive inhibition.

The individuals were selected for a particular group in random order. The 4 groups were designated as: Group 1 (experimental), and Group 2 (control for Group 1); Group 3 (experimental), and Group 4 (control for Group 3). The following table, Table I, presents the program of the conditions of learning and relearning for each of the 4 groups.

The individuals in Group 1 learned Maze I to complete mastery. They relearned Maze I to the same criterion after an interval of 48 days, and were immediately given Maze II to learn. Immediately following the mastery of Maze II, they were given a retention test on Maze I.

Group 2 also learned Maze I to complete mastery, and after a 48-day interval relearned Maze I to the same criterion. However, after relearning Maze I, they were given a 20-min. rest period. Following this period they were given a retention test on Maze I.

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[3] H. A. Carr, The influence of visual guidance in maze learning, *J. Exper. Psychol.*, 4, 1921, 399-417.

[4] S. H. Britt, The relationship between transfer of learning and age of previous associations, this JOURNAL, 46, 1934, 113-116.

Group 3 learned Maze I to mastery, then learned Maze II to mastery, and immediately took a retention test on Maze I.

Group 4 learned Maze I to mastery, rested 20 min., and then took a retention test on Maze I.

TABLE I

PROGRAM OF CONDITIONS OF LEARNING FOR EACH GROUP
(30 Ss in each group)

Group	Learning	Interval	Conditions employed to reach level of complete mastery	Conditions of interpolated interval	Retention test
1	Maze I	48 days	Maze I (Relearning)	Maze II	Maze I
2	Maze I	48 days	Maze I (Relearning)	20-min. rest*	Maze I
3			Maze I (Learning)	Maze II	Maze I
4			Maze I (Learning)	20-min. rest*	Maze I

* Refers to reading activity.

The criterion of complete mastery was the same in every case: 3 errorless trials in succession.

With Groups 2 and 4, the 20-min. rest period was chosen in order that the time-interval between complete mastery and the retention test would be comparable to the time-interval necessary for the work conditions of Groups 1 and 3. This 20-min. rest period consisted of reading a current issue of *College Humor*. *S* was instructed to begin reading wherever the first joke or cartoon appeared in the magazine and to note in writing those stories and cartoons which he considered funny or of humorous value. *S* was told that the scoring of the jokes was a second part of the experiment. This method was used for two reasons: first, it insured as much as possible against review of the solution of the problem during the rest interval; and, secondly, it filled the time-interval with fairly normal non-learning activities.

Three measures of learning efficiency were employed for each maze problem. The total number of trials, the total length of time in seconds (both of these records including the last 3 perfect trials), and the total number of errors were recorded.

In addition to the conditions just described, it was important to control two factors with respect to the associative connections in the original problem. (1) At the beginning of the 20-min. interval for which retention was to be tested, these associative connections in the two age-groups should be of unequal age. This factor was controlled by having Groups 1 and 2 learn the original problem 48 days before the time the problem was mastered by Groups 3 and 4. (2) At the beginning of this 20-min. interval, the associative connections in the two age-groups should be of equal functional strength. This comparability on the basis of functional strength was accomplished by having both age-groups meet the same criterion of mastery immediately before the 20-min. interpolated period. Thus, the individuals in the younger age-groups (Groups 3 and 4) mastered the original problem just prior to this interval; the individuals in the older age-groups (Groups 1 and 2) mastered the original problem 48 days before this interval and then, just prior to the 20-min. interval, relearned the maze to three errorless trials in succession. The purpose of this second mastery for the two older age-groups was to bring these older age-groups up to the same level of mastery as that which the younger age-groups had attained immediately preceding the interval of retention. The associative connections at this point were of equal functional strength for all groups in the sense that all individuals in each group reached the same level of efficiency in terms of maze performance.

Groups 1 and 2 mastered Maze I two different times, before the 20-min. interval of retention, and hence had more practice on this problem than did Groups 3 and 4. It might be suggested that this element of additional practice would be an important factor in the final retention scores. This is probably true, but the criticism is not a serious one either of the procedure or of the results. In any situation in which one group of associations is older than another and the associations have been equated as to strength, it must necessarily be true that the older associations, simply because of the fact that they have been so equated, will have been more practiced than the younger or newer associations. It would have been possible in this experiment to give the older age-groups, Groups 1 and 2, the interpolated activity after the 48-day interval without the re-mastery of the original problem. However, in that case the functional strength of the associative connections in Groups 1 and 2, as the result of some forgetting during the interval, would not have been known and consequently it would have been impossible to equate the younger with the older age-groups on the basis of the strength of associations. Therefore it was considered to be better procedure to equate the older and the younger age-groups, just prior to the interval of retention, on the basis of the efficiency of maze performance by having all individuals meet the same criterion of mastery.

RESULTS

The experimental results are presented under 4 separate headings: *(1)* the amount of retroactive inhibition occurring in the retention of the older associations; *(2)* the amount of retroactive inhibition found in the retention of the younger associations; *(3)* the relative amount of retroactive inhibition under conditions *(1)* and *(2)*; *(4)* the influence of the age of association upon retention.

(1) The amount of retroactive inhibition occurring in the retention of the older associations. The amount of retroaction that occurred in the retention of the older associations is obtained by comparing the retention scores of Group 1 with those of Group 2. These two groups are alike in that they learned Maze I and then relearned it 48 days later, just prior to the interval for which retention was tested. They differ as to the activities which filled this interval of retention. The interpolated activities were reading (Group 2), and learning another maze (Group 1). The influence of the interpolated maze upon the retention of the first maze-solution is indicated by the differences in retention scores between the two groups.

The relearning, recall, and saving scores of Groups 1 and 2 are shown in Table II. Differences between the two groups are presented in Table III. These data show that the acquisition of the second maze during the interval interfered with the retention of the first problem according to 6 of the 8 measures of retention employed. No measurable difference between the two groups appeared in terms of the amount of time taken in relearning the first maze. A slightly facilitating effect occurred, after the interpolation of the second problem, in terms of time of recall (amount of time taken on the first relearning trial). The detrimental effect of the interpolated problem upon the retention of the previously learned maze solution is indicated by: an increase of 27.7% in the number of trials required in relearning, an increase of 45.3% in the number of errors made in relearning; an increase of 98.0% in errors of recall; and, in regard to saving scores, a 9.64% less saving in the number of trials, a 2.43% less saving in the amount of time, and a 1.27% less saving in the number of errors. Thus a comparison of the retention scores for the two groups indicates that the interpolated problem had a slightly detrimental effect upon the retention of the first problem in terms of saving scores, errors and trials of relearning, and errors of recall. The ratio of each difference to its σ (Table III) indicates that the differences are only fairly reliable. According to saving scores, the margin of superiority of Group 2 over Group 1 is not large, although the facts are consistent in indicating poorer retention under the interpolated learning condition.

(2) The amount of retroactive inhibition found in the retention of the younger associations. The second set of results are those obtained under the conditions in which the interpolated problem was given immediately after the complete mastery of the original problem. In order to determine the amount of retroaction in the younger associations, it is necessary to compare Group 3 with Group 4 on the basis of the efficiency of retention of Maze I. These two groups differed from each other only as to the activity during the interval between the mastery and the relearning of Maze I. The individuals in Group 4 rested for 20 min., while those in Group 3 learned Maze II.

The retention scores for each group are presented in Table II; comparative retention scores are shown in Table III. The influence of the interpolated maze upon the retention of the first maze was detrimental according to all criteria employed in measuring retention. The effect of learning the interpolated problem upon the retention of the previously learned maze solution was: in terms of relearning scores, an increase of 120.0% in the number of trials required, an increase of 230.3% in the amount of time taken, and an increase of 927.2% in the number of errors made; in terms of recall scores, an increase of 203.3%

TABLE II

RETENTION DATA FOR MAZE

	Relearning						% Saving			Recall			
	Trials		Time		Errors					Time		Errors	
Group	Av. no.	σ_{av}	Av. sec.	σ_{av}	Av. no.	σ_{av}	Trials	Time	Errors	Av. sec.	σ_{av}	Av. no.	σ_{av}
1	8.47	2.35	128.97	23.52	10.07	3.28	68.00	87.12	96.98	25.67	1.35	4.10	.77
2	6.63	.87	128.63	13.24	6.93	1.64	77.64	89.55	98.25	26.53	1.96	2.07	.65
3	15.03	1.68	428.60	1.35	117.83	59.01	28.21	57.78	63.59	74.63	18.64	34.90	11.07
4	6.83	.77	129.76	18.98	11.47	4.82	71.52	88.88	97.95	24.60	1.53	2.53	.85

in the amount of time taken, and an increase of 1279.4% in the number of errors made; and, in regard to saving scores, a 43.31% less saving in the number of trials, a 31.11% less saving in the amount of time, and a 34.36% less saving in the number of errors. Thus the facts are highly consistent in indicating a marked decrease in the efficiency of retention under the conditions of interpolated learning (Group 3) as compared with those conditions in which no second problem was mastered (Group 4). Statistically, the differences between the two groups in relearning and recall scores have a rather high degree of reliability, as indicated by the ratio of each difference to its σ (Table III). The chances in 100 that the direction of the difference would be confirmed by subsequent work are 96 or better.

TABLE III

GROUP DIFFERENCES IN RETENTION

Groups compared	Relearning			% Saving			Recall	
	Trials	Time	Errors	Trials	Time	Errors	Time	Errors
1 minus 2	1.84	.34	3.14	9.64	2.43	1.27	−.86*	2.03
σ diff.	2.51	26.99	3.67				2.38	1.00
D/σ diff.	.73	.01	.86				.36	2.03
%1>2	27.70	0.00	45.30				−3.20*	98.00
3 minus 4	8.20	298.84	106.36	43.31	31.11	34.36	50.03	32.37
σ diff.	1.85	136.33	59.21				18.70	11.10
D/σ diff.	4.43	2.19	1.80				2.68	2.92
%3>4	120.00	230.30	927.20				203.3	1279.4
3 minus 1	6.56	299.63	107.76	39.79	29.34	33.39	48.96	30.80
σ diff.	2.89	137.03	59.10				18.69	11.10
D/σ diff.	2.27	2.19	1.82				2.62	2.77
%3>1	77.40	232.30	1070.10				190.70	751.20
4 minus 2	.20	1.13	4.54	6.12	.67	.30	−1.93†	.46
σ diff.	1.16	23.14	5.09				2.49	1.07
D/σ diff.	.17	.05	.89				.78	.43
%4>2	3.00	.80	65.50				−7.20†	22.20

* Score of Group 2 greater than Group 1.
† Score of Group 2 greater than Group 4.

(3) The relative amount of retroactive inhibition in the retention of the older and the younger associations. In Sections *(1)* and *(2)*, immediately preceding, it has been shown that the interpolated problem, Maze II, had a detrimental effect upon the retention of the original problem, Maze I, in both the older and the younger age-groups. The next question concerns the relative amount of retroaction in the retention of the older and the younger associations. Are two groups of associations, which are of equal strength but of unequal age, equally susceptible to retroactive inhibition?

The relative susceptibility of the older and the younger associations to retroactive inhibition is obtained by a comparison of the differences between Groups 1 and 2 (showing retroaction for older associations) with those between Groups 3 and 4 (showing retroaction for younger associations). These data are shown in Table III, and indicate the relative effect of the interpolated problem upon the retention of associations of different age. In terms of relearning trials, there was a detrimental effect of 27.7% for older associations, as compared with a detrimental effect of 120.0% for younger associations; on the basis of the time taken in relearning, no measurable retroaction was shown in the case of the older associations, while a detrimental effect of 230.3% occurred in the case of the younger associations; and, in terms of errors in relearning, a detrimental effect of 45.3% and 927.2% was shown for the older and the younger associations respectively. As to errors in recall, there was an increase of 98.0% for the older associations as compared with an increase of 1279.4% for the younger associations; and, as to the amount of time taken in recall, there was a facilitating effect of 3.2% for the older associations as compared with a detrimental effect of 203.3% for the younger associations. In other words, the older age-condition showed decidedly less susceptibility to the detrimental effect of the interpolated problem than did the younger age associations according to all relearning and recall scores. The same result is found when the two age-conditions are compared on the basis of saving scores. The effect of the interpolated problem upon the retention of the older associations was only slightly detrimental; the difference in the percentage saving, between Groups 1 and 2, was 9.64% in trials, 2.43% in time, and 1.27% in errors. The corresponding figures within the younger age-group, showing a decided decrease in the efficiency of retention, are 43.31% in trials, 31.11% in time, and 34.36% in errors. These comparisons indicate clearly that the amount of retroactive inhibition resulting from an interpolated maze problem varies with the age of the associations of the original problem. A decidedly greater amount of retroactive inhibition occurred in the retention of the younger than in the retention of the older associations.

A second means of determining the relative amount of retroactive inhibition in the two age-groups is to compare the raw scores on the final retention test of Group 1 with those of Group 3. These two groups had the same maze problem interpolated immediately preceding the retention test; they differed from each other in that the associative connections for the original problem were established for a much longer period of time (48 days) in Group 1 than in Group 3. Comparative retention data are presented in Table III. The differences between Groups 1 and 3 are pronounced, according to all criteria of retention employed; and the various measurements are consistent in indicating greater efficiency of retention of the older than the younger associations.

(4) The relation of the age of the associations to the efficiency of retention. A comparison of the retention records of Groups 2 and 4 indicates the relation of the age of associations to the efficiency of the retention of Maze I, when no second problem is interpolated during the 20-min. interval. The amount retained under these two conditions is shown in Table II. Comparative retention data are presented in Table III. These data indicate that older associations probably "fade less rapidly" than younger. According to 7 of the 8 criteria employed in measuring retention, the maze solution is more effectively retained in the older age-group than in the younger. Although the facts are rather consistent in indicating this result, the margin of superiority of Group 2, the older age-group, over Group 4 is small, and, statistically, the differences are of low reliability.

DISCUSSION OF RESULTS

If two associations are of the same functional strength but of unequal age, the older association is apparently less susceptible to retroactive inhibition than the younger association. However, this does not mean that the older of two associations of equal strength is more resistant, by virtue of its age, to the detrimental effects of activities learned subsequently. To state that the age of the original habit is greater in one group than in the other, at the time of the interpolated acquisition, is very probably nothing more than a convenient way of calling attention to the fact that the retentive process underlying the habit has been subjected to such things as greater amount of activity and different sequence of events. Whenever the age-factor in such a situation is varied, as in the present instance, other factors are also necessarily varied, such as distribution of effort, amount of practice, and familiarity with the test situation. These factors are probably of great importance in accounting for the relationship that has been found between the age of the associations and susceptibility to retroaction.

In the present experiment, the older age-group learned the original problem at one sitting, and 48 days later practiced the same problem until a definite level of efficiency in performance was reached. Their effort was thus distributed into two different work periods before the introduction of the second problem. On the other hand, the younger age-group learned the original problem at one sitting and was given the second problem to master immediately thereafter. The greater distribution of effort in the older age-group may be definitely related to the decrease in susceptibility to retroaction under this condition, as contrasted with the susceptibility to retroaction in the younger age-group.

A second variable, probably of some importance in this connection, is the amount of practice. The introduction of a second practice period in the older age-condition resulted not only in a greater distribution of the total effort on that problem, but also in a greater amount of practice in the maze for which retention was later to be tested.

Another variable deserving to be mentioned is the amount of the interpolated activity. The amount of interpolated activity is probably an important factor in this connection, for it refers to a very significant characteristic of the activity designed to produce retroaction. Maze II was used as the interpolated problem for both age-groups. However, the younger age-group had a great deal more interpolated activity than the older age-group, as indicated by the following scores in mastering the second maze: the older age-group took 21.63 trials, 632.83 sec., and made 203.63 errors, whereas the younger age-group took 39.20 trials, 1195.10 sec., and made

478.46 errors. Hence, Group 3, the younger age-group, took almost twice as long, required approximately twice as many trials, and made twice as many errors as did Group 1 in learning the interpolated problem. Although the interpolated problem was the same, the amount of interpolated maze activity was not. The possibility that the amount of retroactive inhibition may vary with the amount of interpolated activity has been noted,[5] although this factor has not been the subject of any experimental investigation.

As indicated above, the interpolated maze was mastered in fewer trials, with fewer errors, and in less time, by the older than by the younger age-group. There was greater positive transfer from the older than from the younger associations to the learning of the second maze;[6] and the interpolated problem interfered less in the retention of the older than in the retention of the younger associations. This indication is in agreement with results obtained by Webb from which he concluded that "there is a negative correlation between positive transfer and negative retroaction. Those conditions which produce the maximum amount of positive transfer give the least amount of negative retroaction."[7] Some suggestions of this relationship noted by Webb appeared also in another study in which the interpolated maze was learned more quickly but had less detrimental effect upon the retention of the original maze when the latter was mastered under punishment conditions, as contrasted with normal non-punishment conditions.[8]

SUMMARY AND CONCLUSIONS

The present investigation was conducted to determine the relationship between susceptibility to retroactive inhibition and age of associations when the associations are of equal strength, two age-conditions being employed. In the younger age-condition the original maze, for which retention was to be tested later, was mastered immediately preceding the interval (20 min.) of retention. In the older age-condition, the *S* mastered the original maze and then 48 days later, just prior to the 20-min. interval, mastered the original problem again and thus reached the same level of efficiency in maze performance as that exhibited at this time by the younger age-condition. One-half of the *S*s in each age-group learned a second maze during the 20-min. interval, and the other half did

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[5] M. E. Bunch and F. D. McTeer, The influence of punishment during learning upon retroactive inhibition, *J. Exper. Psychol.*, 15, 1932, 473-495.

[6] The relationship between age of associations and transfer is discussed by Britt, *op. cit.*, 115 f.

[7] L. W. Webb, Transfer of training and retroaction, *Psychol. Monog.*, 24, 1917, (no. 104), 83.

[8] Bunch and McTeer, *op. cit.*, 494.

NEW PHENOMENA OF APPARENT VISUAL MOVEMENT

By SAMUEL W. FERNBERGER, University of Pennsylvania

The large amount of work on the problems of apparent movement since the work of Wertheimer[1] has been adequately summarized by Mibai[2] so that further reference seems unnecessary in this place. In practically all of these experiments, however, single objects were employed as stimuli. One notable exception is the experiment of Wertheimer with falling lines to demonstrate that apparent movement could take place simultaneously in two directions. The present experiment differs from the previous ones by employing double stimuli.

APPARATUS AND METHOD

Stimulus-cards were exposed in a modification of the Dodge tachistoscope so arranged that two illuminated stimulus-cards would appear successively in the same field. Combined with the tachistoscope was a modification of the timing device described by Jenkins[3] which utilizes a condenser-charge apparatus so that either one or the other chamber, at the end of which is placed a stimulus-card, is successively illuminated. The two chambers are at right angles to one another and the images of the two test cards are brought into the same field for the *O* by inserting a half silvered mirror at an angle of 45° in the lines between the eye and the two test cards. The timing device was arranged so that the interval of exposure was 124 sigma. In other words, Test Card 1 was in the field at the beginning of each experimental situation. After a 'Ready,' 'Now' signal, the key was pressed which changed the illumination from Test Card 1 to Test Card 2 for 124 sigma and then, automatically there was a change of illumination from Test Card 2 back to Test Card 1.

Fifty members of the department staff and graduate students in psychology at the University of Pennsylvania acted as *Os*. Each experimental situation was repeated several times until the *O* was prepared to give a complete introspective description of what had occurred. Instructions were given the *Os* as follows.

> You will see two (or one) figures in the visual field. After I give you a 'Ready,' 'Now' signal, there may be some change in these figures in the field. I am primarily interested in having you report any changes in spatial relations. Each stimulus situation will be repeated as often as you wish until you are ready to describe the changes, if any should occur.

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* Accepted for publication February 1, 1934.

[1] M. Wertheimer. Experimentelle Studien über das Sehen von Bewegung, *Zscb. f. Psychol.*, 61, 1912, 161-265.

[2] S. Mibai. An experimental study of apparent movement, *Psychol. Mon.*, 42, 1931, (no. 190), 1-91.

[3] For a description of the apparatus cf. J. G. Jenkins, A simple tachistoscope of many uses, this JOURNAL, 45, 1933, 150; and W. D. Moedér and J. G. Jenkins, A condenser-charge timer, *ibid.*, 45, 1933, 742-744. Although certain changes were made in the form of the apparatus of the present experiment, the general principles are the same as those found in these two papers.

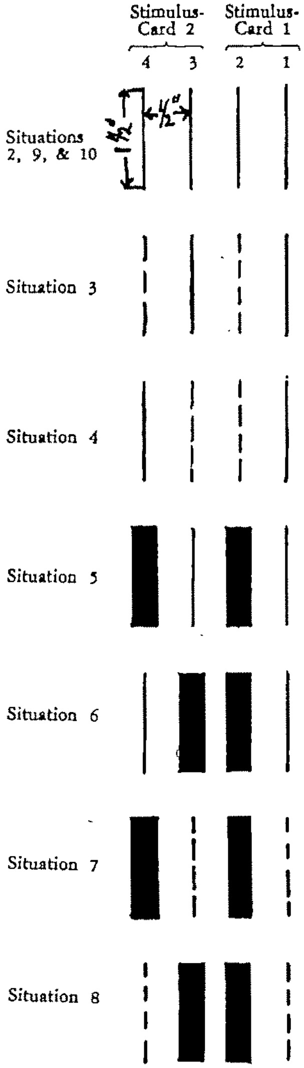

FIG. 1. THE STIMULUS-CARDS
Lines 1 and 2 are on first card and 3 and 4 on the second card. Broken lines represent red. In exposure field, line 2 on Card 1 and line 3 on Card 2 were in identical spatial positions.

It was seldom that *O* required more than four or at most five exposures of any stimulus-situation. No attempt was made to control fixation except in Situations 9 and 10. The stimulus-situations are indicated in Fig. 1. In this figure the spatial relations as they appeared in the visual field are maintained as far as possible. Lines 1 and 2 were on Card 1 and lines 3 and 4 were on Card 2. The two central lines (2 on Card 1 and 3 on Card 2) were in identical spatial position in the exposure field. Situation 1 (not given in Fig. 1) consisted of a single line on each card in the positions of line 2 on Card 1 and line 4 on Card 2. This experience was given so that all *O*s might have the experience of apparent movement of a simple form under laboratory conditions before the beginning of the experiment proper. Situations 9 and 10 utilized the same stimulus situations as was already employed in Situation 2. In Situation 9, *O* was asked, before the signal, to fixate line 1 and in Situation 10, he was asked to fixate line 2.

RESULTS

Situation 1. (Single line on each card.) Every *O* reported apparent movement of the single line to the left (*O*'s left) and back to original position. The extent of this movement was variously estimated as being from ½ in. (the actual distance between the two lines on the field) up to as much as 2 in. The average estimate of the extent of movement was 1.11 in.

Situation 2. (Two similar black lines on each card.) In the case of every *O* these lines were reported as apparently moving in parallel fashion to the left and then apparently moving back to the original position again in parallel fashion. In the present case, the extent of movement was estimated at less than in Situation 1. The estimated range ran from ¼ to 1½ in. and the average for this second situation was only 0.78. Only one *O* estimated that the apparent movement in Situation 2 was greater than in Situation 1; 19 *O*s estimated that the two apparent movements were the same and the other 30 *O*s reported that the apparent movement in Situation 2 was less than that in Situation 1.

Situation 3. (Narrow red and black line on each card; red line to left in both cases.) Invariably apparently parallel movement was reported first to the left and then back to the right to the original position. For 41 *Os* this apparent movement was reported as entirely parallel for both lines. For the other 9 *Os*, the apparent movement was relatively parallel but for these *Os* the red line seemed to move slightly more to the left than the black line.

Situation 4. (Narrow red and black line on each card; on Card 1 black line to right of red and on Card 2 black line to left of red line.) In this situation 42 *Os* report that the red line remains either stationary or relatively stationary and that the black line moves first to the left, apparently jumping the red line and then back to its original position. Five *Os* report an interchange of the lines, each line apparently moving in the opposite direction, crossing one another and then crossing back. Three *Os* report that both lines apparently moved in parallel fashion to the left and changed color and then apparently moved back to the right with another change of color.

An interesting phenomenon was observed in this situation for the first time. In practically all cases in which the red line was apparently stationary or practically stationary and was 'jumped' by the black line, this crossing occurred in the third dimension. For only 6 *Os*, the apparent crossing occurred in the plane of the card; for 24 *Os* the apparent crossing in both directions occurred by the apparently moving line coming out to the forward plane in front of the cord; for 7 *Os* this occurred in the backward plane. Four more *Os* also reported a circular apparent movement—the apparently moving line crossing in one direction in front of the other and, in the other direction, behind the apparently stationary figure.

There is variability of report, in this situation, as to what happens to the central (red) line. Sixteen *Os* reported that this line remained absolutely stationary, while 24 *Os* reported that it seemed to move slightly to the left and back again.

Situation 5. (Narrow and broad black line on each card with broad line always to the left.) For every one of the 50 *Os*, apparent parallel movement was reported—at first to the left and then to the right. Seven *Os* report that this movement was not exactly parallel and that either the thick or the thin line apparently moved more than the other.

Situation 6. (Narrow and broad black lines on each card; on Card 1 broad line to left and on Card 2 broad line to right.) In this case 47 *Os* report that the heavy line remained exactly or approximately stationary and that the thin line apparently crossed the heavy line to assume a position to its left and then crossed back again. The other 3 *Os* report an interchange of positions with both lines apparently moving in opposite directions. In this case 24 *Os* report that the apparent 'jump' is in the plane in front of the card in both directions; 12 report the apparent 'jump' in the plane in back of the card; 4 in the plane of the card; and 3 report circular movement. For this situation 21 *Os* report that the thick (center) line remained absolutely stationary and 21 *Os* report that it apparently moved slightly to the left.

Situation 7. (Broad black and narrow red line on each card; broad black line always to left.) In this situation all 50 *Os* unanimously reported apparent parallel movement to the left and back to the right. In only 1 case was a difference in the extent of the movement reported.

Situation 8. (Broad black and narrow red line on each card; on Card 1 narrow red line to right and on Card 2 narrow red line to left.) In this situation, all 50 *Os* unanimously reported that the broad black line remained actually or practically stationary and that the narrow red line apparently crosses it to the left and back again to the right. The following distribution of the planes of apparent movement of the red line were reported: in front plane both ways, 28; in back plane both ways, 12; in plane of card both ways, 4; circular movement, 3. With regard to the broad (center) line, 30 *Os* reported that it remained absolutely stationary and 20 *Os* reported that it also apparently moved very slightly to the left.

Situation 9. (Two narrow black lines on each card. In the first card *O* was instructed to fixate the line to his right.) In this situation, 9 *Os* report that the right hand line apparently jumped across the left hand line and then jumped back again to its original position while the left hand line on Card 1 remained actually or relatively stationary. The other 41 *Os* reported that the two lines apparently moved in parallel fashion first to the left and back again to the right.

Situation 10. (Two narrow black lines on each card. In the first card *O* was instructed to fixate the line to his left.) In this situation 17 *Os* reported that the fixated (left) line remained stationary while the other line apparently 'jumped' across it to the left and back again. The other 33 *Os* reported apparently parallel movements of the two lines.

Discussion

Many effects already noted in the literature were also reported by our *Os*, such as, for example, lines receding into the third dimension, different apparent rates of movement of parts of the same line, the central line apparently remaining stationary but turning on its axis and the like. It seems hardly worth while to record these secondary phenomena in this place. Our primary problem was to determine if we could set up physical situations which would be the basis for different phenomenal reports of apparent visual movement. The results are perfectly plain in this point. Situations 2, 3, 5, and 7 presented physical arrangements which would have been accomplished by parallel movement if actual movement had been present and, in every case, apparent parallel movement was reported. On the other hand, Situations 4, 6, and 8 presented physical arrangements which would have been accomplished by one figure remaining stationary and the other figure crossing it, if actual movement had been present, and in almost every case in Situations 4 and 6 and in every case in Situation 8 this apparent phenomenal crossing was reported. In the other reports, the phenomenal description was of a possible apparent movement to accomplish this physical result although, perhaps, not the most obvious one.

Our three criteria of differentiation of the two figures in Situations 4, 6, and 8 were respectively color only, size and shape only, and color and size and shape. As one goes up this scale of differentiation the reports of apparent phenomenal crossing increase as follows: color only, 42; form only, 47; form and color, 50. It would seem that in these findings we have a most excellent demonstration of the "constancy hypothesis" of *Gestalttheorie* in respect to the relation between physical and phenomenal pattern.[4] The fact that with greater and greater differentiation of the two figures in Situations 4, 6, and 8, the center figure remains more often stationary also lends support to this conclusion. The times the center figure was reported stationary were as follows: color only, 16; form only, 21; color and form, 30. In other words, greater differentiation of the two figures tends more and more to 'anchor' the central figure.

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[4] W. Köhler, Über unbemerkte Empfindungen und Urteilstäuschungen, *Zscb. f. Psychol.*, 66, 1913, 51-80; Gestaltprobleme und Anfänge einer Gestalttheorie, *Jahresber. ü. d. ges. Physiol.*, 1922, 516. H. Nelson, The psychology of *Gestalt,* this

Still another confirmation of the "constancy hypothesis" is to be found in the frequency with which this apparent crossing in Situations 4, 6, and 8 involved apparent movement in the third dimension. Third dimensional apparent movements were reported as follows: color alone, 35; form alone, 39; and color and form, 43. It is also interesting to note in this connection that 27 *Os* report apparent movement in the same tridimensional plane for all three situations. Inasmuch as this is a purely subjective contribution, one might expect that an individual would apparently see all such movement either in the forward or backward plane or the plane of the card in some such consistent fashion.

Having established this relatively constant relation between the physical situation and the phenomenal report, it seemed worth while, in Situation 9 and 10, to determine what effects certain subjective contributions might make toward the differentiation of the two kinds of apparent movement in a situation which might be considered physically ambiguous. For this purpose, we employed the exactly similar pairs of lines for which, as Situation 2 but without prescribed fixation, the *Os* invariably reported apparent parallel movement. When the right hand line (outside line) was fixated, 9 *Os* only reported the 'jump' type of apparent movement. When the left hand (central) line was fixated the number of 'jump' movements increased to 17. It would seem that fixation of the center line has a slight tendency to 'anchor' this line but the effect is not nearly as great as the differentiation in the types of apparent movement produced by the physical situation.

One last point of interest is the difference in the extent of movement in Situations 1 and 2. In Situation 1, with the apparent movement of a single line, the average estimated extent was 1.11 inches as against 0.78 inches for Situation 2 with the pairs of lines. In this latter situation, the second line apparently gave a point of reference in the field for the movement, which was obviously lacking in the apparent movement of the single line. The situation, although not similar, conforms in general to the observation of Rubin of the differences of the movement of a single light on the edge of a large spool when revolved alone or when revolved when a second light is placed in the center.

CONCLUSIONS

(1) By employing two figures on each of two test cards tachistoscopically exposed so that they appeared on the same field, it was possible to arrange conditions to produce two kinds of apparent visual movement.

(2) If the physical conditions favored apparently parallel movement of the two figures, this type of movement was always reported.

(3) If the physical conditions favored one figure remaining stationary and the other figure crossing it, this type of movement was almost always reported.

(4) Apparent movement of this latter type almost always proceeded through a plane either in front of or in back of the stationary figure.

JOURNAL, 36, 1925, 350-353. H. R. DeSilva, An experimental investigation of the determinants of apparent visual movement, *ibid.,* 37, 1926, 469-501.

(5) The greater the differentiation between the two figures, the more frequently is this second type of apparent movement reported.

(6) These findings give strong confirmation of the "constancy hypothesis" of *Gestalttheorie* in respect to the relation between physical and phenomenal pattern.

(7) Fixation of one of the two figures is less effective in establishing one or another of the two types of apparent movement than is the physical pattern.

INCIDENTAL MEMORY FOR PLEASANT, UNPLEASANT, AND INDIFFERENT WORDS

By ARTHUR SILVERMAN and HULSEY CASON, University of Wisconsin

Although a number of investigations have been made of the learning and retention of pleasant and unpleasant activities, relatively few of these studies have been concerned with incidental memory. Previous studies of the learning and retention of mildly pleasant, mildly unpleasant, and indifferent words have been carried out by Tait, Griffitts, Anderson and Bolton, Chaney and Lauer, Thomson, and Cason.[1] In view of the general importance of the subject, it has seemed desirable to carry out a further experiment using a different procedure and a larger number of subjects. In the present paper we are principally concerned with describing the results of this further investigation.[2]

Material. A total of 468 different words were used, which were the same as those used in a previous study. The principal differences between the words were in the pleasant, unpleasant, and indifferent feelings that were attached to them.[3] Each of the 468 words was printed on a ½ x 1½ in. stiff card, and the cards were thoroughly mixed. Each of the words was used once in making up 4 lists of 117 words each; these lists will be referred to hereafter as the *original lists.* They were made approximately equal to each other in the number of nouns, verbs, and adjectives which were included. Each of the original lists of words was typewritten double-spaced in 4 columns on an 8½ x 11 in. sheet of paper. An *alphabetical list* of all of the 468 words was also prepared, typewritten single-spaced on two sheets of paper. The alphabetical list was used with each *S*, but only one of the 4 original lists was used with a single *S*. The 4 original lists were rotated with different *Ss* so that each original list was used approximately the same number of times.

Procedure. Silverman was *E* for all of the *Ss*. The procedure was individual, and the experimental period generally lasted for one hour. The following procedure was carefully followed with each *S*.

(1) After being seated at a table *S* was given one of the original lists of 117

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* Accepted for publication July 1, 1932.

[1] W. D. Tait, The effect of psychophysical attitudes on memory, *J. Abnorm. Psychol.*, 8, 1913, 10-37; C. H. Griffitts, Results of some experiments on affection, distribution of associations, and recall, *J. Exper. Psychol.*, 3, 1920, 447-464; A. C. Anderson and F. J. Bolton, Inhibition of the unpleasant, *J. Abnorm. & Soc. Psychol.*, 20, 1925, 300-302; R. M. Chaney and A. R. Lauer, The influence of affective tone on learning and retention, *J. Educ. Psychol.*, 20, 1929, 287-290; R. H. Thomson, An experimental study of memory as influenced by feeling tone, *J. Exper. Psychol.*, 13, 1930, 462-468; Hulsey Cason, The learning and retention of pleasant and unpleasant activities, *Arch. Psychol.*, 21, 1932, (no. 134), 1-96.

[2] A bibliography on the relation between feeling and memory may be found in Cason, *op. cit.*, and references to the studies on incidental memory are given in S. M. Shellow, Individual differences in incidental memory, *Arch. Psychol.*, 10, 1923, (no. 64), 1-53.

[3] See Cason, *op. cit.*, 39-41, 44-46.

words, and was asked to grade each word P, I, or U[4] according to his personal reactions; that is, on the basis of the immediate associations evoked by the word or on the basis of his feelings towards the word itself. He was instructed to proceed slowly enough to give each word a fair chance, but not so slowly that his grades would not be representative of his ordinary affective reactions to the words. He was requested to spend the same amount of time in grading each word. It was suggested that he give approximately the same number of grades of P, I, and U, but he was urged to grade the words in such a way that his grades would be typical and representative of his own personal reactions. He was assured that grading the words P, I, and U had no relation to intelligence or to any other personality traits.

Twelve of the 73 *S*s spoke the grades aloud as they wrote them so that *E* could obtain a measure of the relative time required to decide upon the grades of P, I, and U. The number of seconds spent in giving grades of P, I, or U for 1356 words was measured, the first word in each of the 4 columns of the original list not being used in this procedure.

(2) *S* was required to go through the original list of 117 words a second time and to examine all of his grades again in order to be more certain that the grades which he had assigned were typical and representative of him. He was told that an average of about 6 grades had been changed by the previous *S*s, but that he could change as many or as few as he wished. A record was obtained of all of the changes that were made in the grades. The revised or final grades were used in calculating the results. Although it is unlikely that strong feelings were present while the words were being graded P, I, and U, the majority of the words were associated with incidents and events which had or had had a definite emotional coloring for *S*. Most of the *S*s enjoyed grading the words.

(3) During a rest period of 5 min., *E* attempted to prevent *S* from thinking about the words and the grades which he had assigned by engaging him in conversation about daily events and ordinary school matters. If *S* talked about the words he had graded *E* tried to change the topic of conversation.

(4) *S* was given a blank sheet of paper and instructed to write as many of the words in the original list as he could recall. He was asked to try to recall and write the words themselves, but not to write the grades of P, I, and U which he had given them. If he inquired about spelling he was told that only correctly spelled words would count. *S* was encouraged to continue trying to recall the words during the whole of the 5-min. period that was allowed for this part of the procedure.

(5) There was a second rest period of 5 min. as in (3) above.

(6) *S* was given the alphabetical list of 468 words, which included the 117 words in his original list, and was instructed to underline all words that had been present in the original list. The general difference between positive and negative recognition was explained to him. If he asked about guessing, he was told that this was permissible, but that he should not underline more than ¼ of the words in the alphabetical list.

(7) At the end of the experimental period, *S* was asked a number of questions about the general methods that he had used and about his insight into the purpose and nature of the experiment.

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[4] P = pleasant; I = indifferent; and U = unpleasant.

Subjects. Thirty-seven of the 73 *Ss* used were men and 36 were women. In the group there were 8 freshmen, 18 sophomores, 23 juniors, 21 seniors, and 3 graduate students.

Results. A count was first made of the number of words in the original list that were graded P, I, and U by each *S*. The average number of words graded P by the 73 *Ss* was 43.4, or 37.1% of the total number of words (117); the average number graded I was 37.8, or 32.3%; and the average number graded U was 35.8, or 30.6%. The median numbers of words were practically the same, being 43 for P, 37 for I, and 36 for U. Considering the method originally used in selecting the 468 words that were used in the present study, the *Ss* should theoretically have given the same number of grades of P, I, and U for these words.[5]

The measure that was obtained of the time used by 12 of the *Ss* in deciding upon the grades of P, I, and U was the number of seconds which elapsed between the speaking of the preceding grade and the speaking of the present grade. The average number of seconds which elapsed while the grades were being decided upon was, for P grades, 3.6 (n = 476); for I grades, 4.1 (n = 476); and for U grades, 3.7 (n = 404).

It will be recalled that after grading all of the words in the original list each *S* reëxamined these grades and changed any that he desired. The total number of changes in grade for individual *Ss* ranged from 1 to 17, with the average at 5.6 and the median at 5.0. All the changes in grade that were made may be represented as follows:

First grading	Second grading			
	P	I	U	Total
P	—	58	29	87
I	122	—	109	231
U	31	60	—	91
Total	153	118	138	409

These figures indicate that 122 grades were changed from I to P, etc. The principal tendency was to change the I grades to P or U.

S graded each of the 117 words in his original list P, I, or U, and the percentage of each of the three kinds of words that were recalled in Procedure 4 was calculated for every *S*. If *S* graded 40 of the 117 words in the original list P and recalled 8 of these P words in Procedure 4, then his percentage of P words recalled was 20. Using these scores for individual *Ss* as the basis of calculation, the average and median percentages recalled for the 73 *Ss* were as follows.

	Percentage recalled	
	Av.	Median
P	21.0	20.8
I	14.2	14.6
U	17.9	17.0

The relative order of rank in the efficiency of recall for the 3 different grades or for the 3 different kinds of words was P-U-I.

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[5] Cf. Cason, *op. cit.*, and Association in relation to feeling and gross bodily movement, this JOURNAL, 46, 1934, 213.

A calculation was likewise made for each *S* of the percentage of each of the three kinds of words that were recognized in Procedure 6. If *S* graded 40 of the 117 words in the original list P and recognized 30 of these P words in Procedure 6, then his percentage of P words recognized was 75. Using these scores for individual *Ss* as the basis of calculation, the average and median percentages recognized for the 73 *Ss* were as follows.

	Percentage recognized	
	Av.	Median
P	71.2	75.4
I	62.6	64.0
U	69.4	70.4

The relative order of rank in the efficiency of recognition for the 3 different grades or for the 3 different kinds of words was also P-U-I, but some of these differences are not very large.

In the procedure that was followed, it will be recalled, *S* first gave a grade of P, I, or U for each of the words in the original list; then he was unexpectedly asked to recall as many of these words as possible, and finally he was given the alphabetical list of 468 words, which included the original list of 117 words, and a measure was obtained of his ability to recognize the words in the original list. Inasmuch as writing and thinking about certain words in the recall procedure had some influence on the ability to recognize the same words later, one further calculation of the results has seemed desirable. In the case of each *S*, a calculation was made of the number of words originally graded P, I, and U which were not recalled in Procedure 4. Then the percentage of these words which were not recalled in Procedure 4 but which were recognized in Procedure 6 was calculated for each *S*. If, for example, 40 of the words in the original list were graded P and 8 of these were recalled in Procedure 4, then 32 words were of course not recalled. If among these 32 words not recalled in Procedure 4, 24 were recognized in Procedure 6, then the percentage of words not recalled but later recognized was 75. Using these individual scores from each of the *Ss* as the basis of calculation, the average and median percentages of words not recalled but later recognized were as follows for the 73 *Ss*.

	Percentage not recalled but later recognized	
	Av.	Median
P	66.1	68.7
I	57.8	59.4
U	64.5	66.6

The same relative order of rank in the efficiency of recognition for the 3 different grades or for the 3 different kinds of words was obtained as before, namely P-U-I.

The retrospective reports of the *Ss* at the end of the experimental period are of some importance in the general interpretation of the results. The majority of the *Ss* assigned grades of P, I, and U for the words in the original list on the basis of associations with recent events; several *Ss* graded the words on the basis of associations with early events, and a few gave their grades on the basis of associations with people. Most of the changes in grade were made because the *S* recalled some event in the second grading which he had not recalled in the first grading. None of the *Ss* expected the test for recall in Procedure 4. Most of them said that while they were trying to recall the words in Procedure 4 a few words would appear, and then several others would be recalled mainly by association with the words that first came to mind. In only a few cases was recall facilitated by visualizing the original list of words, and thinking about the grades which had been previously assigned was a relatively unimportant factor. The test for recognition was unexpected for all of the *Ss* except one. The majority of the *Ss* felt more certain about positive than about negative recognition. During and at the end of Procedures 1 and 2; most of the *Ss* thought that grading the words P, I, and U was the purpose of the experiment. At the end of the experimental period, most of them thought that the experiment was intended to measure their memory for the words.

Although the order of rank for efficiency in the tests for recall, for recognition, and for words not recalled but later recognized was P-U-I, the differences between P and U and between U and I were relatively small. The results seem to support the conclusion, as previously stated by Cason, that "there is little, if any, difference in the efficiency with which P and U activities can be learned and reproduced."[6]

Although an attempt was made to have the *Ss* give the same amount of attention to the P, I, and U words while they were grading them in Procedures 1 and 2, we are not certain that the experimental conditions can be entirely controlled in this respect. In several of the studies on the learning and retention of pleasant and unpleasant activities, the factors of desire, interest, effort, and later learning seem to have produced a relative increase in the efficiency of retaining and reproducing activities that were distinctly P when they originally occurred as compared with activities that were distinctly U when they originally occurred. In the present experiment, however, these factors were controlled to such an extent that the obtained rank order of P-U-I seems to require some other explanation.

The results do not suggest that there was a marked tendency for the P words to be 'stamped in' or for the U words to be 'stamped out.' There was in fact a clearer tendency for both the P and the U words to be recalled and recognized more efficiently than the I words. During the short interval over which retention was measured there was no observable tendency towards the obliviscence of the disagreeable.

Summary and conclusions. Seventy-three *Ss* graded their feelings towards 117 words as pleasant, indifferent, or unpleasant principally on the basis of their personal associations with the words. A few minutes later they were unexpectedly asked to recall as many of these words as possible. The last part of the procedure consisted in obtaining a measure of the *Ss'* recognition memory for the same list of 117 words. During the 5-min. rest periods, which were given between the original grading of the words and the test for recall and between the recall test and the recognition test, *E* attempted to distract *S* by conversation about ordinary events. The principal conclusions which seem justified from the experiment are as follows.

(1) A larger number of the words in the original list were graded P than were graded I, and a larger number were graded I than were graded U.

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[6] *Op. cit., Arch. Psychol.,* 21, 1932, (no. 134), 75-80.

(2) Less time was taken in deciding upon the grades of P or U than in deciding upon the grades of I.

(3) When *S* examined his grades of P, I, and U the second time, and was asked to change any that he thought should be changed, the majority of the changes in grade that were made were from I to P and from I to U.

(4) When the calculations were based on all of the percentage scores of the individual *Ss*, the relative order of efficiency for the 3 different grades was P-U-I for recall, for recognition, and also for words not recalled but later recognized. It should be noted, however, that this consistent rank order for P, U, and I is based on the averages and medians of the combined results of all of the *Ss*, and that the relative ranks of P, I, and U for individual *Ss* are quite irregular.

THE DIAGNOSTIC VALUE OF INTROVERSION-EXTRAVERSION ITEMS *

By Ross Stagner and Joseph Pessin, University of Wisconsin

One of the major problems in personality test construction is to select valid and reliable items. If, for example, one is to construct a test to determine introversion-extraversion, one must select test items which on repetition will yield the same results, which test the alleged traits, and which differentiate adequately those possessing introvert tendencies from those having extravert tendencies. The first personality tests contained items which were put there by *a priori* assumptions of the test makers, or by the opinions of one or more 'expert' judges. Recently attempts have been made to determine empirically the value of test items selected by rational methods or opinions. Among these attempts may be mentioned the studies of Garrett and Schneck[1] and Heidbreder.[2]

The purpose of the present study is to determine objectively the diagnostic value of introvert-extravert items used in current personality tests. We may state our purpose in the form of a question; how well do test items differentiate between extraverts and introverts?

Procedure. One hundred and forty items were selected from the following 5 widely used personality tests: Marston,[3] Laird C 2,[4] Freyd and Heidbreder,[5] Conklin,[6] Neymann and Kohlstedt.[7] Since there were a large number of items available, it was decided to select only the clearly non-duplicate items. The 140 items were combined in the form of a questionary consisting of two parts the first half referred to likes or dislikes,[8] that is, to *personal preferences;* the second half referred to *personal habits.*

The questionary thus constructed was administered to 171 men in the elementary psychology class during the first semester 1931-32. Men only were used, to avoid the problem of sex differences which must necessarily be considered in such a study (although Heidbreder claims that items diagnostic for men are diagnostic also for women).[9] The tests were scored by an *a priori* key made up from

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* Accepted for publication June 20, 1932.

[1] H. E. Garrett and M. R. Schneck, A study of the discriminative value of the Woodworth Personal Data Sheet, *J. Gen. Psychol.,* 1, 1928, 459-471.

[2] E. F. Heidbreder. Measuring extroversion and introversion, *J. Abn. & Soc. Psychol.,* 21, 1926, 120-134.

[3] L. R. Marston, Emotions of young children, *Univ. Iowa Stud. Child Welfare,* 3, 1925, 22-23.

[4] D. A. Laird, Detecting abnormal behavior, *J. Abn. & Soc. Psychol.,* 20, 1925, 138-141.

[5] M. Freyd, Introverts and extroverts, *Psychol. Rev.,* 31, 1924, 74-87. The list proposed by Freyd was made into a test by Heidbreder (*op. cit.*).

[6] E. S. Conklin, Determination of normal extravert-introvert interest differences, *Ped. Sem.,* 34, 1927, 28-37.

[7] C. A. Neymann and K. D. Kohlstedt, New diagnostic test for introversion-extroversion, *J. Abn. & Soc. Psychol.,* 23, 1929, 482-87.

[8] To avoid overweighting the limited range of likes and dislikes tested by Conklin (*op. cit.*), only 15 of his items were employed.

[9] Heidbreder, Introversion and extroversion in men and women, *J. Abn. & Soc. Psychol.,* 22, 1927, 51-61.

the different tests using these items, giving the introverted answer a weight of 1 in each case. Thus in the final computation a high total score was indicative of introversion and a low score of extraversion.

To determine the diagnostic value of each item, the extremes of the distribution were selected as criterion groups. Approximately 20% of the total number of cases was in each criterion group. On the personal preference part of the questionary the extravert group included 32 *Ss*, and the introvert 31. For the second half of the questionary, the personal habits section, 28 individuals were selected as extraverts and 32 as introverts.

TABLE I

ITEMS IN THE PERSONAL HABITS GROUP HAVING GREATEST DIAGNOSTIC VALUE (Arranged in order)

	Critical ratio values	Introvert answer
Are you habitually absentminded?	12.33	yes
Are your feelings hurt by remarks or actions referring to you?	8.09	yes
Do you often worry about possible misfortunes?	7.02	yes
Are you often self-conscious in front of strangers?	5.82	yes
Are you introspective (turn your attention inward to your own thoughts and ideas)?	5.32	yes
Do you often hesitate on making decisions in the ordinary course of the day?	4.85	yes
Are you often considerably upset when unsuccessful in any undertaking?	4.64	yes
Do you usually enjoy the story rather than the way it is written?	4.42	no
Do you usually pay serious attention to rumors?	4.41	yes
Have you often avoided members of the opposite sex?	4.30	yes
Do you often brood over failure in a game or recitation?	4.00	yes
Are you often sentimental?	4.00	yes
Do you usually think a great deal before deciding anything?	3.67	yes
Are you ordinarily extremely careful about the friends you make (must know one very thoroughly to call him a friend)?	3.65	yes
Have you usually made friends with ease?	3.61	no
Do you often indulge in self-pity when your luck is bad?	3.61	yes
Have you usually expressed yourself best in writing?	3.59	yes
Are you usually critical of others?	3.45	yes
Do you often act on suggestions quickly, rather than stopping to think?	3.30	no
Do you indulge much in day-dreams?	3.30	yes
Are you a radical (do you want to change the world instead of adapting yourself to it)?	3.29	yes
Can you remember most of the errands and details of your daily routine?	3.18	no
Do you usually talk spontaneously?	3.08	no
Do you usually succeed at selling?	3.00	no
Do you blush readily?	3.00	yes

The responses of all *Ss* in the criterion groups to each of the 140 items in the questionary were tabulated and reduced to percentage values. For example, 94% of the introverts and 36% of the extraverts answered "yes" to item 2 in the personal habits section ("Do you often worry about possible misfortunes?"). The percentage difference is 58, the σ diff. is 0.081, and the critical ratio (diff./σ diff.), 7.02. This item then is truly diagnostic and differentiates between extraverts and introverts.

TABLE II

ITEMS IN THE INTERESTS GROUP HAVING GREATEST DIAGNOSTIC VALUE

(Arranged in order)

	Critical ratio values	Introvert answer
To spend your vacation at some quiet place rather than at a lively resort	6.84	yes
To compete in intellectual contests rather than athletic games	6.42	yes
To be by yourself a great deal	5.80	yes
To work alone rather than with people	5.54	yes
To have quiet amusements rather than exciting ones	4.95	yes
To write a paper on the characteristics of contemporary novels	4.69	yes
To look at pictures of airplanes	4.57	no
To keep quiet when out in company	4.57	yes
To visit an automobile show	4.51	no
To go to social gatherings just to be with people	4.49	no
To take an active part in all conversations going on around you	4.24	no
To work with many people around you	4.21	no
To examine the details of some new kind of machinery	4.18	no
To rewrite letters before mailing them	3.85	yes
To change often from one type of work to another	3.85	no
To listen to a lecture on the history of painting	3.85	yes
To read about airplanes	3.54	no
To confide in others	3.45	no
To look at a window display of carpenter tools	3.30	no
To stay at home during a social affair	3.29	yes
To talk to yourself	3.27	yes
To limit your acquaintance to a select few	3.13	yes
To listen to a lecture on classical music	3.13	yes
To play baseball	3.08	no
To convince others of your point of view (omit questions of religion)	3.06	no
To have people watch you do things that you do very well	3.02	no
To have excitement	3.02	no

Results. Of the 70 items in the personal habits section 25 had critical ratios of 3.00 or better. These items have the greatest diagnostic value and are represented in Table I with their critical ratio values.

Of the 70 interest questions a total of 27 have critical ratios above 3.00. These are given in Table II with their diagnostic values. We arbitrarily take the distance 3 σ as indicating a reliable differentiation between the two groups.

Of the 54 items arranged by Heidbreder in order of diagnostic value,[10] 44 were duplicated in our personal habits section. It was then possible to compute a rank-difference correlation and determine to what degree the items common to both were of equal diagnostic value. This was found to be $+0.35$, which is equivalent to a Pearson r of $+0.36 \pm .09$. It appears that, although both studies list the same items as having diagnostic value, the order of value is not in very close agreement.

Discussion. The items which were most diagnostic in the section on personal habits, as shown in Table I, are quite definitely behavior-segments relating to *subjective* responses. They correspond to what Jung meant when he referred to the subjective determination of attention in the introvert.[11] This was not, as Conklin apparently interpreted it,[12] a concentration on intellectual as opposed to physical activities (this is probably characteristic of Bernreuter's "self-sufficiency"),[13] but is simply a lowered threshold for subjective as opposed to objective experiences. The introvert is characterized by his attention to his own emotions, motives, and trains of ideas. He is absent-minded because his attention is not focused on the world of external reality.

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[10] Heidbreder, *op. cit., ibid.,* 21, 1926, 120-134; Freyd, *op. cit.*

The evidence, however, would not seem to justify the practical identification of introversion with emotional instability, as Bernreuter apparently does when he presents an introversion test correlating +0.95 with neurotic tendency. While the two may coincide in many cases, they certainly are not the same trait of personality.

The items found to be diagnostic in the interests section, as shown in Table II, bear on a radically different type of individual apparently, as we should expect from the fact that the two halves of the questionary correlate only +0.30. The most diagnostic behavior-segments here are those relating to social seclusiveness and to intellectual as opposed to activity interests. Nine of Conklin's 15 items, which are exclusively of the intellectual-active antithesis, are shown to have diagnostic significance.

Not a single item of the subjective-objective classification originally defined by Jung is included in Table II. This indicates that the two traits, introversion and seclusiveness, which so often have been thrown together by writers on the subject of personality, really are quite separate and distinct units.

In pursuit of our belief that seclusiveness was related to Bernreuter's conception of "self-sufficiency" and not to "introversion," we computed rank correlations for 24 cases with the following results: interests score with Bernreuter introversion score, +0.12, with self-sufficiency, +0.32; habits score with introversion, +0.62, with self-sufficiency, —0.30.

We believe, then, that our habits questions, as summarized in Table I, give the most diagnostic behavior-units for differentiating introverts from extraverts along the lines originally laid down by Jung. The interests questions in Table II measure some characteristic which is related to self-sufficiency, and indeed might represent an overlapping of a test of intellectual-active interests and one of social-seclusive behavior. Further analysis of this point is needed.

Summary. (1) Seventy items, selected as alleged introverted habits, showed 25 with diagnostic values of 3.00 or better, indicating the presence of considerable consistency within this group of behavior characteristics.

(2) Seventy items, selected as alleged introverted interests, contained 27 that were successful in distinguishing extreme groups.

(3) Those habits which were diagnostic follow the definition laid down by Jung for introversion-extraversion.

(4) The diagnostic interests were more closely related to self-sufficiency, and were apparently quite distinct from the trait of introversion.

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[11] C. G. Jung, *Psychological Types,* 1923, 471 ff.

[12] Conklin, *op. cit.*

[13] R. G. Bernreuter, Evaluation of a proposed new method for constructing personality trait tests, Thesis, Stanford University, 1931.

APPARATUS

A CAMERA FOR SIMULTANEOUS RECORD OF HORIZONTAL AND VERTICAL MOVEMENTS OF BOTH EYES

By Brant Clark, University of Southern California

We have modified the method of corneal reflection first described by Dodge and Cline.[1] A 100-c.p. 6-8 volt lamp overloaded to 10 volts is used as a light source. After passing through a collimating lens, the light is reflected to the corneas by means of two small mirrors. Head-rest and biting-board are used, and a spectacle-frame provided with a specially illuminated chromium-plated ball indicates head-movements.

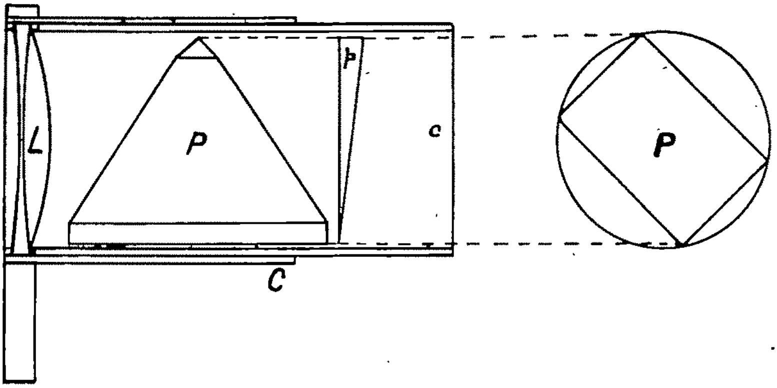

Fig. 1. A Camera for Recording Eye-Movements
The lenticular system is shown for only one eye.

The light-beam from each eye is picked up by a focusing lens (Fig. 1, L), a compound achromatic doublet of 275-mm. focus. Beyond the lens the beam is divided into two parts, one central and one peripheral. The lens is mounted in a brass cylinder (C), 44 mm. in diam., by the use of two tightly fitting brass collars. Within this cylinder a smaller cylinder (c) is fitted just loose enough to rotate. Two prisms are mounted in (c). The first (P) is a total-reflecting prism of optical glass, mounted within a light brass housing which is in contact with the apex and the two non-refracting sides and also hooked under the edges of the total-reflecting surface. The housing slips into two tracks in (c) where it is held in position behind the achromatic lens by a long sheet-brass spring which extends the length of the cylinder. The small cylinder is so rotated that the reflecting surface of the prism forms an angle of 45 degrees with the horizontal axis of the instrument. In the same housing a flat prism (p), 0.8 diopter in power, is placed with its base-apex line horizontal.

The resultant effect of this lens-system is as follows. The rays from the peripheral tated 90 degrees by the total-reflecting prism, i.e. *a vertical movement of the eye will also cause a horizontal displacement on the film.* Thus it is possible to record the horizontal and the vertical movements of both eyes simultaneously on a single record. The two lens-systems (one for each eye) are securely mounted in heavy brass mountings with suitable adjustments for independent focusing. The whole mounting is oxidized and lacquered to a dull black finish.

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[1] R. Dodge and T. S. Cline, The angle velocity of eye movement, *Psychol. Rev.*, 8, 1901, 145-157.

The camera from lens to film is approximately 416 cm. long, giving a magnification of 14 times. The record shows a 1-mm. displacement for each 3-mm. change in fixation at a distance of 30 cm. An episcotister which interrupts the light 25 times a sec., close to its focus, is mounted within, but entirely independent of, the body of the camera.

Sensitized paper[3] backed by light-proof paper is wound on an aluminum drum which takes paper as wide as 6 in. The paper runs through a guide to a lower drum which is turned by a constant-speed motor reduced by a gear-box and variable pulleys. The speed most used gives about four dots per centimeter on the record. The instrument is mounted firmly on three substantial bases heavily bolted to three concrete blocks, 18 x 18 x 30 in., which weigh about 700 pounds each. These blocks reduce vibration to a minimum.

APPARATUS NOTES

DR. LAUER'S 'IMPROVEMENT' OF THE PURSUITMETER

In an article recently published in this JOURNAL, Dr. Lauer states that the pursuitmeter "used by Weiss and Renshaw employed a spring handle stylus. This had some disadvantages and a rigid stylus was tried."[1] This statement is in error, for, as a matter of fact, none other than a rigid stylus has ever been used in any experiment we have made with the instrument. We tried out a number of experimental models, among them the spring handle, but we abandoned them for the rigid stylus as soon as it became clear that the tapering off of stylus-pressure constituted one of the most important things a novice had to learn in order to manipulate the apparatus successfully.

Abundant experience has taught us that no other than an inflexible stylus could be employed if the device is to be used for studies on the acquisition of skill. If the apparatus is used as a *test,* then one must measure either how quickly and how much the operator learns, or how much he has learned prior to the first few trials. There is, therefore, no essential difference between the demand made upon the operator when the instrument is used in a learning experiment or as a test device. Hence Dr. Lauer's proposals to use a spring handle stylus and a ball-point stylus-electrode can only lessen considerably the effectiveness of the instrument.

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[3] Eastman recording-paper No. 697 has been found to be more satisfactory than paper No. 1.

[1] A. R. Lauer, Improvement and standardization of the pursuitmeter technique, this JOURNAL, 45, 1933, 512-515.

The ball-point stylus advocated by Dr. Lauer, was used by us in 1923. It was soon abandoned because a ball point permits a large amount of pronation of the hand, wrist, and forearm. In the original description of the apparatus,[3] we called attention to the fact that the arc of the face of the head of the stylus was turned on a precision bench lathe so as to permit not more than 9° of tilt in any direction without breaking the control circuit and stopping the driving motor. The mechanics of the situation is simple. A ball-pointed stylus permits as much as 180° of pronation or tilt and this completely spoils the apparatus either as a learning device or as a test of how much of this particular manipulation anyone has already learned.

We designed this instrument with the anatomy of the arm and hand in mind. Learning consists largely, as our motion picture analysis shows, of a progressive relaxation of the muscles of the arm and forearm, starting with the pectorals and proceeding centrifugally. (This is precisely the same order in which Coghill observed the individuation of movements to follow in the amblystoma larva.) In the human, virtuosity on the pursuitmeter is gained when the learner's movements shift from the jerky, spastic, phasic type in optical-manual pursuit to the smooth, ballistic form in which vision plays only a minor rôle. How this transition occurs is one of the major problems of the psychology of skill. Muscular tension (stylus pressure) and skill are mutually exclusive. Our original stylus was so constructed that it could be held in apposition with the moving electrode with a small amount of pressure, amounting to not more than the relaxed weight of the hand and flexed arm; breaking the control contact was accomplished by any inadvertence in which the stylus head side-slipped in the plane of its movement, or was brought out of parallel with the target. Such a device at once offers many possibilities to the operator, and many problems for the investigator. I would deny that Dr. Lauer has proposed any improvement, save possibly the replacing of the target with the three-hole escutcheon. Even in this case most investigators would probably prefer the Koerth type of apparatus. I question the usefulness of the apparatus as a *test* for such a hypothetical function as "motor skill," particularly where the test score is the number of times a novice breaks the control circuit in the first several trials. Such scores represent the alternate use of several modes of attack by the learner or testee, several varieties of posture or address, and several types of self-imposed instructions added to those given by the experimenter. In such cases the scores are incomparable, either from *S* to *S*, or when made by the same *S* on different trials, for the reason that their validity is zero, regardless of their consistency or reliability.

Ohio State University SAMUEL RENSHAW

A REJOINDER

Due to the courtesy of Dr. Renshaw I have the opportunity of conjoining a reply to his note. I gladly acknowledge that the statement to which Dr. Renshaw objects should have read, "Weiss and Renshaw adopted a rigid handle stylus after experimenting with various types of flexible and rigid devices." My original statement was based upon information secured while I was working in the psychological laboratory of the Ohio State University. That it was incorrect is due in part to the fact the instrument was variously used during my stay there and also to the fact that Dr. Renshaw mentioned trying out such a stylus. The correction of this error was, however, merely the occasion for a criticism of my article. With many of Dr. Renshaw's points, I find myself in close agreement, which is to say that I regard them as immaterial. With his inference, however, that the rigid stylus is the best type for use in all learning-problems of a sensorimotor nature, and with his argument that the spring device, in studies on the acquisition of skill, defeats the purpose of the apparatus, I find myself in absolute disagreement.

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[3] A. P. Weiss and S. Renshaw, Apparatus for measuring changes in bodily posture, this JOURNAL, 37, 1926, 261-267.

It is possible that Dr. Renshaw's apparatus best suits his problems. On the other hand I have found it unsatisfactory for my studies. When he argues from a different point of view, his remarks are beside the point. When his point of view is similar to mine his remarks are not always germane as he often sets his opinion against my observations. What I attempted to do was to modify the apparatus for a specific use in order to eliminate some obvious sources of error.

Iowa State College Alvhh R. Lauer

NOTES AND DISCUSSIONS

PERCEPTION OF DEPTH IN THE AFTER-IMAGE

In terms of the anatomical view of binocular fusion the images falling on certain parts of the two retinae and these images alone are combined in binocular vision. These areas are called combining or corresponding areas and all other areas non-combining, non-corresponding, or disparate areas. The foveae, for example, are combining areas, also points suitably spaced to the nasal side of the fovea or median plane in one eye and to the temporal side in the other. Some hold to this view so literally as completely to ignore the influence of other important factors in the fusion and the very great versatility of the function. At times the influence of this view leads to rather serious technical blunders and tends in general to act as a handicap in the development of methods of treating the eye.

It is the purpose of this note to caution against a too literal interpretation of the anatomical view and to urge a wider study of the factors contributing to binocular fusion.

One of the requirements of binocular fusion is that the subjective images be referred or projected to exactly the same position or location in space. This includes distance as well as the other two spatial dimensions. In this connection it is scarcely necessary to point out that space reference or projection is very intimately connected with the motor mechanism of the eye. The foveae of the two eyes, for example, combine their images only with convergent lines of regard. Parallel lines of regard would not cause the superimposition of the two projected, subjective images. Space will not be taken here, however, either to review the evidence for the importance of the rôle played by the motor mechanism in space reference or to present results on the factors in binocular fusion. The latter will be made the subject of future papers.

It is our special purpose in this paper to describe conditions under which images on disparate portions of the two retinae are combined. In the customary way of viewing objects at different distances the images of the object fixed or located at the intersection of the lines of regard fall on the foveae of the two eyes and combine. The images of nearer objects which fall to the temporal side and of farther objects which fall to the nasal side of the two foveae do not combine and the objects are seen as doubled. The avoidance of double images is one of the eyes' strongest reflex incentives to movement. The movement resulting from this incentive is convergent for objects nearer and divergent for objects farther than the plane of fixation. These movements not only bring the images to the foveae of the two eyes, which causes their combination, but also place the eyes in that relation to the object which gives the clearest possible perception of image and approximately the clearest possible formation of image. A threefold service is thus rendered in the interests of clearness of seeing; also an important service in the economy of vision in the third dimension. That is, under the influence of these incentives the eyes, unless restrained, automatically converge and diverge and the point of fixation travels from far to near and from near to far. So quickly and effectively is this accomplished that the details of a solid object, even of several objects at different distances seem to be seen at a glance and the change in the adjustment of the eyes to pass unnoticed.

In this, however, there is nothing contradictory to a literal interpretation of the anatomical view of binocular fusion. The convergent and divergent movements of the eyes bring the images to the appropriate combining areas. When we look at a segment of a cone, for example, and direct our fixation to its front, the back doubles. This results in a diverging movement of the eyes to bring the images of the back to the foveae, which causes it to be seen as single and the front to double. The doubling of the front in turn causes a converging movement to recombine the images of the front, and the back doubles; and so the process goes on until the fixation is confined to some particular point by conscious interest or voluntary effort, or is directed elsewhere.

In case we view the after-image of the cone, however, the situation is quite different. The basis of the image here is a physiological excitation which continues after the physical image has been removed. If, in forming the image, fixation is directed to the front of the cone, the images of the front are superimposed on the two foveae but the images of the back are displaced towards the nasal side and thus fall on disparate areas. Now when the physical images are removed and we have remaining only the physiological images, obviously no amount of diverging movement can transfer these images to the foveae. Correspondingly it is commonly said that depth or solidity can not be obtained in the after-image. This conclusion, however, as often happens, is based on a too limited trial of conditions. After-images are usually projected for observation on a surface or background of some kind. Under these conditions depth is not obtained; but if they are projected under conditions which place no restraint or control upon fixation, in other words upon the convergent and divergent movements which have always been associated with the viewing of the object, full and natural depth is obtained. A convenient way of securing these conditions is to observe the after-image in the darkened field of view. When the after-image is projected into the darkened field of view the eyes are free to react to all the incentives for movement produced by the placement of the physiological images or impressions on the retinae and it seems that nothing more than these movements or the incentives to movement, is required to cause both the fusion of the images located on the disparate parts of the two retinae and the natural and full binocular perception of depth.

The result may be made clearer perhaps by a description of the conditions under which the observation is made. A segment of cone, for example a small megaphone, is surfaced in two colors to differentiate between the front and the slant surface. The slant surface may, for example, be covered with red paper and the opening in front with blue paper, with a narrow border of white to give still better definition. When this is viewed from 40 to 60 sec. and the after-image is projected on a sheet of cardboard or other plane surface, the following phenomena occur. When the front of the cone is fixated in securing the stimulation for the after-image, its images are centered on the foveae of the two eyes and the circles which represent the back of the cone are imaged to the nasal side of the two foveae. These circles, it should be noted, are filled by the red of the slant surface. In the after-image the front of the cone is seen as a single image, a yellow disk. The images representing the back of the cone are seen as doubled, *i.e.* as two overlapping disks of green. As is always the case for images which do not superimpose, these disks rival in their tendency to hold the field, *i.e.* sometimes both are present and at other times one and then the other. When the observation is made under the converse conditions of fixation, the green disks representing the back of the cone are combined in the after-image and the yellow disks representing the front of the segment are seen as doubled and as rivalling for the control of the field. When the after-image is projected in the darkened field of view, however, the cone stands out vividly in space in full and natural binocular depth. When the cone is viewed in a strong light, preferably daylight, in order to secure an after-image of good intensity, the result is quite striking. The effect is that of a luminous cone, with an orange front and a blue-green slant surface, hanging in space without visible means of support or suspension. Just as in viewing the object the depth is better seen if the cone is viewed from a point a little above and to one side, so in the after-image the depth will be better seen if in securing the stimulation the cone is viewed from a point above and a little to one side. So far as we have been able to determine, fusion and depth may be obtained in the after-image under any conditions of projection or observation which do not control fixation or place any restraint on the convergent and divergent movements of the eyes. For example, it can be obtained when instead of in the darkened field of view the image is projected towards a dull sky on a suitably dark day. Under these conditions the lines of regard do not become parallel as is often supposed but assume the natural converging position which they take when there is no control of fixation. That is, the conditions with reference to the direction and control of the lines of regard are very much the same as they are in the dark room.

With reference to the above conditions of observation the situation may be summarized as follows: (1) when the appropriate motor factor is present and the images are on combining portions of the two retinae, fusion occurs; (2) fusion also occurs when the appropriate motor factor is present and the images are on disparate parts of the two retinae; but (3) it does not occur when the images are on disparate parts of the retinae and the appropriate motor factor is absent. Under these and other conditions of observation which are entirely correct and proper, a strong case may be made for the importance of the motor factor. In the normal seeing of objects both factors are present and there is nothing to indicate whether or not both are essential to fusion or to differentiate their relative importance as fusion factors. In a later paper the importance of the motor factor will be further shown.

Other ways of demonstrating that images on disparate parts of the two retinae may fuse and will fuse when no control or restraint is placed on the eyes' motor reactions to the placement of their images and further that depth may be obtained in the after-image, may be cited without going very far afield. One of these we have frequently used in connection with those given above in class-room demonstrations. The observer stands at an open window facing a brilliantly illuminated landscape or better still an enclosed space such as a cloister and covers his eyes for 10 min. or more with a black cloth to render them suitably sensitive to light. The cloth is then removed for 1 or 2 sec., the illuminated space viewed, and the eyes re-covered. There soon develops in the darkened field of view a clear reproduction of the scene viewed in full and natural binocular perspective. In this case only the images of the objects in the plane which contains the point of fixation in obtaining the exposure fall on the combining portions of the two retinae. The images of all objects located in planes nearer and farther than the point of fixation fall on disparate areas, yet perfect fusion occurs at all points and full binocular depth is present in the visual impression.

Whatever is to be the ultimate view of binocular fusion, it has at least been shown here that images formed on what are often and perhaps usually considered as disparate parts of the two retinae may fuse. In this connection it should be remembered too that one of the essential conditions of fusion is that the two images be projected exactly to the same position with reference to all three spatial dimensions and that the motor factor both directly and genetically is regarded as intimately connected with all space reference or projection. The evidence which we have thus far been able to compile seems to indicate that binocular fusion is the resultant of not one but several factors and that there is a very considerable interplay of influence among these factors.

Wilmer Ophthalmological Institute
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C. E. FERREE
G. RAND

EYE-MOVEMENTS AND THE PHI-PHENOMENON

Photographic records, taken of the eye-movements during the successive presentation of two lights, indicate that the eyes do not move as often during the experience of phenomenal movement as during the intervals when movement is not perceived.

The eye-movements were recorded by photographing the corneal reflection from the right eye. The photographs were taken with a Cirkut panoramic camera which was adapted so that an entire 12 exposure (2½ × 4¼ in.) film could be unrolled downwards past the exposure-field for each record. The corneal reflection was focused to a small point of light so that it left a narrow line along the length of film; abrupt deviations to the right or left indicated eye-movements in the horizontal plane. The camera was further adapted so that the film was exposed to a second point of light whenever *O* pressed a key signalling the occurrence of an experience of phenomenal movement; the key was held down as long as the movement experience lasted; this left a second line, usually an intermittent one, along the length of the film. Also the film was exposed to a third point of light which coincided with the presentation of the right hand stimulus-light; these flashes left a row of dots along the film. Thus one could read across the film and see the temporal relation between the stimuli, the eye-movement, and the presence or absence of phenomenal movement.

The stimulus consisted of two lights placed in a horizontal relation, 50 cm. apart, and 300 cm. from *O*. Each stimulus consisted of a flashlight bulb exposed to *O* through a circular hole, 0.5 cm. in diam. This was covered with a No. 20 bond paper so that an even brightness without glare was obtained. The lights flashed on in a uniform succession of alternate exposures. The duration of each flash was 50 σ and the interval between the alternate stimuli was 200 σ. A series of 83 alternations, on the average, was recorded on each film. The *O*s, 11 in number, were given preliminary presentations of the stimuli so as to become familiar with the experience of phenomenal movement; at the same time they became adjusted to the position for observation with the head placed in the head-rest and the right eye submitted to the light source of the corneal reflection, a beam from a 100-watt lamp filtered through blue glass. When the *O*s could make the distinction between full movement and end movement they were instructed to press and hold the signal key down only when they perceived full or nearly full movement. The *O*s were given no instruction regarding the direction of their visual regard or their fixation. When the photographic records were taken the camera was set in motion at the moment when *O* first signalled that he was perceiving movement.

Table I shows the average number of eye-movements for every *O* per inch of film, during the periods when movement was reported and also during the periods when movement was not perceived.

TABLE I

SHOWING FOR EVERY O THE AVERAGE NUMBER OF EYE-MOVEMENTS MADE DURING INTERVALS WHEN PHENOMENAL MOVEMENT WAS PERCEIVED, AND WHEN IT WAS NOT PERCEIVED

O		Average no. eye-movements		
		when movement reported		when movement not reported
Br		.40		.77
Go		.45		.46
Gu		.22		.09
Ha		.66		.69
Ho		.27		.30
Hu		.62		1.22
Ki		.19		.84
Sa		.56		.87
Sc		1.03		.82
St		.08		.18
Tr		.75		1.44
	Av.	.47		.70
			Diff.	0.23
			σ diff.	0.14

Table I shows that for 6 *O*s eye-movements occurred more often during the periods when phenomenal movement (*phi*) was not perceived; that, for 2 *O*s, more eye-movements occurred during *phi;* and that, for 3 *O*s, the number of eye-movements was practically the same during the two periods. The difference between the averages for all *O*s is 0.23 (S.D. 0.14) in favor of the movements during which phenomenal movement was not perceived. The eye-movements recorded in Table I varied in magnitude from the angle formed by the distance between the stimuli (nearly 9°) and movements which were just perceptible. (Tremors were not recorded.) Some of the movements appeared to follow the stimulus-lights back and forth, others appeared to jerk or to snap over. There were just as many movements, however, that varied in an irregular manner.

In a second series of experiments the *O*s were instructed to follow the lights with their eyes, *i.e.* to fixate each of the stimulus-lights in turn as it flashed on. Preliminary trials showed that *phi* was not perceived during these voluntary pursuit movements. In further trials when the pursuit movements had been in progress for 15 sec. the *O*s were told, "Now try to perceive *phi!*" The photographic records showed that in all cases the large pursuit movements ceased immediately upon the instruction to perceive *phi,* and only small indefinite eye-movements remained. Also, an interval of time elapsed (varying in length for the different *O*s) before the appearance of phenomenal movement was reported.

In a third series of experiments a comparison was made between two points of fixation during the presentation of the alternate stimuli. Tiny cross-shaped perforations illuminated from behind were used as fixation-points. One fixation-point was located on a horizontal and transverse line midway between the stimulus-lights; the other point was located 200 cm. beyond the stimulus-lights, and was visible to *O* at a point just above the nearer fixation-point. The *O*s were to report the perception of phenomenal movement by pressing a key which actuated one of the pens of a (Foote, fixation-point and in the other cases with the further one, in both [illegible] the alternate fixation-point was presented in place of the one used. In both situations the frequency and duration of phenomenal movement was about equal. From casual comments by the *O*s there had been some indication that phenomenal movement was perceived better if the eyes were fixated beyond the stimuli. But this suggestion was not supported by the records.

The presence of a fixation-point often evoked autokinetic sensations which involved the entire pattern of stimulus-lights and fixation-point. Several *O*s who had perceived *phi* in the previous experimental series now reported (verbally) the autokinetic sensation during a greater portion of their observations. Of 12 new *O*s who had no instruction or experience regarding the *phi*, 6 reported autokinetic sensations predominantly, and the others built up a perception of phenomenal movement.

These experiments, which give results corroborating Guilford and Helson,[1] are offered as a study of the overt oculomotor behavior in relation to the phi-phenomenon. They are not considered to be a test of the presence of incipient eye-movements nor to be an indication of any other type of bodily response which might be related to the experience of *phi*.

Princeton University

WILBUR S. HULIN
DANIEL KATZ

THE REALITY OF BRIGHT AND DULL PRESSURE

Much of the criticism directed against the doctrine of bright and dull pressures seems to center around the feeling that these pressures are esoteric introspective artifacts, producible only under definite laboratory bias and existing only in the imagination of their adherents. It would seem to the present writer that this view may be attributed in part to the tendency to regard these pressures as new sensory elements fitted into Titchener's touch pyramid. However, the reports of the writer's own *O*s indicate that these pressures are not novel elements of experience but simply the "complex sensory resultant of the general bodily adjustment involved in the affective response."[1] The most significant thing about the response of these *O*s was their recognition of a real bodily experience *localizable* in the trunk and in general answering to the description of bright and dull pressure evolved in Nafe's original work.[2] Apparently they believed that this *experience* was of primary importance and that the terminology was a matter of convenience and communication only. As one of the original *O*s has since remarked to the writer, Nafe's description never completely satisfied him, *but* he has been unable to substitute anything better. Titchener himself has frequently mentioned the language difficulties involved in introspection and cautioned against allowing this difficulty to blind us to the experiential reality lying behind the report.

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[1] J. P. Guilford and H. Helson, Eye-movements and the phi-phenomenon, this JOURNAL, 41, 1929, 595-606, esp. 599.

[1] W. A. Hunt, The meaning of pleasantness and unpleasantness, this JOURNAL, 45, 1933, 347.

[2] J. P. Nafe, An experimental study of the affective qualities, *ibid.*, 35, 1924, 507-544.

As any laboratory experimentation involving the direct influence of the present writer would be open to the charge of "laboratory bias" he settled on the questionary method as a possible means of further work. Although such results can not be considered conclusive, they are, nevertheless, interesting, indicative, and suggestive.

In the introductory psychology class at Dartmouth 109 members who were unfamiliar with the field of affectivity, were given the following questionary. They were not students of the writer, nor did he administer the questionary.

> "It has been said that pleasantness is associated with a 'bright pressure,' a touch quality lying between a bright tactual pressure and tickle. It is a diffuse bodily feeling with expansive qualities, resembling expansiveness of the body, and is located in the chest.
>
> "Unpleasantness has been associated with a 'dull pressure,' a touch quality lying between tactual dull pressure and strain. It is not extended and has qualities of constriction, resembling bodily contraction. It is located in the abdomen, or low down in the chest.
>
> "Consider the above statements in the light of your own experience and check your opinion of them on the following scale:
>
> "True Probable Improbable Not True
>
> "Remarks (add any remarks you may care to)"

The questionaries are tabulated, as collected in four groups to demonstrate the uniformity of the sampling. The remarks gave every indication that the questionary was taken seriously and with consideration.

The results show 26 judgments of *true,* 60 of *probable,* 16 of *improbable,* and 7 of *not true.* This would seem to indicate that the doctrine strikes a responsive chord in the majority of the group. It is interesting to note that the remarks on the "improbable" and "not true" questionaries deal with points of difficulty previously noted in the literature. Objections were raised on the grounds that P and U were intellectual and not sensory, that the pressures were only noticed in extreme cases of P and U, and that they were not definitely localizable. A few cases were found where the dull pressures were admitted, but the bright pressures were either denied or called hardly noticeable.

As Young, opposed as he is to Nafe's claims, has generously suggested, "It would be unfortunate if the present discussion obscured genuine problems."[3] The writer feels that genuine problems are being obscured; that in the quarrel over particular terminology and systematic interpretation we are losing sight of an experiential reality, that behind Nafe's particular description lies an important experience that offers a vital approach to the physiology of affectivity, and that Nafe's investigation may well be the beginning of work which will ultimately bring the quest for bodily correlates of pleasantness and unpleasantness to a successful conclusion. With this in mind he tried a new questionary, similar in form, but containing a simplified, common-sense statement as follows:

> "It has been said that pleasantness is associated with a light expansive feeling in the chest, and unpleasantness with a heavy, contracted feeling in the 'pit of the stomach.' "

This was given to further group of 62 introductory students at Dartmouth, 94 introductory students (during their first week's contact with psychology) at Connecticut College for Women, and 54 adults outside the laboratory. This last sample was supposed to represent the mythical man in the street and ranged from two negro elevator boys to a professor of English. The distribution within each group was quite comparable so the results may be pooled.

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[3] P. T. Young, The relation of bright and dull pressure to affectivity, this JOURNAL, 44, 1932, 784.

The results showed 89 judgments of *true*, 91 of *probable*, 10 of *improbable* and 20 of *not true*. Here again the doctrine seems to strike a responsive chord in naïve experience. One can only ask: why?

Connecticut College for Women WILLIAM A. HUNT

A CASE OF ENVIRONMENTAL CHANGE WITH AN ACCOMPANYING LOSS OF LANGUAGE

Cases illustrating rapid forgetting with major environmental change, such as that reported by Carr,[1] are not frequently found in the psychological literature. The following case seems, therefore, worthy of notice.

As elicited in a personal interview with the mother on September 27, 1933 the main facts are these: Katherine, as we will call her, was born in Fayetteville, June 2, 1928. Her parents are Dutch, natives of Holland. Katherine learned and spoke excellent English (a fact substantiated by several witnesses), never used the Dutch, although her mother thought she could understand it. On April 10, 1933, Katherine and her mother arrived in Holland. An incident which occurred five days later will show how dependent Katherine was upon the English on her arrival. On the fifth day of her stay her grandfather took her for a short walk. In about an hour he brought her home. Katherine was crying and her grandfather was distracted. The child had asked to have her shoes and stockings removed (in English) and grandfather could not understand her. By the tenth day of her stay Katherine was speaking a South-African dialect, a Dutch-English mixture. By the fourteenth day, the mother says, "Katherine spoke more and better Dutch than I do English now." (The mother has been in Fayetteville six years.) In three weeks time after her arrival Katherine started to nursery school and suffered no language difficulty in her work or associations. She spoke Dutch exclusively and asked her mother "not to talk that funny way (*i.e.* in English) any more." On July 29, 1933, Katherine's father arrived in Holland and greeted her in English. Since Katherine did not seem to understand him he was compelled to use Dutch. Other incidents also emphasize the extent to which she had lost her command of English: stories and nursery rhymes learned in English were forgotten or at least could not be repeated in English. So thoroughly had she learned the Dutch that on a visit to Rotterdam she could speak the pure Dutch instead of the dialect used in her grandmother's home in Zierikzee.

The family left Holland on September 9, 1933. On the return trip a stewardess who had known and talked with Katherine on her way over in April attempted to talk to Katherine again in English. Katherine did not understand the stewardess until she spoke Dutch. The family arrived in Fayetteville on September 22. A day or so later Katherine went to play with one of her former chums. Katherine would become angry with him because he could not understand Dutch and he with her because she no longer spoke English. Her mother told the writer that on two occasions, September 24 and 26, Katherine had asked her parents to tell one of her playmates in English what she was trying to tell him in Dutch. During the writer's interview Katherine did not speak to him, partly on account of shyness no doubt, but also because she had not then relearned the English. She did, however, speak frequently with her mother in Dutch.

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[1] H. A. Carr, *Psychology*, 1925, 251-252.

The child was seen almost daily from September 27 to October 3 by an assistant,[2] who also visited other friends of the family who knew the child and corroborated all the above items. They also informed us that upon her arrival in Fayetteville she did not know the English words for eyes, nose, or mouth although she readily gave the Dutch words. Katherine rapidly reacquired the use of English. In doing so she first used monosyllables only, incomplete sentences, etc., just as any child does who is first learning to speak. By October 3, 1933, she was playing with her chum and was speaking English almost entirely and apparently understood everything said to her in English.

Briefly, then, we have here the case of a five-year-old girl who lost the use of English after two weeks time spent in Holland (where she heard no English whatsoever) and who learned Dutch during that time. In about the same period of time spent in an English-speaking environment she has reacquired English. She still retains the Dutch since her parents are now using that in the home in the hope that she may not lose it. On October 31 her Binet mental age was five years, eight months.

One or two items give evidence of the fact that not all knowledge of English had vanished in Holland. In first speaking the South-African dialect she built her sentences according to the syntactical rules of English though this was not true when she later spoke Dutch. She used an English accent on her Dutch words. (Here it might be recorded that on October 31, at the time of her test, she used some Dutch accent which was not true of her English before she went to Holland.) Again she might start off with the first few words of familiar nursery rhymes in English, would be seemingly unable to proceed in English, but would continue with a sort of Dutch translation of the poems.

It should be recorded also that the child made a brief visit to Holland when she was two years of age and at that time started 'baby talk' in Dutch which she dropped as soon as she left. Up until this time her parents had talked Dutch almost entirely, Katherine's mother was just learning English at the time, but since then very little Dutch has been used in the home. It is these last items that make Katherine's case not as clear as might be wished from the standpoint of total loss of English and acquisition of Dutch *de novo* but the fact of major loss is nevertheless clearly shown.

University of Arkansas R. H. WATERS

THE SAVING SCORE AS A MEASURE OF RETENTION

The saving score originated by Ebbinghaus[1] continues to be used as a device for expressing results in retention experiments. The score is usually given as a percentage representing the ratio of time or repetitions saved in relearning to the time or repetitions required for original learning.

An ambiguity arising in the use of the saving score has puzzled many of the experimenters using this measure, but there is to my knowledge no clear discussion in the literature of the alternatives involved in calculating the score. The problem both in learning and in relearning; (2) the test trial may be omitted both in learning and relearning; and (3) the test trial may be included in learning, omitted in relearning.

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[2] Miss Rachael Jones assisted in this part of the investigation.

[1] H. Ebbinghaus, *Über das Gedächtnis,* 1885. (Trans. by H. A. Ruger and C. E. Bussenius, 1913, 1-123.)

The results obtained by using each of these three methods may be illustrated in a simple table. The exact values are unimportant. Suppose that in learning by the anticipation method to a criterion of one perfect repetition there are required six trials, including the final criterial trial (perfect anticipation). In a retest (A) the first trial yields a perfect recitation (maximum possible saving); in another retest instance (B) relearning requires six trials (there is no saving). Under the three types of scoring the savings given in Table I result.

TABLE I

SAVING SCORES FROM AN EXAMPLE OF MAXIMUM SAVING AND OF ZERO SAVING AS CALCULATED BY THREE METHODS

	(A) Maximum saving			(B) Zero saving		
	Trials required		Saving score	Trials required		Saving score
Test trials	Learning	Relearning		Learning	Relearning	
1. Included	6	1	83%	6	6	0%
2. Excluded	5	0	100%	5	5	0%
3. Included in learning, excluded in relearning	6	0	100%	6	5	17%

It is evident from the table that the identical experimental data may yield a variety of scores for maximum and minimum saving. The only score which gives a maximum of 100% and a value of 0% for no saving is the second, that which *excludes* the test or criterial trials. Either alternative (1) or (3) will give results at one extreme which depend on the number of trials required for original learning. Uncertain maxima or minima are undesirable aspects of any scoring scale. The differences between the several scoring devices are accentuated when the learning trials are few in number. For more difficult materials, requiring a larger number of trials, the scores are less affected. The second score, the one used by Ebbinghaus, would seem to be the one which ought to be recommended for general use as the standard for saving scores. Finkenbinder,[3] who introduced the anticipation method, apparently used the first procedure suggested above, not correcting for criterial trials. Luh,[4] whose methodological study has served as the basis for much later experimentation, is explicit in adopting the Ebbinghaus procedure.

The matter could be permitted to rest here were it not that there is learning during the test trial in the original practice, and subtracting this trial seems to do violence to experimental fact in order to conform to a scoring device. McGeoch states the situation thus:

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[2] In the anticipation method the test trial is the final criterial trial, during which there is perfect anticipation of the items which are presented.

[3] E. O. Finkenbinder, The curve of forgetting, this JOURNAL, 24, 1913, 8-32.

[4] C. W. Luh, The conditions of retention, *Psychol. Monog.*, 31, 1922, (no. 142), 1-87.

> "It is obviously necessary to take into account all antecedent practice, whether criterial or not, but it is not necessary to include the criterial trials of the particular relearning because the material is assumed to have been relearned by the time the criterion can be met and the criterial trials may be thought of as tests of relearning."[5]

McGeoch is here favoring the third type of score, which Table I shows to yield a possible 100% maximum saving, but an uncertain minimum score, depending upon the trials required for original learning. He is aware of the difficulties involved in the use of any one of the scores, and has indicated that scores calculated by alternative methods yielded results similar to those he reported.[6]

The procedure of subtracting test trials, with its definite 0% and 100% possible saving, should always be used, except under circumstances in which the experimenter, following McGeoch's practice, makes explicit his reasons for using another scoring device.

Stanford University — ERNEST R. HILGARD

THE THEORY OF FORGETTING

The experiments of Jenkins and Dallenbach on "Obliviscence during Sleep and Waking"[1] have indicated, in the words of these authors, that "forgetting is not so much a matter of decay of old impressions and associations as it is a matter of interference, inhibition or obliteration of the old by the new." The great theoretical significance of these experiments has now received widespread recognition. The present writer wishes to call attention to certain evidence which furnishes a very impressive corroboration of Jenkins and Dallenbach's thesis, and at the same time permits it to be given a somewhat more specific form.

Störring[2] has recently described a unique psychopathological phenomenon which is of capital interest from the standpoint of general psychology—a complete and isolated defect of immediate memory. As a result of gas poisoning, which presumably caused some organic change in the brain tissue, the patient studied by Störring has entirely lost the ability to retain any fresh impressions. He manifests no abnormalities of behavior which cannot be referred to this single specific defect. He is able to answer concise questions intelligently, but cannot carry on a protracted conversation. By the time he has finished speaking a sentence of any considerable length, he has forgotten what he was talking about. All persons whom he has met since the time of the accident remain strangers to him; and all events which have happened since then (even such important events as his own marriage) have been completely forgotten. He knows nothing about his memory-defect or about the accident which caused it.

On the other hand, he has an excellent recollection of events previous to the accident, and his behavior in every respect shows that the experiences acquired under normal conditions have not been lost. Störring's observations were made nearly five years after the time of the accident. The patient was still able to recall the events of the day preceding the accident, in much the same detail that a normal person would recall the events of yesterday. If asked, "What day is this?" he would invariably name the date of the accident. Events which happened in the few weeks preceding the accident were also accurately remembered.

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[5] J. A. McGeoch, The comparative retention values of a maze habit, of nonsense syllables, and of rational learning, *J. Exper. Psychol.*, 15, 1932, 676.

[6] *Ibid.*, 669.

[1] J. G. Jenkins and K. M. Dallenbach, this JOURNAL, 35, 1924, 605.

[2] G. E. Störring, Über den ersten reinen Fall eines Menschen mit völligem, isoliertem Verlust der Merkfähigkeit, *Arch. f. d. ges. Psychol.*, 81, 1931, 257.

Although Störring is apparently unaware of the work of Jenkins and Dallenbach, he arrives at a very similar conclusion: "It is not time in itself, but the impressions occurring in time, which normally contribute to the forgetting of old impressions."[3]

Störring goes on to suggest that, in the case of the normal person, each recall of an earlier experience leaves a new trace of its own, and that these new traces somehow interact with the old trace and tend to blur it away. However, the well-known facts of retroactive inhibition indicate that no such specialized hypothesis is required; almost any sort of mental activity, it would seem, has a tendency to destroy the memory of earlier experiences. Jenkins and Dallenbach's results certainly point very conclusively in that direction.

Störring's case, however, throws some new light on the question of the *temporal locus* of retroactive inhibition. The indication is that normal forgetting depends not on the mere occurrence of new activities as such, but rather on the occurrence of new activities which themselves leave *after-effects* or traces. Forgetting (in other words, retroactive inhibition) is not due to an immediate influence of a present activity upon a memory-trace, but to a later interaction between one trace and another.

After an interval of sleep, as Jenkins and Dallenbach showed and as has been verified by Van Ormer,[4] there is less forgetting than after an equal interval of waking life. It is possible that the low retroactive effect associated with a period of sleep depends upon the joint presence of two factors: a low level of activity, and an instability of the *new traces* produced by such activity. Immediate memory is poor in the hypnagogic state, and it is notorious that dreams are easily forgotten. Activities occurring during sleep which left no new traces behind would presumably have no retroactive influence.

University of Kansas D. M. PURDY

Howard Crosby Warren: 1867-1934

Dr. Howard C. Warren, Stuart Professor of Psychology at Princeton University and head of the Department of Psychology until 1931, died after a few days' illness in New York, January 4, 1934. He was one of that group of American pioneers in experimental psychology which includes Titchener, Cattell, Angell, Sanford, and Witmer. The influence of his frank and generous personality, of his modesty, courage and purposiveness, has been felt by several generations of psychologists, both in America and abroad. He was a graduate of Princeton University and devoted his entire professional life so untiringly to that institution that his name is indelibly associated with Eno Hall and Princeton psychology.

Howard Crosby Warren was born at Montclair, New Jersey, June 12, 1867. An injury at the age of eighteen months necessitated much medical attention in America and Europe and seriously interfered with his preparatory education, which was very irregular. That he was able to enter Princeton in 1885 is proof of that quiet courage and strength of character which were so much in evidence throughout his life. In 1890 he was appointed an instructor in logic and came into close contact with James McCosh. There followed several years of study abroad. On his return to Princeton, he was appointed a demonstrator in psychology under J. Mark Baldwin. His advancement was steady. In 1904 he was made Director of the Psychological Laboratory and in 1914 Stuart Professor of Psychology. The development of psychology at Princeton was greatly retarded by its close relation to philosophy, as the teachers of this latter subject had overwhelming influence in departmental affairs. Through courage and persistence he was able, after many years' struggle, to obtain in 1920 a separate department of psychology, and he was named its first chairman. Due to his efforts and in part to his financial support, a separate building, Eno Hall, was erected for experimental psychology in 1924. He withdrew from the directorship of the Laboratory in this year in order to devote more time to his literary tasks, but he retained his offices in the laboratory building and, as chairman of the Department, continued to give unsparingly of his time and advice regarding the advancement of psychology at Princeton.[1]

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[3] *Op. cit.*, 303.

[4] E. B. van Ormer, Retention after intervals of sleep and of waking, *Arch. Psychol.*, 21, 1932, (no. 137), 1-49.

The development of his thought and the influences underlying it are easy to trace. Due to his inability to take much exercise, he had abundant time for reverie and speculation and he began early to question dogmatic belief. This critical attitude and dislike of the purely conventional became a striking feature of his character. Combined with this attitude was his hatred of false values, sham and cant. The puritanical ideas and mysticism of his early youth gradually gave way to a mechanistic view, which persisted to the end of his life. The Darwinian theory appealed to him early in his career. During his college days he became interested in Spencer, Huxley, Clifford, and Tyndall. He found the psychological teachings of James McCosh interesting—most probably his introspective nature, habit of self-analysis, and emotional control made him particularly receptive to problems of the mind,—but Spencer and the associationists already appealed to him much more than a Scotch Presbyterian interpretation of mental processes. When he went to Europe after graduation, he fortunately came in contact with Ebbinghaus, Stumpf, and Wundt, and he was definitely won over to experimental psychology but not to the theoretical doctrines of Wundt, which evidently appeared too mentalistic to the youthful scholar. The psychophysiological aspects of Münsterberg's action theory made a strong impression on him. He was also induced to accept the concept of the nervous arc through the arguments and controversy about feelings of innervation, which was at the time the battlefield of psychologists. And so at this early stage of his development, his general point of view was formed. Although he was tolerant of the views of others, it is evident from his writings that he never changed. This tenacity of purpose toward the things which seemed to him really worth while was his most outstanding characteristic in matters both intellectual and moral.

In the early years of his academic career he published the results of experiments in memory, rotation, etc., but there are relatively few experimental papers in his bibliography, which consists to a very large extent of theoretical discussions of fundaas he would have liked.

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[1] For a portrait of Professor Warren see frontispiece of this number (facing page 189); for a more complete account of his life, see his autobiography in *A History of Psychology in Autobiography*, I, 1930, 443-469.

As we have seen, he was interested from the outset of his studies in the English school of psychologists and he published a number of papers in this field of psychological theory, but it was not until 1921 that his *History of the Association Psychology* appeared. Shortly before, he published a textbook, *Human Psychology* which, being not quite suited to introductory courses, he developed into his *Elements of Human Psychology,* a book which made a wide appeal largely on account of its semi-behavioristic point of view, its concise statement of essential points, and the absence of extreme views on controversial subjects. He believed that what distinguished the various contemporary schools of psychology was their different interpretations of only partly known facts. He was greatly influenced by the movement which culminated in Watson's radical behaviorism, but he believed that every method available should be used in investigating the phenomena which are broadly included under the name of psychology. He was, therefore, a strong believer in introspection, with a leaning toward behaviorism. He was a dogmatic advocate of determinism. His early flight from mysticism made any theory which verged on vitalism or teleology, such as current purposive psychology, extremely repellent to him. He thoroughly believed in consciousness, but as he himself expressed it "neither awareness nor any of the complex forms of conscious experience, guides or modifies in any way the course of responsive activity. The form and direction of the response is fully determined by neural conditions, except in so far as external forces hamper or promote its completion."[2] His insistence on a neurological explanation of all mental processes follows from his double-aspect theory of the relation of mind and body, which he thoroughly discussed in his presidential address before the American Psychological Association.[3] He also emphasized the importance of response, but he never entirely agreed with the motor theorists any more than with the behaviorists. For him the sequence of events is stimulus—consciousness—response.

He rendered inestimable service to psychology by developing the *Psychological Review* publication.[4] As editor of the *Psychological Review,* he encouraged any honest, serious, and intelligent attempt to solve the important theoretical problems of the science.

Throughout most of his life he was interested in words and definitions. In 1915 he was appointed chairman of the Committee on Terminology of the American Psychological Association. At the time of its discharge in 1924, the grand total of completed definitions had only reached seventy-nine terms. He therefore set out to write a dictionary of psychology. This project occupied most of his attention during the last years of his life. It was practically completed before his death, and will undoubtedly stand as a reminder to future psychologists of his untiring service to his colleagues and to his chosen science.

Princeton University H. S. LANGFELD

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[2] H. C. Warren, Outline of a psychological standpoint, this JOURNAL, 39, 1927, 34.

[3] Warren, The mental and the physical, *Psychol. Rev.,* 21, 1914, 79-100.

[4] For his relations to these journals, see the account by S. W. Fernberger, Howard Crosby Warren: 1867-1934, *Psychol. Bull.,* 31, 1934, 1-4.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

The Psychology of Pleasantness and Unpleasantness. By J. G. BEEBE-CENTER. New York, D. van Nostrand Co., 1932. Pp. viii, 427.

The purpose of the book is "to remedy the ghost-like state of the psychology of pleasantness and unpleasantness by bringing its facts together into a single orderly structure." This purpose is well accomplished through the survey of experimental work upon pleasantness and unpleasantness and the presentation of an organized view of the subject.

Beebe-Center distinguishes clearly between the actual affective experience and the concept of hedonic tone. The concept defines a quantitative variable with positive and negative phases and a range of indifferent values centered about zero. Zero value is not distinct from P and U but an ideal boundary between them. After considering the terms 'affection,' 'feeling,' 'affectivity,' 'feeling tone,' 'affective tone,' the term 'hedonic tone' is adopted as the one best suited to describe the algebraic variable in question. The question whether the P and U *experiences* are quantitative is ruled out as metaphysical.

The second chapter of the book is an exceedingly useful summary of the methods of experimentation in affective psychology. Starting with a classification of methods by Külpe, the discussion follows the traditional distinction between methods of impression and expression. From Fechner's method of choice have developed the three classical methods of impression: (1) order of merit; (2) paired comparison; (3) single stimulus, or single exposure, which includes the scale of values and the 'percentage of pleasantness' methods. The methods are discussed in detail with recent modifications and criticisms.

The reviewer notices the omission of the method of qualitative analysis, *i.e.* the description of qualitative, intensive, and temporal changes of affective experience. The explanation for the omission is doubtless that the concept of hedonic tone disregards the affective experience as such. Nevertheless the term "hedonic tone" is used in many places throughout the book as if referring to an affective experience rather than to a concept. Even the word "expression" implies a dualism between affective experience and bodily process.

In Chapter III Beebe-Center points out that toward the close of the 19th and at the opening of the present century the psychology of Wundt and his students was concerned with the systematic question of mental elements. It is not surprising that early work upon affection was directed toward the problem of mental elements. Typical questions of this period are: 'Is affection an element or an attribute of sensation?' 'What qualitative dimensions of affective experience can be demonstrated?' The polemic of Titchener and others against the Wundtian theory is well known. The experiments upon mixed feeling, clearness, localization, extensity and volume are reviewed. Beebe-Center points out that to-day the term "sensation" is not used to refer to a constitutent of experience but rather to a conceptual entity constructed from observed experiences. The same distinction holds in the discussions bearing upon the nature and existence of an affective element. Mental elements are constructed by psychologists with different systematic backgrounds. Observers with such different backgrounds give varying and to some extent conflicting [illegible] are then interpreted in ways which yield conflicting conclusions. "The views that hedonic tone is an attribute of sensations, of complexes of sensations, of qualitatively unidimensional affective elements, of qualitatively pluridimensional affective elements, are not inconsistent descriptions of experience, but alternative hypotheses constructed from experience. Again the views that affections may and may not give rise to mixed feelings, are and are not clear, localized, or extended, do not constitute pairs of contradictory characterizations of a single entity, but independent characterizations of different entities (p. 106)." Rather than to embark upon the construction of affective elements whose adequacy must remain problematical, Beebe-Center believes it is wiser to regard the problem of hedonic tone in relation to mental elements as if it did not at present exist.

In a postscript three conclusions are stated "whose interpretation is not at all dependent upon controversial systematic issues." It seems to the reviewer, however, that the postscript commits the author to the systematic position upon which the validity of these propositions depends.

Chapter IV, upon hedonic tone in relation to primary external stimuli, contains 102 of the total 427 pages. There is much detailed discussion of the relation of hedonic tone to sensory quality, to the area and form of stimuli, and to the intensity of stimuli; of experiments involving quantitative measurement; of the Weber-Fechner law for hedonic tone; and of temporal characteristics such as duration, temporal form, position in time, and time error, in relation to hedonic tone.

The reviewer is impressed with the very respectable amount of experimental work already accomplished on the stimulus-conditions of hedonic tone; but also the impression is gained that affective psychology is relatively over balanced on the sensory-stimulus side. The reader of this chapter should perhaps be cautioned that a judgment of liking and disliking is equivocal so far as pleasant and unpleasant experience is concerned.

The problems of affective contrast, adaptation, and habituation are taken up in a separate chapter. The general point of view is that hedonic tone depends not only upon primary stimuli but upon secondary external stimuli which may occur simultaneously or successively.

An interesting and important chapter considers hedonic tone in relation to motivation. Our likes and dislikes are determined by cognitive and evaluative dispositions. They are also determined by physical needs such as the need for water, for food including such specific substances as salt, sugar, and acid. The facts indicate that hedonic tone is markedly influenced by physiological needs; the persistence of a need is accompanied by U and its removal by P. The relationship between hedonic tone and need, however, is an indirect one. The link between the two is probably the stimulation of internal sense organs. Psychological motivating factors, such as wishes, will ultimately be ruled out of scientific psychology, Beebe-Center believes, but they are considered. The fulfillment of a wish usually determines P and thwarting determines U, the degree of P or U varying with the strength of the wish and the strength of its resistance.

Changes in hedonic tone undoubtedly take place with development but it is difficult to know how far learning and how far maturation is involved. In the infant emotional reactions alternate without appreciable transition. These changes are largely determined by physiological states such as hunger. In early childhood hedonic tone can be reliably inferred from emotional behavior. With the child of 3-4 yr. old, wishes, usually for trains of activity such as playing with blocks, determine hedonic tone. Developmental changes in relative preference are demonstrated for color (Marsden, Shinn, McDougall, Valentine, Dashiell, Winch, Garth, Michaels, Dorcus); for musical intervals (Valentine, Dashiell); and for food stuffs (Davis). The experiments of H. E. and M. C. Jones, and those of Watson and Rayner, upon conditioned emotional reactions are described in the context of affective and emotional development.

The relation between hedonic tone and muscular and glandular responses is next considered. The responses are reviewed under two main headings: (1) vegetative processes, including those considered in the earlier researches upon the "expression" of feeling, and the galvanic reflex, and (2) locomotor processes. Under the second heading is a valuable discussion of the energy of muscular response in relation to P and U, and a consideration of different types of response, *e.g.* extension-flexion, tension-relaxation, seeking-avoiding.

In Chapter IX different aspects of associative memory are taken up in their relation to P and U. Pertinent experimental studies relative to recall, recognition, forgetting, and learning of P and U materials are reviewed. There is little doubt that learning accompanied by P is generally more effective than learning accompanied by U. Experiments are considered under two headings: (1) memory in relation to hedonic tone *during* learning, and (2) memory in relation to hedonic tone *following* learning. Under the latter heading the author discusses the law of effect. The animal experiments upon reward and punishment do not relate directly to the problem of hedonic tone following learning. The animal work, however, is not incompatible with the view that *as a rule* learning followed by P is more effective than learning followed by U. This view is confirmed directly by the experiments of Tait, Frank, and Ludvigh.

The reviewer gains the impression that mere concomitance of P and U with the difference phases of associative memory has been studied, and that a more dynamic and analytical point of view is needed.

A chapter is given to the neural processes associated with P and U. The work of Bechterew, Goltz, Cannon and Britton, and Bard, indicates that the neural centers for behavior expressive of hedonic tone are in the thalamus or hypothalamus, closely related to the emotional centers. The work of Head and Holmes on patients suffering from thalamic lesions and experiencing affective disturbances is direct evidence of the thalamic hypothesis. Beebe-Center believes that hedonic tone depends directly upon neural processes in the thalamus, even though this contradicts the doctrine that conscious processes depend upon the cerebral cortex alone.

The final chapter is a thorough summary of the theories of hedonic tone. One group of theories stresses the nature of the conscious carrier. Under this heading are the views of Ziehen, Stumpf, Titchener, Warren, Nafe, and Krueger. The mental elements are not found but rather made by the psychologists. Another group of theories stresses the immediate conditions of hedonic tone. Causation is sought in the psychical realm (Herbert, McDougall) or in the physiological realm. The physiological theories are of two types: (1) those which relate hedonic tone to processes specific in nature but not in localization (Lehmann, Marshall and Thorndike, Troland, Herrick); and (2) those which relate hedonic tone to processes in a specific part of the nervous system (Allport, Marston). Allport's view is definite but contrary to fact. The other physiological theories lack verifiable implications however true they may turn out to be in the long run. What is needed is the formulation of limited working hypotheses capable of experimental verification. One such hypothesis is proposed by the author; namely, that hedonic tone depends upon a specific type of sense-organ process in the proprioceptive group which, under sensory instruction, mediates bright and dull pressure, and under hedonic instruction gives rise to relative or absolute hedonic tone. Hedonic tone depends upon the relative "density" of the process.

In general, the book is a thoroughly competent piece of work. One of its strong points is the reliance placed upon experimental method and result, and the relative freedom from dogmatic statement. Beebe-Center has covered a large, complicated, and heterogeneous mass of experimental material and has shown balance and good judgment in evaluating it. The organization of the book is well worked out. For a long time Lehmann's *Hauptgesetze* was the main reference upon affective processes, and Titchener's chapters in *Feeling and Attention* are still classical sources—but much water has run through the psychological mill in recent years. To-day Beebe-Center's book is the best introduction to the psychology of pleasantness and unpleasantness.

University of Illinois PAUL THOMAS YOUNG

Vision and Colour Vision. By R. A. HOUSTOUN. London, Longmans, Green & Co., 1932. Pp. vii, 238, with 102 figures and diagrams.

This compact volume, along with its predecessors, the *Treatise on Light,* and the more popular *Light and Colour,* leaves the laboratory psychologist tremendously in the author's debt. The topics treated range from units, discrimination of intensity, acuity of vision, visibility of the spectrum, laws of color mixing, hue discrimination data, recurrent vision and flicker phenomena, fatigue and after-images, simultaneous contrast and spatial induction, to theories of color vision and color blindness, color vision and normal variation, a detailed examination of color-blind subjects, and the theory of color vision. To each a separate chapter is devoted.

More adequate justice than is usual in German texts is accorded the American and British investigator. Reeves' table of pupillary changes is adopted, Adrian's and Granit's demonstrations of lateral conduction in the retina utilized, Ives', Jones', and Coblentz and Emerson's data on visibility, hue discrimination and the like, made use of. The researches of McDougall and of Hecht are also freely cited, though their conclusions are not invariably conceded.

Original investigations carried on at Glasgow by the author, over a period of twenty years, set the keynote of the discussion. By applying the methods of statistics, and plotting the data of König, Ives, and others in unusual ways (using reciprocals and logarithms), a number of novel positions are arrived at. Weber's law vanishes into thin air, as does also the duplicity theory of Von Kries. The sharp differentiation of fovea and periphery is denied; and visual purple accepted as the sole photochemical substance—a view not out of harmony with Edridge-Green's. The position of the absorption band in the rhodopsin molecule, as determined by the free periods of its constituent electrons, is assumed to vary about a mean, as do the thresholds of the rods and cones; yielding a normal curve with its peak in the yellow-green. When intensities are low, however, light acts selectively on the electrons whose period is nearer that of violet; for these are more easily knocked out of the molecule by resonance, free or forced (Lorentz' theory). Hence the Purkinje shift in twilight vision (94 ff.).

Many concepts and postulates of the psychologist are utilized to advantage in the discussion. The distinction between psychological and physical units—often overlooked—is basic. The book leads off with an exploratory attempt to factor given brightnesses into subjective units. Incidentally, Ives' metre-candle-millimetre is given preference over the more theoretical millilambert and photon advocated by Troland (both larger units). Yellow, one notes with satisfaction, is accepted as a primary, coördinate with red, green and blue; though the author is justly suspicious of the criterion of unanalyzability, still flaunted by followers of Ladd-Franklin, to the discredit of psychology as science. Would it not be better to admit the absurdity, *per se,* of any *psychological* primary?

Especially refreshing is the treatment of color-blindness in this text. The psychologist irked by the perennial spectacle of the physicist's warping of the facts of normal and deficient color vision into trichromatic framework heaves a sigh of relief at the frank admission that the Young-Helmholtz theory is both contrary to commonsense and mathematically untenable (223 ff.). Incidentally, Houstoun is one of the few present-day theorists to take cognizance of Helmholtz' altered position on color-blindness in the second edition of the *Physiologische Optik.*

Picking up a suggestion tossed off somewhat sceptically by Rayleigh, in 1881, to the effect that anomalies of color vision may represent merely the extremes of normal variation, Houstoun (in 1918) set out to test unselected populations. Groups of 100, 527, 604, 1148, were rated for hue discrimination, the Rayleigh equation, insensibility to red light (Ch. XII). The results demonstrate the fallacy of positing protanopes (red-blind), deuteranopes (green-blind), and red and green anomalies as distinct groupings. Color-blind, anomalies, and normal visioned are intermingled within the limits of the normal curve, or among the 'outliers.' The classifications utilized so glibly by Von Kries and Nagel are obvious artifacts, constructed on false postulates. As for the Gaussian curve for so-called 'anomalies,' (the group set off by Rayleigh from the color-blind, marked by insistence on a preponderance of either lithium red or thallium green in a mixture to match sodium yellow, and further exploited in 1890 and 1907-9 by Schuster and by Guttmann), half of its scatter proves to be a matter of observational error.

Unique among physicists today, Houstoun takes into account the minute structure of the retina—as well as the electron and quantum concepts—in his theory of color vision (Ch. XIV). Rods and cones are not differentiated as to color functions; but cross-connections and the segregation of synapses in two distinct reticular layers are utilized to explain the peculiarities of dark adaptation, the photochromatic interval, the 'opponent' pairs (red-green, blue-yellow) of the Hering theory.

One could wish, it is true, that Houstoun had assimilated the psychologist's abstraction of the dimension of saturation from that of brightness (or brilliance) more adequately. Like the majority of physicists, working with transmitted rather than reflected light, he apparently lacks a working concept of gray, *i.e.* a middle neutral point of departure for the saturation scale. In the opinion of the reviewer, the inference based on his final crucial 'contouring' experiment that the vision of the color-blind is *trichromatic,* not dichromatic, is vitiated by confusion of saturation differences with those of hue. The color-blind are as quick to individualize *saturation* as brightness differences of yellow, and the former are not ruled out in the procedure. The use of filters (so-called 'monochromatic'—Wratton-Wainwright) instead of spectroscopic light, and of a daylight source, diminishes further one's confidence in this final test.

These are mere flies in the ointment, however, and detract not at all from the value and brilliancy of the book as a whole. It should be on the shelf of every psychologist who wishes to keep abreast of visual research, in touch with relevant findings in nerve physiology and physics as well. The format of the volume is on the usual high level of excellence of the publisher. There is a brief but useful index, chiefly of names.

Cornell University ELSIE MURRAY

Principles of Mental Development: A Textbook in Educational Psychology. By R. H. WHEELER and F. T. PERKINS. New York, T. Y. Crowell, 1932. Pp. xxvii, 529.

Koffka and Ogden, to whom this book is dedicated, long ago convinced us that *Gestalt* had a message for education, could one but understand it. In the ten years or more since we began to take note of these prophets of a new day, *Gestalt* has developed and expanded. It is with high hopes, therefore, that one approaches a book written in large part by one of the converts in that development. These hopes grew brighter upon casual skimming but dimmed with more careful reading. The authors have missed an opportunity. In this review therefore I shall not attempt to assess the *Gestalt* position or its possible contribution to education. Rather it is my ungrateful task to show how Wheeler and Perkins have failed to make that contribution. This involves a selection of detailed points for critical analysis; an honest effort has been made to select weaknesses which are typical.

But first a general statement as to the nature of the book. It is an attempt to write a textbook of educational psychology in terms of the configurational principles. The usual divisions thus receive a reinterpretation and restatement though the classical outline is fairly apparent. One has the impression that topics receive emphasis rather more in proportion as they yield opportunity to expound the Gestalt position than because they are really contributory to the development of teaching effectiveness. None the less there is an obvious effort to show the practical implications of the theory—on the whole pretty successfully. Now for the criticisms.

In the first place, the book is hastily and carelessly written. For what else are we to make of such circularity of statement (if not of reasoning) as occurs on page 5? "Any [such] point of view . . . will yield fatal results, because the point of view is wrong and leads to incorrect methods." This is, unfortunately, typical of the loose logic and language throughout. Thus: "It has been discovered that behavior follows the laws of dynamics; there is, therefore, no 'original' *vs.* acquired nature for the modes of response indicated by the terms instinct and learning follow the same laws" (p. 122). This, I submit, is a non-sequitur. Clearly it is possible for the two modes of response to follow the laws of dynamics yet be differentiated from each other. In technical terms our authors incorrectly imply the distribution of the predicate.

Formal logic is somewhat less clearly violated, perhaps, in the discussion of learning to walk. "Awkwardness . . . is applied to one movement when it is compared with another, made at a later stage in the learning process. But the earlier and later movements are made under *different* sets of conditions, hence it is illogical to compare them." Yet we find the authors two pages later comparing finer coördinations with grosser and less differentiated ones—which are surely "made under different sets of conditions." I concede that the authors have a point, comparisons must take account of differences in conditions, but the point does not seem to have been clearly thought out and we therefore get a blanket condemnation of a perfectly good concept like "awkwardness," despite the fact that their own use of "skill" implies its opposite. Much of this carelessness of statement goes back, it seems to me, to an all pervasive dogmatism which leads to the too easy solution of problems by purely verbal devices. Again and again, once a phenomenon is "explained" as being a manifestation of a total pattern formed under dynamical laws, the problem is left there—half solved. For example, the correct timing of a skilled movement, the authors contend, cannot be attained by putting together separate movements; they must emerge from a total pattern. But why the correct pattern emerges—which is the critical point—is left unexamined, apparently because of the magical virtues of "pattern as a whole" and "dynamic laws."

Our authors clearly dislike the concept of original nature. Hence they refer to Freeman's investigation and interpretation but ignore Burks'. They quote with understandable chortles Thorndike's experimental refutation of Thorndike's law of repetition of a response. They do not mention Thorndike's strictly parallel experiments showing the ineffectiveness of the repetition of a stimulation; their own theory centers around that.

Sometimes, to be sure, the dogmatism seems as much a matter of style as anything else. Wheeler and Perkins flourish "laws" as making a thing necessary or impossible on virtually every page. They are fond of such expressions as "demonstrates" or "proves" and "disproves" where a sounder phrase would be "is evidence for" or "against." I doubt that Peterson would say, as Wheeler and Perkins do for him, that his well-known experiment "disproves the repetition theory." In neither scientific nor affective logic are theories so easily dispossessed. Perhaps it was some faint realization of this which led them to buttress this position by another resounding dictum which I quote entirely: "Moreover, that learning does not take place when the conditions are satisfied for the repetition theory, is also conclusive evidence against the theory" (p. 350). Again one notes the straining for certainty in that "conclusive." For the rest, no one can quarrel with the dictum, which merely restates one of Mill's canons. Surely, however, the issue is whether the conditions were in fact satisfied. That issue is here evaded by the vigorous assertion of a platitude. The climax of dogmatism is reached with a summary of "facts about the learning process" (p. 275 ff.). Nos. 5 and 12 may be quoted to show the character of these "facts": (5) "Learning is the expression of a potential." (12) "Each step in the learning process is organized under the law of least action."

These are interesting and legitimate speculations. I do not object to their being presented even to elementary students as a statement of the authors' credo. But to dub them facts is to give the student an altogether false impression of the situation.

Enough has been said to indicate, I think, that this is a thoroughly dangerous book to put in the hands of any uncritical reader; and more's the pity, for it has some admirable features. The attitude towards education is enlightened and progressive, though not always closely connected with the psychology. But, at any rate, a number of significant psychological principles are focussed in support of a more enlightened scheme of education. Furthermore, a variety of experimental evidence is skillfully marshalled in support of a theory of learning or at least of an orienting point of view which is interesting and significant.

Credit also must be given the authors for the tremendous task of restatement of familiar facts in the configurational language. It is no slight task to tear so many experimental facts loose from their accustomed interpretations and show, concretely, that they fit in the patterns of thought of the new theory. The task is, moreover, important; a theory is to be judged as much for its ability to assimilate established fact as for encouragement of *ad hoc* experimentation. But to put the matter baldly, the skillfully marshalled data and the significant schemata under which they are ordered would emerge more clearly and would be more impressive if there were less tub-thumping.

Ohio State University HORACE B. ENGLISH

Methoden zur psychologischen Untersuchung der Wirbeltiere. By WERNER FISCHEL. *Methoden zur psychologischen Untersuchung der Wirbelloser Tiere.* By F. J. J. BUYTENDIJK and WERNER FISCHEL. Handbuch der biologischen Arbeitsmethoden. Edited by Emil Abderhalden. Abt. VI. Methoden der experimentellen Psychologie, Teil D. Heft 3. Berlin, Urban & Schwarzenberg, 1932. Pp. 233-338; 339-419.

From the title of the first study one might expect it to be a critical treatment and evaluation of methods in animal psychology, but it is not. Methods are discussed to the same extent that they are in any critical treatment of animal psychology. On the whole the discussion is not critical, and at points where the author is critical it has to do with the interpretation of results rather than method. As a review of animal psychology the study is very well done. The problems are carefully stated and in most cases the studies referred to are accurately presented. No background in animal psychology is required of the reader.

In the introduction Fischel points out some of the difficulties met with in animal experimentation: The more important of which are: (1) the difficulty in knowing when an animal is performing at its maximum capacity; (2) the difficulty, in studies of intelligence, in establishing the relation between experience and insight; and (3) the difficulty, if an animal has higher abilities, in excluding these in order to study the more simple.

The first of these suggests that motivation and attention must be considered, yet studies dealing with these topics are omitted. The second and third points are used to some extent in the evaluation of the experimental results.

The scope of the article includes perception, learning, and insight. Under perception the author reviews typical experiments, with various animals, on the discrimination of brightness, form, color, depth, and the response to common elements; and the response to relationships having to do with relative size, brightness, and quantity. He wisely argues against the interpretation that the response to a relationship is a demonstration of insight. Due to the fact that various methods in discrimination experiments give varying results, a discussion of the dependence of results on the measuring instruments would have been highly desirable.

Under learning, experiments on enclosed and elevated mazes, orientation, delayed reaction, understanding, the breaking up of habits, and memory (in so far as it concerns what might be called *expectation*) are discussed. The treatment of maze learning is largely limited to a comparison of the two types of mazes, the problem of kinaesthesis, and a discussion of the use of the maze for comparing normal and variously treated experimental groups of rats. There is no attempt at an analysis of learning. Numerous experiments on delayed reaction are cited, but no attempt is made to evaluate or to harmonize the varying results obtained from different methods.

Hunter's "sensory thought" is regarded, not as an explanation of delayed reaction, but as the very thing which has to be explained. The function of the goal in learning experiments cannot be merely regarded as that with which a certain act is associated, but rather it must be regarded as an end which the animal desires to arrive at and satisfy a need. Experiments by Tinklepaugh and the author are presented to show that animals respond to specific memories of specific rewards.

Insight is defined as an ability to foresee the consequences of an act. To demonstrate this ability (1) a difficulty must be overcome without trial-and-error; (2) chance solutions must be excluded; and (3) the problem must not be simplified by demanding behavior which is natural for the animal.

These specifications are not based on any particular analysis of insight and appear to be merely an attempt to exclude the personal factor. The author is very critical of the influence of past experience in many of the tests of intelligence. He seems to insist that in order to credit an animal with insight it must do something new, but he does not tell us the qualifications of a really new act. He states that primary solutions have only been achieved by animals at the level of monkeys. Maier's experiments on rats which are not excluded by any of the above specifications are inaccurately presented as experiments on transfer. With this error in statement these experiments naturally fall under secondary rather than primary solutions.

The second monograph is a review of experiments made on invertebrates (excepting bees and ants) with special emphasis given to method. It is principally an analysis of learning in various animal forms.

The most simple phenomenon akin to learning is that of adaptation (Gewöhnung). This phenomenon cannot be a matter of fatigue. The animal either ceases to react or modifies its reaction because of previous stimulation. The apparent learning of Paramecia (capillary tube experiments) is shown to be due to changes in the constitution of the water. The behavior of both Protozoa and Coelenterata are found to depend upon immediate stimulation. The past affects these forms merely by producing adaptation.

Echinodermata, however, are capable of true modification by experience. When star-fish are fastened to a platform with wire they succeed in escaping by random activity. After several trials they concentrate on the successful line of activity of certain arms. Out of their repertory of reactions one pattern finally dominates because of past experience.

Among Annelides the authors find cases of association which are demonstrated by learning of the T-maze. These animals are regarded as forming a habit by repeating previous responses, but the function of memory is limited to the reflex level.

Mollusca show adaptation which transfers from one stimulus situation to another. (This reminds one of the spread of inhibition in conditioning experiments.) There are also experiments with these forms which show true conditioned responses as well as the learning of Y-mazes. These forms, however, are at best only capable of forming position habits and cannot learn to respond to a characteristic difference between stimuli.

Learning in lower forms differs from that in higher forms in that the reward determines the behavior in the latter, whereas the lower forms respond by repeating previous acts. Thus lower forms respond to immediate stimulation plus the associations connected with the stimuli. The future is non-functional in determining behavior.

Crustacea are superior to the previous forms in that when trained to go to one side of a Y-maze they show a reduction in time when trained to go to the opposite side. Because of this ability the authors regard them as the lowest forms in which memory definitely modifies later behavior.

The discussion of Arachnidae and Insecta emphasizes instinctive behavior and its relation to past experience. No new criteria of learning are presented, but it is pointed out that among insects there is learning in discrimination experiments. Instinctive responses are regarded as depending both upon internal and external stimulation. Reflex and chain reflex theories of instinct are criticized in the light of experimental results.

From the analysis of learning presented one may make out several levels of learning. These are (1) adaptation, which is a modification of behavior by experience, but not true association; (2) the elimination of certain responses, but not the formation of new ones; (3) the formation of associations at a reflex level; (4) the formation of a position habit; (5) the formation of responses to certain characteristics in the stimuli; and (6) the learning of responses in which memory functions so as to make the response dependent upon future consequences. The latter is true learning.

These distinctions, it appears to the reviewer, are purely artificial and are seemingly determined by the authors' supposition that lower evolutionary forms must be simpler than higher forms. This supposition may be true, but how adaptation and the elimination of certain responses by past experience differ is most unclear. Nor is it clear in what way the position habit differs from the elimination of all but one of several responses; or why the position habit is not a response to certain sensory (kinaesthetic) differences. If a position habit furnished no sensory differences how would it be possible to set up differential responses? The most unsatisfactory distinction is that between mere association and learning in which the future plays a rôle. A dog is said to go to food in a learning situation because of food, but Mollusca do the same thing by repeating a previous response. The evidence for this distinction is that lower forms require as many trials to learn to reverse a position habit as they do to learn the original habit; whereas higher forms show a saving or transfer.

Despite these objections we must regard this study as stimulating and carefully worked out.

University of Michigan NORMAN R. F. MAIER

Experimental Psychology. By JOHANNES LINDWORSKY. Translated from the fourth German edition by Harry R. DeSilva. Preface by translator. New York, Macmillan Co., 1931. p. xix, 406.

In this book psychology is the science whose subject matter is the world of consciousness. Its method is introspection and its problem is to describe its subject matter, classify its facts, and explain them. Explanation may be in terms of antecedents and consequents within the realm of the psychological when facts are complete or it may be in terms of the physiological origin of psychological events. The difficulties of privacy of conscious content which led to objective psychology are averted by the device that psychology may include within its investigations expressions of conscious states, chiefly through language. By this means one may know the nature of the conscious states of another by highly reliable analogy.

Sensation is attained by abstraction, results when the sense organ is stimulated in the simplest manner possible, and is defined as "a relatively independent, simple, concrete, palpable, objective conscious content." Act is distinguished from content in sensation but the author does not carry the distinction of act and content systematically through his psychology. "Sensation complexes" are produced when a multiplicity of stimuli simultaneously strike a single sense organ or more than one sense organ. Such complexes sorely need investigation and will be shown to be of great importance systematically. Grouped under sensation complexes are, first, tonal fusion because it is the simplest case and then visual and tactual space perception because they possess the attribute of extension denied auditory experience. Image though not attributively distinguishable from sensation is, nevertheless, classed with sensation as a psychical element.

The next psychological construction is the combination of images with sensations and sensation-complexes. This is a field so little touched by experiment that the author, confronted by dearth of facts, gives a few examples of these constructions but can go no further. Synaesthesis is one of the examples and it exhibits a characteristic of all, that completory images constitute part of all perception.

Returning to the elementary we find that cognitive content though not unequivocally granted the status of element commensurate with sensation and image because it is impalpable is, nevertheless, basic and ranks as "a genuine conscious content" which has been shown experimentally to occupy the "mid-point" of consciousness. "Knowledge of relation," "active relating," and "abstraction" are the examples of simplest cognitive achievements. Advancing to the complex once more there is given a group of complex structures, the "combination of the function of relation with sensation and sensation-complexes." Through the creation of psychological structures of this order the restrictions of a psychology of sensory elements are abrogated and the way is open to incorporate *Gestalt* perception and perception of movement and time into the author's system of psychology.

Two more elementary experiences complete the list of the elementary: feeling and volition. Feelings are "real experiences" and may not, in spite of the systematic difficulties they offer, be omitted from psychology. Although the facts of feeling are competently presented, feeling as an entity stands rather aloof from the systematic psychology of the book as a whole. In the matter of volition, experimental evidence justifies the conclusion that there is a characteristic experience of volition. Its peculiar property is that it is "an inner activity" and it must be directed toward a goal and is, consequently, able to reveal itself only through a "mode of behavior."

In this review it is not necessary to present in detail the author's treatment of what is conventionally called memory and association. Aside from his individual terminology the essential unique feature is his emphasis upon the fact that in mental life association is a constellation. One image is never given alone in consciousness but is always associatively connected, through the experiences of life, with a network of other images by associative connections of unequal strength in which, in recall, processes of inhibition and facilitation operate, determining what images will appear in consciousness.

We now pass to the "higher mental achievements of adults": "higher cognitive achievements," "higher feelings," and "the volitional life." Throughout the range of higher cognitive achievements which are complicated structures of psychological units a mode (or modes) of awareness of relation is an indispensable constituent.

These structures are comparison, apprehension of objects, the practical discrimination between perception and image, formation of concepts, and the establishment of certainty. Inferential thinking is the apprehension of known relations. In the forms of productive thinking where voluntary activity is essential there are also present relational elements. Other terms in the psychology of thought need not be mentioned specifically. Memorial experiences are also components of these higher cognitive processes but especial emphasis is laid upon relational constituents. Even in the higher achievements of memory themselves emphasis upon relational activity is maintained. In the absence of adequate facts in the field the author finds himself unable to complete a doctrine of feeling. The measure of a higher feeling is that in its production "the knowledge of a material relation conditions the reproduction of a complex feeling." Volitional life is dependent upon motives. A motive is "everything which is represented to the mind as a value realizable through the voluntary act," and value is anything that is advantageous for the individual concerned." When the resolve to act has been made and, as in the reaction experiment, the stimulus to act is given there results a determining tendency as the effect of the act of volition, which carries the action to completion. Although the author discusses the rôle of the image of the goal at some length it need not be reviewed here. Reflexive, instinctive, and volitional action are called "outer" actions in contrast with "inner volitional action" which is the result of an act of volition and is important for what follows. Not every phenomenon of attention is classifiable as an inner volitional act but the baffling status of attention in psychology tempts the author to make it, in part, such a volitional act. This position is made plausible on account of the fact that movements of the sense organs are mechanisms of attention and that traditionally attention is "a certain voluntary or involuntary behavior of the individual." It follows curiously but naturally that if attention is an inner volitional action then images "move." "Controlled movement" of images is the "guidance of thought processes by a task;" and fantasy, "free movement of images." This free movement reveals talent, is closely related to productive thinking, and requires the largely fortuitous development of a new apprehension of relation in the complex to establish it as a creative act.

The book closes with a treatment of socially influenced mental achievements: language, morals, art, and religion, and with the exceptional mental states: sleep, dreams, and hypnosis.

The critical features, its full presentation of fact, its suggestions for further research, and its highly individual organization combine to make it an important contribution to psychology.

Bethel, Maine H. G. BISHOP

Experimental Psychology. By H. GRUENDER. Milwaukee, Bruce Publishing Co., 1932. Pp. xi, 455.

In spite of the growth of "experimental" psychology beyond the limits of any particular set of interests or tenets of any school, the term suggests to most psychologists facts and theories derived from laboratory sources. Consequently as one approaches a book with this title he is apt to have a more or less definite "mental" set as to what he shall find in it. Even if the book does not contain an abundance of statistics, tables, graphs, and illustrations, as some of the best treatises on the subject do not (viz., Fröbes and Myers), nevertheless one expects the author to stick closely to factual material, to eschew argument as far as possible, and to treat theories and points of view differing from his own with the fairness and tolerance characteristic of scientific objectivity. But the general tone of this book is not that of the experimentalist in several important respects, chiefly in its doctrinate views and its use of innuendo and summary dismissal of facts which do not fit into the author's system. It is not often in a book on experimental psychology that one finds Aristotle's and the schoolmen's views treated with more respect and consideration than the findings of experimentalists; *a priori* definitions made the touchstone for the acceptance and rejectance of facts; theories involving the ego, free will, and "an All-wise creator" accepted while Titchener's theory of meaning is dubbed a "metaphysical darkroom." Yet certain parts of the book display keen critical insight and old material is presented with a vigor and freshness that make interesting reading even for the psychologist familiar with it all. A brief discussion of certain topics will show more clearly why this book impressed the reviewer as it did.

Gruender's definition of psychology as the science of conscious life, concerned with "the facts of internal experience" which are private but have a subject-object character, reminds one of Brentano's psychology but for its insistence upon a thinking subject or ego. While animal behavior strictly cannot be included in psychology if we accept this definition, it is included on the basis of its analogies with human behavior. But it remains only an interesting set of analogies because there is an impassable gulf between human and animal behavior. In fact we are told that "we had better keep the monkey out of the psychological laboratory. He is really out of place there" (p. 128). Köhler's experiments "have done nothing to further the solution of the problem of animal intelligence" but the author's sensitive-impulse theory of instinct does, even though it "supposes the existence of an All-wise creator Who directs all His creatures . . . and safeguards man's unique place in nature as king of the visible creation" (p. 300).

There are several excellent chapters on vision, hearing, thinking, and attention, the latter being full of practical suggestions. But sensations of hunger, thirst, nausea, dizziness, pins and needles, itching, tingling, and "others of a rather obscure nature are better left to the physiologists for detailed discussion," although the reasons for abandoning them so hurriedly are not given. The work of the Würzburg school receives able defence and Titchener's position on this subject is successfully assailed, in the reviewer's opinion. The discussion of the various theories of thinking fills a lacuna in psychological texts which usually neglect this subject entirely.

Throughout the book the author struggles valiantly with sensationalistic psychology and atomistic physiology, without making clear to this reader where the line is to be drawn in our acceptance of these principles. Thus sensations are accepted as the nucleus of mental processes "stripped of all accretions" but a sensationalistic account of all thinking and the will is rejected. Likewise the neural groove theory with its assumption that repetitions strengthen associations is accepted as adequate for sensory phenomena but not for intellectual processes. We are thus in the presence of duality throughout the gamut of psychological processes: perceptions may be analyzed into sensation but the ego resists analysis; some thinking is merely reassociation but recognition requires association plus [*sic*] the ego; some actions are purely automatic, others subject to voluntary control or "free-will."

The author's command of modern movements in psychology may be gleaned from his placement of sensationalism with *Gestalt* psychology: "sensationalism . . . is really one of the many varieties of the theory of *Gestalt;* namely, that variety which admits no units of structure except sensations" (p. 19). In the foreword by the author we are told that "this book is the outgrowth of a series of lectures given to graduate students at the St. Louis University."

Bryn Mawr College HARRY HELSON

Maternal Behavior in the Rat. By BERTOLD P. WIESNER and NORA M. SHEARD. Edinburgh, Oliver & Boyd, 1933. Pp. xi, 245.

This monograph describes and analyzes maternal behavior and its physiological concomitants in the albino rat. In point of view, the authors display a persistent leaning toward dynamic formulations and in so doing place the major emphasis upon endogenous rather than exogenous causes of specific behavior mechanisms. At the same time they do not fail to take account of external stimuli and field situations; it is merely that they elevate the intraorganic factors to new ranks of importance in the total array of causative agencies.

According to their analysis, maternal behavior is comprised of several relatively independent components; namely, parturition, nest-building, retrieving, defense, licking of young, and nursing. All of these components are sufficiently discrete to admit of clear-cut definition, yet some, notably retrieving, serve better than others as indicators of maternal drive in a complex series of investigations.

The major problems studied are as follows: prolongation of maternal behavior beyond the usual times; the incentive value of young rats of different ages; maternal behavior toward the young of alien species; retrieving, by virgins put into a variety of maternal situations; the rôle of parturition as an instigator of maternal responses; and finally the effects of destruction of the mammillae, removal of the ovaries, and injections of endocrine extracts.

Illustrative of their findings are the following facts. Primiparae and multiparae which have ceased to take notice of their young, when the latter are moving freely about the cage, will foster the very young rats of other mothers for a long period of time if the young are made accessible. Two phases of prolonged maternal behavior appear: (1) an initial phase, ending about 6 weeks after parturition, in which almost all mothers will display vigorous maternal behavior toward foster young in the helpless age, their behavior being scarcely distinguishable from that during the first two weeks after parturition; and (2) a terminal phase in which two groups are to be distinguished, in one of which maternal behavior wanes some 2½–6 mo. after parturition as shown by component after component dropping out of the total maternal ensemble, but in the other phase vigorous maternal responses continue for 14 mo. or more. In the early post-partum period subsidence of nesting seems to be less a function of time than of the availability of materials and the age of the young, but time is not unimportant as eventually all nesting ceases even under the most propitious field conditions. Nursing postures of the mother usually disappear with lactation, but sometimes they are exhibited after total cessation of milk secretion. Maternal drive will appear and be maintained in parturient females in the absence of turgor of the mammary glands and even if the latter have been completely excised; likewise, in the absence of the ovaries, post-partum maternal responses appear and continue until the end of the normal suckling period.

By supplying young mice, rabbits, and kittens to parturient females it was found that the incentive value of such alien species for the induction of maternal behavior is greatest when the young are in the helpless stages of immaturity. Helplessness, rather than size or weight, appears to determine the mothers' attitude.

Using retrieving as the principal indicator of maternal drive, the authors found by extensive tests that some virgins, when isolated in the cages employed in the maternity ward, will carry in (retrieve) very young rats; others, not reacting in initial tests, will do so if given young rats as nest-fellows for a few days; still others fail to retrieve in either situation. The latter were especially useful in crucial tests on induction of retrieving through the injection of various gonadotropic extracts, particularly those from the anterior pituitary body or from pregnancy urine. The authors demonstrated that retrieving can be induced by these gonadotropic extracts but that control injections of extracts from other tissues did not induce similar responses. So far, little is known as to the specific component of the extract that induced retrieving, but no doubt further work with simpler fractions of these extracts will clarify this point.

The absence or weakness of other components of maternal behavior when retrieving was induced by the injection experiments left one in a quandary as to whether retrieving, as induced by gonadotropic injections, was essentially the same as retrieving appearing with motherhood. In appearance they are alike, but of course their internal motivation may be quite dissimilar. Future analyses may throw some light upon this important question, for, as the authors state, their present investigations do not purport to solve all problems to which their efforts have been directed.

Throughout the book the authors have displayed a nice balance between exposition, critical evaluation, and correlation of their own findings as well as those of others. This kind of monograph will have high interest value for students and teachers alike in the field of psychobiology.

Stanford University CALVIN P. STONE

Growth and Development of the Child: II. Anatomy and Physiology. By HALLOWELL DAVIS and others. The White House Conference on Child Health and Protection. Report of the Subcommittee on Anatomy and Physiology. New York, Century Co., 1933. Pp. xxi, 629.

A number of sections of this volume deal with topics of interest to physiological and genetic psychologists. Among such significant sections are those which deal with the following topics: the early stages of the embryo, the nature of voluntary musculature, growth of the skeleton development of the face and dentition, the central nervous system, the development of reflex behavior, the development of centers in the spinal cord and brain, the visual and auditory mechanism, and the general bodily functions in childhood, including a rather detailed consideration of the glands of internal secretion. Most of the sections are followed by brief and generally well chosen lists of references. The distinction of the editors, the special committee, and the able list of contributors make this volume a significant brief summary of facts important for all those who are interested in the normal development and the pathology of children.

In this brief review no effort will be made to indicate the many admirable characteristics of the book. Instead, and possibly ungraciously, some indication will be given of certain statements which seem to be so popularized that they present a misleading impression of modern knowledge in some of the fields dealt with in the book which touch upon scientific psychology.

In a consideration of the development of function in the nervous system, for example, much emphasis is given to the theory of myelinization without sufficient treatment of the objections to this theory. The account of the early development of reflex behavior is so brief as to be scarcely adequate to this important subject. It is interesting to note, moreover, that on pages 195 and 196 the work of Minkowski, Bersot, and Bolaffio and Artom on fetal reflexes is briefly reviewed and then on page 213 the surprising statement is made that "Minkowski is the only investigator who has studied the movements and type of reflexes found in young human fetuses." Only five pages are devoted to the nerve "centers." These pages are possibly satisfactory enough in what they do present, but it is unfortunate that in the list of references given, as well as in the treatment presented, Lashley's work is not reviewed, although the earlier work of Franz is considered.

The treatment of the visual mechanism is opened with the pessimistic statement that "an understanding of the visual process requires a knowledge of three factors: (1) the nature of light, a problem in physics; (2) the conversion of light into a nerve impulse at the retina, a puzzle in physiology; (3) the conscious response to this impulse, a perplexity in psychology." In this volume the problems, puzzles, and perplexities of this subject are not made less by the teleological language in which its treatment is couched, as may be shown in the following example: "Nature tries to offset these errors through a small pupillary diaphragm, and through the catenary curvature of the cornea and lens." The consideration of visual depth perception is so open to criticism that it may well be quoted here in full:

> "Light, color, and form perception are possible to either eye singly. Judgment of depth may be crudely accomplished by one eye through accommodation consciousness, through the interpretation of the size of the retinal image in terms of the known size of the object giving the image.
>
> "Spontaneous and accurate visual perception of depth requires the coördinated use of the two eyes affording two images of the object viewed from different angles, and the building up of these two images in the brain into a concept possessing three dimensions, length, breadth, and thickness. The ability of the brain to correlate two such images is commonly called the fusion faculty. The fusion faculty is not present at birth, but is developed as the visual acuity grows sharper, as the ocular muscles become coördinated, and as the brain learns to fuse the images during the first few years of life. The fusion faculty develops imperfectly, or not at all in those individuals in whom there is a lack of one or more of the essential prerequisites."

The treatment of the ear is largely anatomical. The fact that Dr. Hallowell Davis, eminent as an experimental physiologist in the field of audition, is chief editor of the volume makes such statements as are given in regard to auditory function seem quite unexpected. The following quotation will make this clear: "The internal ear, or labyrinth, simply takes up the sensations which are conveyed to it through the air or other media, and pass [passes] these sensations to the brain for elaboration and appreciation."

The treatment of the internal secretions is more complete than most of the other sections in the volume. The final section of the work, "Increments of Growth," is largely based upon the excellent work of Jackson, Paterson, and Scammon.

Evaluation of this volume is difficult. The importance of the book has been pointed out. It must be admitted, however, that it can scarcely be considered as a textbook and still less as a handbook or as a contribution to knowledge. Rather the work must be thought of as a very unevenly valuable summary of some of the main facts which bear upon early human development. For some workers with children it will have a great value. It seems, nevertheless, in many ways to be a duplication of material already available, often in more complete form, for those who have access to a few of the standard works in this field. It is especially to be regretted that some statements that are not fully accurate have been allowed to creep into a volume which will by some be considered naïvely to possess the authorization of the Federal Government.

Brown University LEONARD CARMICHAEL

Psychology Today: Lectures and Study Manual. Edited by WALTER V. BINGHAM. Preface by Levering Tyson. Chicago, Univ. Chicago Press, 1932. Pp. xii, 271, 185.

These radio lectures are divided evenly among general fields of psychological investigations—Psychology Today, Child Development, Our Changing Personalities, Animal Behavior, Psychology of Education, and Psychology and Industry. Five psychologists contribute 15-min. talks in each of these fields. The lectures in the first division are either of an introductory or of a somewhat general nature; those in the other five divisions deal with more specific problems and conquests. There are, in addition to these lectures, which cover 271 pages, 185 pages in the latter part of the book which includes the six *"Listener's Notebook"* materials. These materials likewise cover the six divisions mentioned, but the paging in this section is not continuous through the several divisions, a matter which adds to the difficulties of turning to references by readers who prefer to go to references as they are encountered in the lectures. Every *Listener's Notebook* includes helps in the form of general introductory statements, summaries of lectures, questions, tables, references, pictures of the lecturers, of human and animal subjects, and of apparatus. The lectures are, of course, of a semi-popular sort, but on continuous reading they give the impression of a rather good presentation of the investigations that psychology is carrying on effectively into various fields of human activities, shedding light on processes of work and living which have in the past been much neglected. Many of the lectures are entirely free from the tendencies to moralize (in the form of "should do" this and that) or to make explanations and applications on the basis of inadequate data. They plunge directly into narration of work being done, of problems being investigated experimentally from this and that angle by the trying out of hypothesis after hypothesis until basic relations are revealed. This sort of lecture comes mainly from investigators who play leading rôles in the actual research in their several fields. Usually a fair degree of caution against over-enthusiasm and inaccurate statements is evident. The authors, however, may possibly regret the occurrence of statements like these (italics the reviewer's): "Therefore, at one pause, and while holding still, *they* [the eyes] *recognize* or take in two or three words," etc. (p. 17); "Not only attitudes toward nationalities, but most of the other important attitudes that we cling to intensely, seem to have their *foundations in childhood experience*" (p. 32); "Indeed nature seems to be prolonging human infancy *in order to* make more mental growth possible" (p. 51); "To draw a vertical line is one ability; to draw a horizontal line is another ability. Each ability must await its turn in the order of development" (p. 55); "Children play games involving elements of danger, partly *because of the joy of escape,* and partly *because of this pleasurable agitation*—the tonic result of just a momentary fear" (p. 59); "This [stroking the sensitive skin of the infant's body and of his mouth] *is the basis of all emotional love responses,* love of mother, father—love between the sexes;" "All true [*sic*] mental disorders have a physical cause" (p. 134); "In the great apes, however, such ideational or *thinking behavior* has been demonstrated experimentally again and again" (p. 140); "The animal then becomes dominated by a single impulse, and searches about for the proper object to satisfy the inner craving. *Nature has seen to it* that the drives which are necessary to survival are deeply rooted in the organism and transmitted by heredity" (p. 154 f.). H. M. Johnson (*Psychol. Rev.,* 39, 1932, 293-323) and Willis D. Ellis (this JOURNAL, 44, 1932, 365) have dealt with other unfortunate errors either of fact or of implication and have stressed the importance of adhering strictly to fact in scientific lectures of this kind. Despite such interpretations and over-positive statements as illustrated above this series of thirty lectures represents a valuable experiment toward putting the layman in more close touch with current research work in psychology. While not all research scientists are successful radio lecturers, the twenty-seven psychologists (two of whom gave two lectures each) and the physiologist (W. B. Cannon) constituting the lecturers heard in these series were all well known and include a number of eminent investigators.

J. P.

The Reading Abilities of College Students: An Experimental Study. By ALVIN C. EURICH. Minneapolis, Univ. Minnesota Press, 1931. Pp. xv, 208.

This investigation "sought to evaluate the efficacy of reading, vocabulary and study exercises in improving reading comprehension, rate of reading, specific vocabulary, general vocabulary, retention in reading, English composition, general scholarship, and general mental ability as measured by the Minnesota College Ability Test." A number of interesting tests were revised or invented, including those for reading rate, comprehension and retention, and specialized vocabulary lists. Drills on paragraph reading, vocabulary from readings, and how-to-study exercises were given to paired sections of freshman English composition students for a total of about 6 hr. during one quarter, with one group running a second quarter. Results showed significant improvement on the specific vocabulary taught but in practically nothing else, and they were confirmed by subsidiary experiments.

In the opinion of the reviewer, this experiment brings together systematically a large amount of information on details of reading improvement, and the measurements need not be repeated by others unless they can show reasonable qualitative grounds for expecting different results. The measurement angles of the study seem almost top-heavy, including large numbers of full page tables and appendices and a continual reference to statistical results in the text. The method of teaching was, perhaps necessarily in their group situations, a "shotgun" attack of teaching the same things to all without regard to diagnosis of their individual difficulties. Because this study dealt primarily with groups of average students, in a non-diagnostic way and for only a short time, it is quite possible that the findings of insignificant progress are still compatible with the much greater progress reported by other investigators for poor readers under more intensive individualized instructions. The situation seems a close parallel to the experiments on transfer of training which show small amounts of transfer under the ordinary type of "practice" but very significant transfer when due attention was paid to the qualitative aspects of training.

Eurich's study strongly reflects the present tendency in educational psychology to place the major emphasis upon the accuracy of the statistical techniques employed at the expense of the qualitative analysis of the problem. Its main importance is that of providing measures and results for checking certain current beliefs about the improvability of reading and pointing out the inadequacy of some commonly used procedures. It should thus save the time of future workers in the field by showing what not to do, but a thorough survey of the possibilities of reading improvement would necessarily include a great deal more study of the diagnosis of particular needs and the development of the most appropriate methods of dealing with individual cases.

University of Oregon ROBERT H. SEASHORE

Psychopathic Personalities. By EUGEN KAHN. Tr. from Bumke's Handbuch der Geisteskrankheiten by H. Flanders Dunbar. New Haven, Yale Univ. Press, 1931 Pp. xii, 521.

"The terms 'psychopathic' and 'psychopathy' are used to designate a large group of characteristics or conditions which lie in the broad zone between mental health and mental illness (psychosis)" (footnote, p. 9). Sixteen psychopathic types (the nervous, anxious, sensitive, compulsive, excitable, hyperthymic, depressive, moody, affectively cold, weak willed, impulsive, sexually perverse, hysterical, fantastic, cranks, and eccentric) are described and analyzed in terms of three "levels" of the personality, which are impulse, temperament, and character. "Impulse is an animal, vital urge toward satisfaction of (biological) need" (p. 73). Temperament varies with the fundamental and life mood of the individual, with his excitability and affectivity, and with the tempo, vigor, and form of his expression (p. 172). "Character regulates the interplay between the total personality and the environment" (p. 49), and varies with the attitude of the personality toward the ego and toward the environment and with the interaction of these two attitudes (p. 244). Upon the balance of these three interacting levels depends the degree of concordance or discordance of the personality (p. 52).

The author rejects the pan-sexualism of Freud but accepts as valid the concepts of the unconscious, repression, and the complex. He agrees with Adler in emphasizing the importance of *purpose* or *striving* but criticizes him for the failure to recognize the rôle of *Anlagen,* or genetic predispositions, as causal factors in psychopathic manifestations. Stern's conception of the psychophysical neutrality of the individual is accepted. The treatment of psychopathy is discussed only in the last paragraph of the book, from which the following sentence is quoted: "We are still unable and will perhaps always remain unable to change the constitutional structure of psychopathic personalities, but we can educate many of them to understand themselves, to come to terms with themselves, and to set themselves genuine goals which they may reach in spite of their psychopathic peculiarities."

The volume contains an author index but no subject index. The bibliography of twenty pages contains one French title (Janet) and one English title (a translation of a German work); the remaining titles are of works in German. The writers most frequently referred to, in addition to those already mentioned, are Birnbaum, H. Hoffmann, Kraepelin, Kretschmer, Kronfeld, and Kurt Schneider. The bulk of the material in the volume is evolved by the "arm-chair" method on the basis of the author's clinical experience. The psychologist seeking ready help in his clinical practice is not likely to find it here. If, however, his interest lies in the theory of personality, and if he is not terrified by an abstruse style and a multitude of new technical terms, his labors will be rewarded by a novel insight into the structure of personality.

New Jersey Reformatory, Annandale D. T. GRIFFIN

Contributi del Laboratorio di Psicologia. By A. GALLI, A. GEMELLI and others. Milan, Publ. Catholic Univ. Sacred Heart, 1931. Pp. 572.

The sixth volume in series six (Biological Sciences) of the publications of the Catholic University of the Sacred Heart (Milan) contains 19 items. Spatial limitations preclude more than a brief statement concerning some of the more important.

A. Galli, The perception of forms in peripheral vision. Perimetric method, Galli found, indicates 5 phases in perceptual reaction: (1) vague existence of something in visual field; (2) some form elements; (3) a figure formed of non-uniform parts, but without significance; (4) precise parted figure with meaning; (5) naming the form.

A. Galli, Concerning the perception of apparent motion produced by different sensory stimuli. Apparent movement studied with variation of number and type of stimuli does not, the author concludes, exclusively support *Gestalt* theory.

A. Galli, Contribution to the study of perception of changes in brightness and illumination. Differential threshold for the perception of both brightness and illumination is increased, this study shows, with the increased speed of stimulus presentation.

A. Gemelli, Researches on the diagnosis of motor ability. Gemelli concludes that the curve of apprenticeship is a more helpful index than aptitude tests.

A. Gemelli and G. Pastori, On the reëducation of decerebrate animals. The authors found that decerebrate fowls are capable of reëducation.

The remaining contributions were by A. Galli and A. Zama, Researches on the perception of plane geometric configurations masked in whole or part by other configurations; A. Gemelli, Emotions and sentiments; A. Gemelli, On the nature and genesis of character; A. Galli, Essay on the psychological analysis of the act of faith in Saint Augustine; A. Galli and L. Nechi, Researches on unstable children; A. Galli, Contributions to the study of moral judgments of normal and abnormal children; A. Gemelli, Current psychotechnical problems in national industry; A. Gemelli, On psychotechnic activity in the laboratory; A. Gemelli, On the nature of manual ability; A. Gemelli, Experimental researches on the forms of voluntary movement; A. Gemelli and A. Galli, On the adaptability of human action to machine action; A. Gemelli, On the value of simple reaction-time with respect to its applications to personal selection; A. Gemelli, Problems of experimental psychology in the study of physical exercise; A. Gemelli, On the selection of aviation pilots.

Indiana University J. R. KANTOR

Le sens de la direction et l'orientation lointaine chez l'homme. By PIERRE JACCARD. Preface by J. Larguier des Bancels. Paris, Bibliothèque Scientifique, Payot, 1932. Pp. 354.

The author of this book on the sense of direction and remote orientation in men is not satisfied to give us a mere statement of this problem as it stands today, but he presents a rather exhaustive account of the way the relevant phenomena have been regarded in the annals of human thought. On this account, and because of the critical scientific attitude of the author, the book is of significance.

He does not hesitate to bring into the discussion mention of Noah's dove returning to the Ark, and other legendary incidents which indicate early views. Considerable space is given to the legendary, the near scientific, and the scientific inquiry into the behavior of animals as regards their sense of direction and orientation at a distance as well as to that of man. The latter is, of course, the main problem of the treatise. The author notes the efforts of the erudite mind to extricate itself from the anthropomorphic and fanciful interpretation of natural and psychological phenomena. One notes the author's severe criticism of Darwin for disregarding the now esteemed 'law of parsimony' with the implications of animistic meanings, and of even more recent interpretations such as are found in Jacques Loeb's mechanistic conception—for example in his "tropisms." Those who still hold to 'instinct' will find little comfort here, and such speculative writers who are still willing to base their conclusions on reports from travelers on the sense of direction and orientation of the *uncivilized* will find that such peoples when placed under controlled conditions do not possess any such natural sense of direction.

The book is to be regarded as a negative reaction to Levy-Bruhl's theories as they were accepted in France. For the 'mysterious sense of direction' or capacity for distant orientation in either animals or man, the author substitutes as a working principle the guidance by landmarks and other stimuli at hand of all such behavior. While further experimentation is still needed, doubtless this principle will eventually be established. It is noted that American psychologists have not fallen into the pitfalls of anthropomorphic interpretations of such behavior and of other instinctive conduct.

University of Denver THOMAS R. GARTH

Das erlebende Ich und sein Dasein. By OTTO JANSSEN. Berlin and Leipzig, Walter de Gruyter & Co., 1932. Pp. viii, 247.

This book belongs to that German school of philosophical thought the development of which is marked by the names of Husserl, Scheler, and Heidegger. Its phenomenological method and its problems are *not* those of empirical psychology and cannot be represented adequately without a considerable discussion of the philosophical implications. The book can be recommended only to the philosophically interested and trained reader.

The first part discusses the 'living and experiencing ego:' Ego is shown not to be subject to any definition; its chief characteristic is that of being a 'who' and not a 'what.' Emotions, wishes, attitudes in which the ego 'lives' constitute the sphere of the 'ego-belonging' (das Ichzugehörige) as opposed to the 'ego-strange' (das Ichfremde): colors, shapes, etc. However, pathological experience shows that the intrinsic bond between the 'ego' and the 'ego-belonging' can be loosened (depersonalization), and even the apparently impossible duplication of the ego in its own field is achieved in some cases of splitting of personality. In the second part the relation of ego to being and consciousness is discussed and relational theory of consciousness is sharply criticized.

Of interest is the author's evaluation of the phenomenological method. Throughout the whole book he argues against the logistic interpretation of the phenomenological findings which ascribes to them apodictic validity. He shows repeatedly that relationships apparently constitutive for the ego (*e.g.* its non-spatial character, its unchangeability, its uniqueness in its field) can prove empirically non-valid under special conditions, and are therefore only probable, not necessary. This, as the author himself points out, stresses the empiricism of the phenomenological method, but at the same time the acknowledgment of a discrepancy between phenomenological and empirical findings leads to the problem of validity and limitations of the phenomenological method. It would seem to one that this problem, though seen clearly by the author, is not solved adequately by the introduction of the concept of 'Geltungswechsel:" a mysterious, logically unexplainable, non-temporal change of an intrinsic relationship, which for example makes possible the entering of the essentially non-spatial ego into the sphere of the spatial.

Worcester State Hospital E. HANFMANN

Man as Psychology Sees Him. By EDWARD S. ROBINSON. New York, Macmillan Co., 1932. Pp. vii, 376.

In this book Robinson attempts to present some of the experimental findings of modern psychology together with a general point of view toward man and his nature, in such a way that they can be understood by the above-average reader who is not technically trained in psychology. In the reviewer's opinion he has succeeded admirably in this endeavor.

The book is divided into two parts, the first dealing with man and his nature from the modern psychological point of view, the second part discusses psychology as a science.

In the first part, the topics of motivation, heredity, learning, thought, personality, and the mind-body problem are dealt with. Many psychologists will find the last topic a little out of place but it is adequately treated in a manner comprehensible to most of those who will read the book. The reviewer feels that the first chapter, that on motivation is the weakest, drawing rather heavily as it does on doubtful psychoanalytic concepts and explanations. In justice to the author it should be said that such concepts are presented tentatively, conservatively, and without explicit endorsement. It is doubtful, however, that uncritical readers will recognize this. The chapter on learning, although the most technical in the book, is perhaps the most adequate.

The point of view presented in part two is eclectic—the agreements, rather than the differences among the schools and men in psychology, being stressed. The present limitations of psychological information and doctrine are pointed out.

For a sane, conservative, authoritative book on psychology for the general reader the reviewer knows of none better.

University of North Dakota C. W. TELFORD

Genetic Psychology. By A. R. GILLILAND. New York, Ronald Press Co., 1933. Pp. xii, 351.

The evolution and the development of structures and functions are traced through 20 chapters, the subjects of which are suggestive of the contents. This work on genetic psychology is presented by the author as, "with one exception, the only text of recent times, which reflects the evolutionary or genetic approach to psychology." Chapters I and II are introductory in character. The first states the essential problem of a genetic psychology and the second considers the general characteristics of life and the theories as to its origin. The phylogenetic order from the protozoa to the human inhabitants of the bronze age is unfolded clearly yet simply through the four succeeding chapters.

The treatment of inheritance in Chapter VI is somewhat more technical than is commonly found in current texts in child psychology and is yet too condensed to be conducive to the greatest clearness. The chapter on prenatal development deals almost exclusively with structure but is appropriately and copiously illustrated. The chapters The Child at Birth, and Physical Growth and Skill, are too condensed to be of any great value even to elementary students, a criticism which holds also for the chapters on music and art, adolescence, the growth of personality, the growth of character, and disease and health. Students in general psychology, child psychology, or educational psychology will find more adequate treatment of these subjects in the current texts of these respective fields. The chapters on play, intelligence, perception, language, memory and thinking, and the emotions are somewhat richer in content and compare favorably with other chapters in contemporary texts.

The author suggests that the book may be employed as a textbook where the approach is biological and that it is especially suitable for collateral reading. The reviewer is more in accord with the second of these suggestions, and believes that the first seven chapters are more valuable for this purpose than the subsequent ones. It is essentially an introduction to psychology through the genetic approach. It would be quite inadequate as a textbook for students who have already had a course in elementary psychology. The lay reader would find it interesting. Each chapter is followed by a short list of selected references.

Iowa State College THOMAS F. VANCE

Integrative Psychology: A Study of Unit Response. By WILLIAM M. MARSTON, C. DALY KING, and ELIZABETH H. MARSTON. New York, Harcourt, Brace & Co., 1931. Pp. xvi, 558.

This book illustrates the folly of trying to write a systematic psychology with all of the impedimenta of a new vocabulary, new definitions and a "unified idea" under the guise of a textbook for beginners. Among the systematic positions adopted, we may name the following: (1) psychology is a science of behavior where behavior is characterized by unique integrative activities; (2) such behavior is to be found only among living creatures possessing a synaptic nervous system, *i.e.* only among the higher animals; (3) the real subject matter of psychology is not behavior at all but the consciousness that is correlated with the integrative action of the organism; (4) the integrative mechanisms in human beings are spontaneously operating devices which free the individual from environmental control and from the taint of materialism; (5) fundamentally, the unit responses of a unit organism are responses of opposition or of alliance. These may be made in the direction of greater or of lesser activity and thus the authors come to four fundamental types of action, viz., dominance, compliance, submission, and inducement.

A considerable amount of the current experimental literature is woven back and forth across this systematic background. One does not get the impression, however, that the systematic ideas have been forged out of the facts. On the contrary, the authors have felt keenly the confusion wrought in the minds of beginners by so many systems of psychology and they have, therefore, solved a teaching problem by inventing another system. It seems doubtful whether this method of writing textbooks can serve either the task of teaching elementary psychology to beginners or the task of fabricating the systematic foundations of a science.

University of Illinois COLEMAN R. GRIFFITH

The Nature of Human Conflicts, or Emotion, Conflict and Will: An Objective Study of Disorganization and Control of Human Behavior. By ALEXANDER R. LURIA. Tr. from the Russian and edited slightly by W. Horsley Gantt. Preface by the translator and foreword by Adolf Meyer. New York, Liveright Inc., 1932. Pp. xvii, 431, 133 figs.

This book may appeal to persons interested in neurodynamics and as an example of Russian scientific research in psychology. It is an objective study of the motor responses which accompany reactions to controlled word association. Graphic records of these responses are compared and significant variations are linked with inner conflict. Luria assumes that these motor responses are of two main sorts: (1) those produced when the personality suffers no conflict; and (2) when there is conflict. The former responses are immediate and smooth while the latter are both delayed and irregular. He explains the disturbed motor response as due to a disorganization of culturally instilled psychological mechanisms and a reversion to simpler and lower levels of reaction. That speech does not function smoothly when there is conflict is due to the same causes that make motor response atypical, *i.e.* speech depends upon the same psychological mechanisms of a higher sort instilled or modified by cultural experiences.

His cases include children as well as adults, the abnormal as well as the normal, and normal adults tested under both usual and unusual conditions.

His final chapter on the "Control of Behavior" is theoretic but convincing. For instance, his view that the use of vocal or subvocal speech is an effective autostimulus for desired action finds ample support in everyday life since speech frequently accompanies the performance of difficult motor operations, as the golf novice will testify. That this speech is a cause of properly timed coördinations is probable but not so obvious.

The book as a whole is scholarly and valuable but it is so difficult to read as to make it improbable that it will have wide use.

Trinity College, Hartford, Conn. R. B. W. HUTT

An Experimental Manual in Psychology. By JOHN F. DASHIELL. New York, Houghton Mifflin Co., 1931. Pp. ix, 161.

This manual is designed to be used in conjunction with the author's textbook, the *Fundamentals of Objective Psychology.* It is admirably suited for this purpose, since every important topic in the text is paralleled by a concrete experiment. The objective approach is emphasized, though this does not preclude experiments in sensory, perceptual, and higher thought processes. Topics of current interest receive due emphasis, such as motivation, emotion, personality, social influences, the conditioned reflex, and fatigue.

Each experiment is logically arranged with problem, materials, procedure, results, questions for discussion, and pertinent references clearly outlined.

Illustrative cuts of apparatus accompany most of the topics, which supplement the wealth of photographic illustrations in the author's text. An appendix contains copious notes for instructors, including a list of psychological supply houses and instructions about apparatus.

Although fitted to beginning students, the manual is extensive enough for profitable use by more advanced students, and the range of topics is sufficiently broad to be useful as an accompaniment to almost any introductory textbook in psychology.

University of Chicago A. G. BILLS

Psychology of Sex. By HAVELOCK ELLIS. New York, Ray Long and Richard R. Smith, Inc., 1933. Pp. xii, 377.

The author of this book is also responsible for the classic five-volume study on the subject of psychology of sex. The present monograph is largely a modernized and practical abstract of the larger work, and this process of modernization has made it much more valuable for the clinician, particularly for the physician who must discuss sex problems with his patients. The general tone of the book is descriptive. While the actual scientific validity of the information which it contains is not impaired by this fact, investigations are discussed from the point of view of results and their significance rather than from that of experimental procedures. In this respect it resembles the work of Freud which Ellis uses in a simple and understandable fashion, neither accepting it completely nor rejecting it. The usual topics to be found in a book of this nature are considered, such as the biology of sex, description of sexual impulse in youth, sexual deviation and homosexuality. There are also chapters on marriage and the art of love, which topics are dealt with in a generalized but sane fashion. Case histories are of necessity lacking, but the bibliographies on each topic are excellent. A teacher of sex education or a gynecologist should be able to find considerable use for this book, but the researcher into sexual psychology will find Ellis' older work, supplemented by the references given, more valuable.

Institute for Juvenile Research LOWELL S. SELLING

Experimental Social Psychology. By GARDNER MURPHY and LOIS BARCLAY MURPHY. New York, Harper & Bros., 1931. Pp. ix, 709.

If there is any truth in the statement that men have an almost unlimited capacity for resisting the application of knowledge to their own affairs, a part of the truth must derive from the fact that the amount of reliable knowledge has been woefully small. The book now before us plainly says that some of this knowledge is at hand. It says, also, that methods for getting more knowledge are gradually emerging. There is, therefore, a great difference in the language and in the mood of the present book as compared with earlier works on the subject. It is obvious, of course, from such chapter headings as "The Biology of Motives," "Nature and Nurture in the Causation of Individual Differences," "The Learning Process in Social Situations," and "The Development of Social Behavior in Early Childhood," that *Experimental Social Psychology* is an amalgam of several other psychologies; but this fact is, perhaps, irrelevant when we consider the great advantage that comes from having the experimental material on problems of a social sort brought together in a single volume. At times, the chapters read as though a collection of abstracts had been pasted together; but even this can be pardoned, pending the day when the same authors, perhaps, can review the field more systematically and critically. The book is richly documented and it shows abundantly that the social implications of psychological facts can have a reputable meaning.

University of Illinois COLEMAN R. GRIFFITH

Religious Behavior. By David M. Trout. New York, Macmillan Co., 1931. Pp. xiv, 528.

The author of this book has attempted to describe in objective terms a group of phenomena which are, by tacit acceptance, spiritual, and therefore beyond the ken of behavioristic analysis. On the other hand the point of view is not behavioristic in the systemic or mechanistic sense. The approach is avowedly organismic, which may account for the fact that it is sometimes a bit involved.

To start with there is a new definition of religion which is borrowed from Paul's *Epistle to the Corinthians;* namely, that the abiding religious realities are hope, faith, and love. These are treated as organic and purposive attitudes, rather than subjective entities. Goal-seeking becomes, therefore, the test of religiosity. The definition is later expanded to imply that telic behavior must also be loving behavior in order to be religious. The goals must be social rather than selfish. Being religious is a matter of adjustment to the total organismic pattern of life.

In its emphasis on a sort of mystical rapport with the race, it leaves out fear, hate, and self-service which have played a part in all religions.

The treatment is original throughout. A survey of researches and a suggested list of problems is included. There is an excellent bibliography.

University of Colorado THOMAS H. HOWELLS

Die gerichtspsychologische Begutachtung von Autounfällen und die Eignung zum Chauffeur. By Karl Marbe. Leipzig, Hirschfeld, 1932. Pp. 64.

The author discusses the actions performed by locomotive engineers, truck drivers, and automobilists in a brief, but clear and exact manner. He recites his experiences as expert in court. The American psychologist will find little in this book that is new to him. Perhaps the author's contention (among many other suggestions) that the wording of laws and orders must be free from inducing that which is actually wrong, is worthy of experimental study in America. When a locomotive engineer, *e.g.* is ordered "to enter a siding in spite of the stop signal and then to proceed further," can he be blamed for overruling also that stop signal which he sees at the end of the siding and for thus causing a wreck? Or, when a motorist in a certain city lawfully crosses a street where a sign prohibits turning to the right, and then lawfully crosses a street where a sign prohibits turning to the left, and then approaches a crossing where the conspicuous sign reads "turn right or left" without mentioning a prohibition, can he be blamed for crossing straight ahead and overlooking an inconspicuous sign 25 ft. sidewise which reads "no forward passage"?

University of Missouri MAX F. MEYER

Psychodynamik des primitiven Denkens. By F. M. Feller. Leipzig and Vienna, Franz Deuticke, 1933. Pp. vi, 57.

In this very short book the author points out his solution of some of the ethnological problems, such as totemism, the taboo of dead bodies, exogamy, and other primitive ideas. His method is the study of the findings of early ethnologists rather than the analysis of any new data of his own deduction. His expression and interpretation are on a basis developed by combining Wundt's laws of association and deep psychoanalysis. It is his idea that primitive people do not differ from those of the present day in that they do not advance so far in development, but rather that they develop along different channels and have the capacity for the same development.

Some of his interpretations seem a bit far fetched but since they are derived by means of a type of psychological study which is not yet entirely approved by psychologists and since he comes to his conclusions largely through logical methods, too much cannot be expected. Such a book is undoubtedly of more interest to ethnologists than to psychologists.

Institute of Juvenile Research LOWELL S. SELLING
Chicago

The Psychology of Study. By C. A. MACE. New York, R. M. McBride & Co., 1932. Pp. viii, 96.

Six essays give a common sense summary of thoughts upon study. Mental functions are dealt with in a severely practical way. Man, it is assumed, "is an instrument for responding to stimulation in ways which suit his purpose." The discussion centers about perception, memorization, originality and thinking, concentration and the will to work.

A fundamental maxim of the writer emphasizes letting questions come, getting them clear, following their lead, and then asking one's self another question. The importance of the organization of habit and of the affective disposition is emphasized. Study requires the will to work and the organization of external conditions. Except for extreme cases, individual differences boil down to a difference in speed. References are to F. Bacon, Chesterton, Coué, Descartes, Ebbinghaus, Freud, Roger Fry, Henry and William James, Locke, McDougall, Newton, Nietzche, and Torricelli; experimental workers receiving relatively little recognition.

In an interesting and pleasing style the author presents what is commonly accepted as useful information about studying; some of the statements will be questioned by psychologists, but in general the book can be recommended as giving useful suggestions to the general reader.

University of Georgia A. S. EDWARDS

Hygienische und gewerbetoxikologische Untersuchungsmethoden. By EUGEN LEDERER. Abderhalden's Handbuch der biologischen Arbeitsmethoden, IV, Teil 16. Berlin and Vienna, Urban & Schwarzenberg, 1932. Pp. 519-932.

While there is little in these 400 pages which can be said directly to be psychology, there is much technological information which occasionally will be welcome especially to him who is working in the field of applied psychology. For instance, climatic conditions, temperature, moisture, air movement, clothing are influences which sometimes concern the psychologist. Likewise illumination by daylight or artificial light, seating arrangements at various kinds of work, noises which disturb the worker, vibrations so strong that they are a danger to health. A greater number of measuring instruments are described, chiefly of German design and manufacture. Specially emphasized among them are those for determining the presence of poisonous gases in the air in great dilution.

University of Missouri MAX F. MEYER

The Logic of Science. By H. R. SMART. New York, D. Appleton & Co., 1931. Pp. x, 237.

This is a fairly readable introduction to the thesis that the peculiar nature of the relation between the object and the subject in scientific knowledge is a logical rather than a physical or mental relationship. That is, the function of a logic of science is to search out those abiding features in the content and form of scientific knowledge which serve to identify the several branches of science and give unique standing to the whole of experimental inquiry. Throughout a series of eight chapters in which the author is obviously more at home when speaking of the mathematical and the physical sciences than of the biological and psychological sciences, the "abiding features" of scientific method are identified. These features touch psychology at two points, viz., in the definition of subject matter and in the relations between psychology and logic. The author suggests that psychology take mind as the biologist takes life, viz., in a functional rather than in an existential way. Since psychology studies the origin and development of functions out of which logical relationships can spring, the relations between logic and psychology are of a mutually dependent order.

University of Illinois COLEMAN R. GRIFFITH

Workbook in Elementary Psychology. By A. E. BROWN. New York, Macmillan Co., 1932. Pp. iii, 182.

Instructors who give large classes in semester or quarter courses will be interested in this teaching device. The workbook is based chiefly on the revised edition of Gates' *Elementary Psychology,* but it contains optional references to a half dozen other texts. A provision for conference groups within the class as a whole overcomes the objection that this method of instruction is mechanical and stereotyped. A sufficient variety of material is offered to prevent the student from becoming bored in his work. In addition to the forty-one study units, four review lessons are included. Each study unit contains (1) directions for study; (2) additional notes, which are valuable to beginning students; and (3) an assortment of study exercises or test questions of the yes-no, matching, completion, enumeration, analogy, and discussion type. The sheets are perforated and punched so that they may be removed, turned in to the instructor, and filed in the standard 8½ x 11 in. notebook.

Southwestern College W. R. ATKINSON

Endocrine Medicine. Vol. IV. By WILLIAM ENGELBACH. Foreword by Lewellys F. Barker. Springfield, Ill., and Baltimore, Md., Charles C Thomas, 1932. Pp. 117.

This volume contains 49 pages of bibliography classified for the several chapters in the first three volumes, an index of names of 14 pages, and a 47-page index of subjects. With these aids the reader is able to locate readily any subject dealt with in the first three volumes, as well as the original sources drawn upon by the author. The three earlier volumes of the series (reviewed in this JOURNAL, 46, 1934, 169) deal respectively with "general considerations," the infantile and the juvenile endocrinopathies, and the adolescent and the adult endocrinopathies. The volume will be useful to psychologists interested in the problems involved in this field.

J. P.

Methoden des Tierversuches in der Arbeitsmedizin. By EBERHARD GROSS and HERMANN HEBESTREIT. Abderhalden's Handbuch der biologischen Arbeitsmethoden, IV, Teil 16. Berlin and Vienna, Urban & Schwarzenberg, 1932. Pp. 933-1025.

This is a detailed discussion of the methods used in experimental studies of the effect on animals of injurious substances when acting on the lungs, the digestive tract, and the resorbing skin, or when irritating the skin. Much technological information.

University of Missouri MAX F. MEYER

The Evolution of Human Behavior. By C. J. WARDEN. New York, Macmillan Co., 1932. Pp. ix, 248.

In this book the author offers a treatment that is "as non-technical as could well be without sacrificing something of scientific accuracy" of the evolution of human behavior, rather than the evolution of human bodily structure.

In the first part of the book, after a clear, brief treatment of the organism-environment relationship both in psychology and in evolution, a review is presented of the traditional biological problems of evolution. The next chapter is devoted to a consideration of the natural kinship of man to infra-human animals. In this chapter emphasis is placed upon morphological development in the phylogenetic series according to the manner of the classical evolutionists. A number of diagrams are here reproduced from strictly biological works. One of these diagrams, taken from Tilney's "The Brain from Ape to Man," showing the developing "primate horizons" with precise indications of association areas and the like, may possibly be misleading when presented as divorced from the complete setting of its original text.

The next sections of the work deal with the progress of the anthropoid into the true human individual. Obviously much of this treatment must be speculative. In this part of the book such problems as the morphological kinship of ape and man and the phylogeny of language are briefly discussed. In the latter part of this section much reliance is placed upon the sort of evidence presented by Osborne in his book, "Man Rises to Parnassus," and is subject to the same favorable or unfavorable criticisms that have been given to that book. Superimposed upon this background, a brief review is presented of man since the upper paleolithic time. Concerning this development Dr. Warden writes: "The appearance of this new species marked the last great advance in structural evolution and ushered in the age of modern man. Homo sapiens possessed a larger and better balanced brain than any of his predecessors. He was distinguished also by numerous refinements in bodily form and by superior manipulative skill and language capacity. In time, the natural increase in numbers in the homeland forced the species to spread over the earth, and to become diversified into the three great racial stocks—negroid, mongoloid, and caucasoid."

The last chapter of the book is devoted to an evaluation of present trends in evolution. Here the position is taken that the urbanization and degeneration of the "great race" does not mean the end of human culture, "so long as other races are left behind which have the physical strength and mental caliber to carry on."

As E. G. Conklin pointed out a number of years ago, scientific biology during the present century has largely turned from the speculations of phylogeny to the experimental study of ontogeny. A similar shift in emphasis can be seen in scientific genetic psychology. This shift does not mean, however, that problems of racial development such as those treated in this book are uninteresting or that they should be neglected. It merely points to the fact that they are peculiarly difficult to deal with from the scientific standpoint, as they are not even potentially open to experimentation. This situation probably explains the fact that in the present book there are not many references to the work of experimental psychologists, save such material as is given in a review of Wundt's speculations in regard to language and in a few similar excerpts. In some respects it is, however, surprising that the author has considered reference to the experimental studies of anthropoid behavior inappropriate to certain parts of the argument.

From the standpoint of the reviewer, however, the major shortcoming of the treatment is the almost complete neglect of the ontogenetic evolutionary problem. This omission has led the author to avoid any reference to the processes by which the alleged developments of the race become effective in the individual. It might not have been without value, for example, to consider the evidence of modern ontogenetic studies in evaluating the phylogenetic theory of the behavior of children who walk on all fours. A detailed consideration of ontogeny might well have taken the present volume beyond the bounds set by the demands of a popular book. But at all times especial care must, it seems, be taken by the necessarily speculative student of phylogeny not to attribute to the essentially hypothetical racial environment phenomena which may experimentally be shown to result from the interaction of the environment and the developing organism in ontogeny.

An excellent brief list of references is given at the close of the book.

Brown University LEONARD CARMICHAEL

Crime, Law, and Social Science. By JEROME MICHAEL and MORTIMER J. ADLER. New York, Harcourt, Brace & Co., 1933. Pp. xxix, 440.

Here is a penetrating logical analysis of the relation of the social sciences to the practical problems of criminal justice, a revision of a survey originally made under the auspices of the School of Law of Columbia University for the Bureau of Social Hygiene. The authors, a legal scholar and a philosopher, have formulated the theoretical problems of the criminal law, examined and evaluated the research in this field, and summarized their conclusions and recommendations in the form of a plan for an institute of criminology and criminal justice.

The analysis is thorough, the logic incisive and compelling, the conclusions positive and devastating. The argument running through the book, and oft repeated, is that in the present state of our knowledge no empirical science of criminology is possible, for such a science must depend on an empirical science of psychology and of sociology, which as such, are non-existent. Psychology, it seems, is suffering from dull logical tools and an immature epistemology. The failure of psychology to measure up to the exacting requirements of an empirical science as formulated and defined by the authors, is attributed to the lack of definition of subject matter and a misconception of the nature of the scientific method. Most of the material presented by psychologists, it is claimed, is physiological rather than psychological. Furthermore, psychologists in their attempt to get away from a simple rational science which is no longer respectable psychology, have swung to the other extreme. With "an exclusive emphasis upon observation to the total neglect of the abstractions of analysis" they have thrown the subject over to a "raw empiricism." Data yielded by psychological investigations of criminal behavior are both invalid and insignificant because the concepts prerequisite to observation are not clearly defined and the statistical descriptions utterly lack direction by theoretical analysis. These contributions therefore amount to little more than descriptive knowledge.

An extensive, if not intensive, survey of the many research studies in the causation, treatment and prevention of crime lead the authors to the conclusion that the indispensable requirements of scientific procedure and method have not been met. They have failed to give us any knowledge whatever of the etiology of crime, for they have failed to formulate the causal problem in terms of the dependent and independent variables; nor can they tell us whether criminals can or cannot be differentiated from non-criminals in terms of any set of factors. Though the authors distinguish between the subject matters of psychology and sociology they have a blanket condemnation of research studies contributed by both. Many of the studies cited are not, of course, the work of psychologists.

On the constructive side they propose plans for an institute of criminology and criminal justice to attempt a solution of the two basic problems, the theoretical problem of etiology of criminal behavior and the practical problem of control. They recommend that the kind of research that is now being done be discontinued. It is significant that in the criminological division of the proposed institute, psychologists and sociologists would be replaced by logicians, mathematicians, statisticians, and physicists, the latter for their knowledge of the basic concepts of empirical science. It is the contention of the authors that the psychologists and the sociologists, lacking broad philosophical orientation, adequate knowledge of the methodology of science and the instruments of logic and mathematics, "have shown themselves utterly incompetent to construct a science of criminology." An exception is made of the special field of psychometry where an empirical science is developing.

A weakness of the book is its self-conscious attempt on theoretical grounds to place all psychology outside of the closely circumscribed "empirical sciences" without adequate consideration of psychological contributions other than criminological. This thought-stimulating book is, however, a challenge to psychologists, sociologists, and other students of criminal behavior. To such it is a valuable and, in the reviewer's opinion, a very important contribution.

Dartmouth College EDWIN POWERS

Revelation in Mormonism. By GEORGE B. ARBAUGH. Chicago, Univ. Chicago Press, 1932. Pp. x, 252.

This book constitutes an excellent case-study of the development of a sacerdotal religion. Of all the major anthropomorphic religions Mormonism is unique in that it is more modern, that the sources are observable, and that more critical attention has been given to them. In spite of the fact that the background of Mormonism has been considerably blurred and distorted because of the bias of early proponents or opponents, the mass of evidence seems adequate to characterize it.

The author presents a very intimate account of the various expediencies of the promoters of an organized cult. The first part deals with the craze of a century ago in rural New England for the discovery of buried treasure and lost manuscripts of ancient times. This craze infected Sidney Rigdon and Joseph Smith and culminated in the Book of Mormon and several other books and pamphlets, and finally in the restoration of an ancient but lost priesthood and church organization. The second part deals with adaptive changes by which revelations became familiar and mundane, and were used by Joseph Smith to obtain for himself a monopoly in seership and social control. The third part presents a period of competitive and contradictory revelations, in affirmation or rebuttal of various claims of succession to the headship of the church after the death of Joseph Smith. A number of the resultant sects are yet in existence, but revelation, in the old sense, has been tacitly abolished by the larger groups and the heavens are again silent.

The mystic element has never been prominent in Mormon revelation. The appeal has always been in terms of economic and familial advantage, institutional sanction, and theological phantasy, rather than in ecstatic rapport with the Divine. It is notable that Mormonism has never inspired any important poetry, art, or music.

The apparent service of this study is chiefly that it does a thorough job of assembling the available data. The author does not attempt a great deal in the way of either a psychological or a sociological interpretation of these data. He holds that the revelations were altogether the product of deliberate fraud, rather than of split personality as advocated by I. W. Riley. The obvious opportunism in them is cited in support of this view.

There is virtue of course in calling a spade a spade, but on the other hand one can be too dogmatic about his categories, especially in regard to religious and spiritistic phenomena. Deliberate fraud, self-deception, hallucination, and social leadership are, in religion, such a part and parcel of each other that they are seldom seen in pure form. A comparative interpretation is always more truthful. This fact is demonstrated by the author's own testimony that the defrauded, gullible ones became in their turn the defrauders. In most religions there seems to be a queer paradox of coexisting deceit and sincerity.

There is an abundance of references to sources, and a valuable bibliography is appended.

University of Colorado THOMAS H. HOWELLS

The Gestalt Theory and the Problem of Configuration. By BRUNO PETERMANN. Tr. by Meyer Fortes. New York, Harcourt, Brace & Co., 1932. Pp. xi, 344.

This treatise on the Gestalt theory is unique in its presentation in that the author does not overwhelm the reader by shoving the whole system before him at once. By a careful analysis of publications on the subject Petermann introduces one concept after another in their genetic order. By this method he is able to take into account the shifting of standpoints within the literature. Furthermore, it enables him to attribute the various concepts to individuals rather than to the system as such. Finally, Petermann's genetic method enables him to present systematic concepts in their inextricable relation to experimental findings.

Within the last score of years we have heard much of the "new" Gestalt Psychology which is purported to have had its origin in Wertheimer's investigation, *Über das Sehen von Bewegung,* in 1912. Petermann shows that the principle characteristics of the "total situation" of behavior, which seem to constitute the groundwork of the configurational theory, are traced in their progressive development from Von Ehrenfels through Cornelius and Lipps. Other contributions to this concept are Benussi's "Production" theory, and G. E. Müller's "Coherence" theory.

After giving us a clear statement of the working principles of the Gestalt theory in Book I, Petermann summarizes the systematic field of the theory as follows: (1) The theory is not confined to psychology. Its essential principles are applicable to biology and physics as well. (2) Psychological Gestalten are essentially physical prototypes. (3) In biology the theory attempts to resolve the controversy between vitalism and mechanism. (4) In psychology, it undertakes to derive "the entire range of psychical phenomena from one unitary and determinate basal doctrine, in the form of a unitary theory."

In Part 2 of Book II there appears a review of the literature on figural structures upon which the Gestalt theory has relied for empirical verification of its proposed laws. Special emphasis is placed upon Wertheimer's "Gestalt factors," and Köhler's interpretation of these. It is concluded that Wertheimer's description gives an accurate account of the spatial collocation which takes place in the building up of figural unity. However, Wertheimer's description "by no means brings analysis to an end." Petermann introduces here his own theory of "cognizance in the mode of apprehension." He believes that in addition to the factors taken to be responsible for contrast, reversible perspective and other figural phenomena, attentive characteristics come into play. In general, Petermann takes the attitude that in their zealous intent to construct a psychological system Köhler and Koffka have lost sight of the "subjective conditionalities of our Gestalt experiences, which are under certain circumstances so conspicuously exhibited." It should, however, be pointed out that the term "attentive apprehension" when inadequately defined, contains no more analytic value than the magic term *Gestalt* and its accessories.

In evaluating the book as a whole we readily admit that it is the clearest statement and the profoundest critical assessment of the configuration theory now available to the current English speaking public. We should add that we are greatly indebted to the translator, Meyer Fortes, who has done a difficult task well.

University of Vermont P. H. EWERT

Principles of Judicial Proof. By JOHN H. WIGMORE. 2nd ed. Boston, Little, Brown & Co., 1931. Pp. xix, 1056.

The first edition of this book (1913) has long been favorably known to those psychologists who are interested in the testimonial process. The book has been particularly useful because it presented fairly a generalization of psychological experimentation in the field, and an evaluation of it by the foremost American legal authority on evidence. Its purpose was to set forth a method whereby each bit of evidence might be evaluated as it comes, the value indicated by a symbol, and the symbol placed in a schema. It was the author's hope that by this method the final evaluation of the testimony as a whole might be made on the basis of the sum of the evaluations as given simultaneously in the schema, instead of the usual method of cumulative evaluation as the testimony is received. The plan, however, as originally stated did not receive the attention either of jurists or of psychologists that it deserved; and in this new edition, which is entirely revised and re-written, the author attempts not only to bring the book up to date but also to place his method in bolder relief. The book is devoted to an analysis of the whole evidential process in the interest of this method, and at every turn the author finds psychological factors like emotion, habit, perception, recollection, imagination, intention, belief, temperament, and the like. Moreover, in addition to an excellent digest of the *Aussage* experiments, the author, in a chapter entitled "Experimental Psychometry," presents in great detail the blood pressure ("Lie-Detector") method of detecting guilt with particular reference to Keeler's work. There is also, in the same chapter, a revised account of the word-association method in which he quotes extensively from Crosland's monograph. The author thus shows his interest in these experiments although he does not think that at the present time they are of great value.

In many ways the book is a challenge to psychology. The author's method of evaluation of testimony itself raises a problem for the psychology of conviction. Frequently, moreover, statements are made which a psychologist may question but which cannot be answered save by experimentation. On the whole, however, Wigmore's psychology is admirable, coming as it does from one who is outside the field. The book's excellent digests and bibliographies, and its suggestiveness make it invaluable to anyone concerned with the application of psychology to the problems of judicial proof.

H. P. Weld

Cornell University

Meaning and Change of Meaning. By Gustav Stern. Göteborg, Wettergren & Kerbers, 1931. Pp. xiii, 456.

This book should prove a godsend to those psychologists who are struggling with problems of language, containing as it does not only a great mass of linguistic material, conveniently arranged and carefully documented, but also a full and thoroughly up-to-date discussion of the problems of verbal meaning. No other existing work approaches it in scope and excellence, and it is improbable that a successful rival will appear for some years to come.

The present-day psychology of language suffers from two outstanding defects. In the first place the psychological study of language presents so many intrinsic difficulties that the laboratory scientist, fearful of embarking on a voyage which might carry him beyond the confines of his neatly furnished work-shop, has usually been led to ignore the more complex semantic problems and limit himself to semi-physiological experiments in phonetics. In the second place, students of linguistics proper who have adopted a physiological approach have all too frequently failed to realize that psychology has undergone considerable change since the days of Wundt's *Völkerpsychologie.* We have in consequence, with a few notable exceptions, a psychology of language which is either inadequate in scope or archaic in point of view.

One of the merits of Professor Stern's book is that it is open to neither of these charges. He has shirked no problem because of its difficulty, and he displays a knowledge and a grasp of recent psychological literature which should put many a *Fach*-psychologist to shame. The book is presented as a first textbook in semasiology, devoting itself in the first half to a psychological analysis of verbal meaning in terms of linguistic functions, and in the second half to a classification and a detailed study in terms of the same functions of seven fundamental types of sense-change. Stern's psychological point of view is functionalistic, and his approach genetic. His principal linguistic source is the New English Dictionary, and for his psychological material he draws with equal willingness from recent publications in English, French, and German. It is evident that he leans heavily on Head's psychopathological studies, and it is gratifying to find that he attaches due importance to the too-seldom read works of Selz. The reviewer has failed to observe any serious omissions or misinterpretations.

Cornell University R. B. McLeod

Some Effects of Incentives: A Study of Individual Differences in Rivalry. By Joseph Zubin. (Contr. to Educ., No. 532.) New York, Bureau of Publications, Teachers College, Columbia University, 1932. Pp. 60.

This is a well controlled experimental study designed to measure the effect, on certain functions, of motivated individual versus individual rivalry, and to determine whether the individual differences shown were reliable. The functions involved are (1) addition, and (2) comparison of numbers. The "motivated" individual rivalry was obtained by promising a prize to each pupil who should excel his immediate superior in rank order. The children were not told what the prize would be. In the non-motivated rivalry it was assumed that no incentive was present. The subjects were 217 school children from Grades 6, 7, and 8. The results show an average gain of 5.5 ± .33 examples per min. in the incentive periods over the non-incentive periods; and a smaller coefficient of variation of incentive speeds than of non-incentive speeds. For addition the difference was 1.70 ± .55, and for comparisons it was 1.97 ± .30. The correlation of average gain with age was 0.11 ± .06, and with intelligence 0.13 ± .07.

The study offers little that is new in technique. Since the meaning of a "prize" is not the same to all children, it is, of course, not accurate to assume an incentive of equal force for each child. The narrow range of activities and interests of the group used prevents broad generalizations; this limitation, however, suggests further investigations both above and below the ages and grades studied here.

University of Texas O. B. DOUGLAS

Why We Oppose the Occult. By EMILE CAILLIET. Tr. by George Franklin Cole. Philadelphia, University of Pennsylvania Press, 1931. Pp. v, 200.

"To Professor Edgar A. Singer, Jr., this study, made at the University of Pennsylvania under the auspices of the Adam Seybert Foundation of Intellectual and Moral Philosophy, is respectfully dedicated." Thus, this study is apparently a successor to the famous "Preliminary Report" of the Seybert Commission on spiritistic phenomena in 1887, and supported by funds from the first bequest ($60,000) to be accepted by an American university to provide for research in the field of metapsychics.

The author has brought together anthropological data, some of it obtained during a sojourn of four years in Madagascar, supporting his thesis that the violent opposition to the occult expressed by modern society issues from an atavistic terror of magic and the dead, a horror induced by the association of ideas that has "branded the occult with indelible scars." That this atavistic fear is 'contagious' in civilized society he illustrates by an experience of his own (p. 94). His treatment of the relations of magic to religion and to science, will interest the psychologist with anthropological leanings, his delineation of social forces issuing from magic will interest the student of social psychology who is now engrossed in the measurement of attitudes, and his illustrations of the potency of magic offer data to the student of hypnotism and psychopathology. The student of metapsychics will be interested especially in the introduction, beginning with a quotation from Bergson which sets the problem, and in Part III which contains the chapters on "Spiritualism challenged," "The dilemma of metapsychics," and "A false front." The dilemma is that metapsychics in seeking to divest itself of spiritism and the supernatural, and to confine itself to the abnormal, yields its place in the realm of science to psychopathology. Science, including experimental psychology, does not oppose the occult, but undermines it.

The book offers little to interest the experimental psychologist. It has no index, but contains an appendix of 14 pages of citations and notes.

Stanford University J. E. COOVER

Schule der Abstraktion und der Dialektik: Neue Wege begrifflichen Denkens. By LEO JORDAN. Munich, Ernst Reinhardt, 1932. Pp. 160.

The author attacks the philosophical concepts of abstraction and dialectics from a linguistic point of view. He finds that the development of the meaning of words in children and the various differences in shades of meaning of corresponding words in the Japanese and German languages do not substantiate the philosophical analysis.

Philosophy, for example, ignores the analysis of percepts. When a child uses a word, Jordan holds, this word refers to a characteristic trait which stands out in the percept. All objects having the same trait are referred to by the same class name. Later experience determines whether the first analysis will be satisfactory. A word, therefore, does not have reference to a common element of a class, but rather to any characteristic which happens to stand out in the perceptual experience. The experience of meaning, not the etymology of the word, must be the point of departure in any study of abstraction. Abstraction in the true sense is therefore not the isolation of a common element, but the experience of a relationship; and relationships cannot be perceived.

Dialectic is defined as double abstraction. "Good or bad," for example, contains two abstractions which appear to be opposites, but when used together they form a single new abstraction having a new meaning. Combining abstractions in this manner give us abstractions of a higher order.

Jordan's treatment is of philosophical rather than of psychological interest. It is divided into an analytic and a synthetic part, the method of approach being primarily epistomological.

University of Michigan N. R. F. MAIER

The Dramatic Personality of Jesus. By KNIGHT DUNLAP and ROBERT S. GILL. Baltimore, The Williams & Wilkins Co., 1933. Pp. vi, 186.

All the work of Jung, Rosanoff, Prince, Jaensch, Spranger, and the rest of our modern students of personality is here ignored. All of the scholars who have seriously argued that Jesus was psychopathic are brushed aside with the three words, "not a psychopath." There are good chapters on the racial and social backgrounds, the ethical teachings, the theology of Jesus, and, what is perhaps the best of all, a chapter on the Jesus of the gospels, in which the available evidences from the gospels and gospel sources are well brought together with references given. But such collections of Biblical references are not unique, and, unfortunately, the many others who have worked on these subjects are not mentioned. Several pages are given to the doctrine of the virgin birth, but the contribution of this doctrine to the personality pattern of Jesus, if any, is not made clear. Many traits are itemized and Jesus is said to have been well-adjusted to sex problems; but again these are nowhere brought together and used as indications of the kind of personality Jesus may have presented to his associates. The style indicates familiarity with the literature on the social backgrounds and the various problems concerning Jesus and his teachings, but only the names of Clay and Malinowski are actually mentioned and these appear without specific reference. There is no bibliography, and no index, and there are no footnote references to literature other than the Bible itself.

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REACTION-TIMES OF CHILDREN

By B. R. Philip, University of Alberta

There is ample evidence to show that certain innate abilities, such as intelligence and memory, improve with age. Studies such as those of Gesell[1] also indicate that some motor abilities develop even when there has been no training, so that maturation is a factor in learning. It would therefore seem of interest to determine the development with increasing age of a motor ability not previously learnt. As most motor abilities are utilized in some form or other almost constantly throughout life one would expect that there would be some transfer to the performance of a supposedly new motor task. The amount transferred in this fashion is hardly subject to experimental investigation: consequently one selects an ability which appears to involve to a minimum extent transfer from kindred habits already formed.

Such an ability seems to be that involved in reaction-time. Certainly there is little or no previous practice of reaction-time at least in the form used in the psychological laboratory. It is true that all throughout life individuals are constantly being called upon to act with the utmost celerity, but generally the act involves the greater part of the musculature and is not as definitely localized in a few muscles as is reaction-time. Moreover, reaction-time has certain advantages over most motor skills that render it suitable for investigation: it is adapted to the entire age range; its simplicity makes it ideal for younger children, yet the concentration required to act with maximum quickness challenges the most mature subjects; it can be easily and quickly measured in exact units.

As there are various stimuli to which one can react, and as reaction-time

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* Accepted for publication April 18, 1932.

[1] A. L. Gesell, Maturation and infant behavior pattern, *Psychol. Rev.*, 36, 1929, 307-319.

may or may not have a preparatory signal—types of reaction which seem to be closely similar—opportunity is thus offered to determine the correlation between cognate motor abilities. In addition there is the possibility of finding the relationship between intelligence such as measured by the ordinary verbal group tests and the motor ability of reaction-time. Finally the data collected for these purposes enable one to test on a more elaborate scale than has hitherto been possible the criterion devised by Woodrow to measure attention.[2]

The following specific objects were held in view in this investigation: (a) How does reaction-time vary with age in school children? (b) What is the relationship between various types of reaction-time? (c) How is intelligence related to reaction-time? (d) Does Woodrow's method of reaction-time difference measure attention?

PROCEDURE

Subjects. The *Ss* for this experiment were all the children from Grades IV to VIII inclusive of Grandin Separate School, Edmonton. To yield an adequate number of younger *Ss*, 16 girls and 20 boys from Grade III were included in the group and their reactions were taken in the month of April, some 5 mo. previous to their entrance into Grade IV. A few children over 16 yr. and 6 mo. were not included in the tabulations. A total of 311 *Ss*, 165 girls and 146 boys, took all the tests. They were distributed by age as follows:

Age	9 yr.	10 yr.	11 yr.	12 yr.	13 yr.	14 yr.	15 yr.	16 yr.	Total
Boys	22	22	22	19	18	22	14	7	146
Girls	20	18	25	31	28	25	12	6	165

Age 10, throughout this paper, means from 9 yr. 6 mo. to 10 yr. 6 mo.

The experimental work was begun in October, 1930, and continued till the end of April, 1931.

Apparatus. The apparatus used was a commercial set-up called a psychodometer, the timing element of which consists essentially of a Dunlap chronoscope synchronized by a 50 d.v. tuning fork. *S* sat comfortably before the reactor's board in a quiet room. Before him were the reactor's key—a telegraph sounder—and the light stimulus—a 6-volt flashlight bulb shining through a frosted window. Twenty feet away, so that the noise of the instruments would not disturb *S*, sat *E* before a table on which were the chronoscope, the tuning fork, the master key, and the stimulus-keys. *E* could see *S* from the side so as to be sure he was reacting properly; a precaution which generally was necessary only while the *S* was taking practice reactions.

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[2] This method has been used by the following investigators: H. Woodrow, The measurement of attention, *Psychol. Monog.*, 16, 1914, (no. 76), 1-158. About 20,000 reactions from 12 subjects—7 adults and 5 children. B. R. Philip, The measurement of attention, *Stud. Psychol. & Psychiat.*, 1, 1928, (no. 2), 1-65. About 7800 reactions from 61 college students. H. Easley, An attempt to isolate the factor of attention, this JOURNAL, 43, 1931, 202-215. About 6000 reactions from 68 college students. The present paper is a study of about 40,000 reactions from 311 children.

As the sounder was about 60 cm. away from the children it was not thought necessary to make any correction for velocity of sound. Neither was any correction made for delay in the light bulb coming to incandescence, as the lag in the bulb as measured on an oscillograph was approximately 2 σ, hence of the same approximate value as the time required for the sound to reach *S*. The lag in the instruments was not investigated; however, for instruments of this type it is small and fairly constant.

Method. All the reactions were taken in the afternoon from 1.30–4.00. The children came from their class in pairs. They were shown how to place their hand on the key, using the first and second digits only, while the forearm rested comfortably on the table, and how to lift their hand quickly when the sound or the light stimulus was perceived. The instructions were simple:

> Place your hand on this key as follows. I will say "Ready" as a warning. Shortly after the sounder will give the signal to act. When you hear the sound, lift your hand as quickly as you possibly can, and then place it on the key for another trial. Remember you must lift your hand the very instant you hear the sound.

The sound stimulus was then given several times so that the children could readily identify it, and from 10–12 reactions were taken but were not recorded. During this trial period *E* watched *S* so as to be sure that he was reacting properly. Now and then he would encourage him to react as quickly as possible. When certain that the reactions were being properly made the regular series was begun.

The reactions were taken in the following order: (1) sound with warning; (2) sound without warning; (3) light with warning; (4) light without warning. Thirty reactions of each kind were recorded; they were divided into 3 groups of 10 each. A short rest of about ½ min. was given between each group of 10 reactions, while *E* checked his tenth reading and reset the dial of the chronoscope to zero. During this period the child was told to take his hand off the key and to rest.

When the first group, the reactions to sound with warning, had been taken the second child who had been watching his companion had his reactions to sound with warning taken; meanwhile the first *S* rested. Thus they alternately reacted and rested. The rest periods were quite important as the children easily became fatigued. They had a tendency to cramp their hand too tightly on the key despite the warnings of *E*, and some few when questioned complained of cramped fingers. Even though each child had a short rest of about 30 sec. after every 10 reactions and a rest of from 5–10 min. between different groups of reactions, a slight fatigue effect was apparent which will be referred to later.

Two experimenters took all the reactions, the writer and Mr. Aiello, a graduate student in psychology. Both *Es* had practiced together for 6 experimental periods so that their methods of giving the reactions were very similar and they had learned to read the movements of the dial to interpret any unusual reaction. For all practical purposes their technique was identical.

The instructions when no warning signal was given were:

> This time I will not say "Ready"—you must be ready all the time. Remember as soon as you hear the sound [see the light] lift your hand as quickly as you possibly can from the key.

Before every group of reactions the instructions were carefully explained, and a practice series of from 10–12 reactions were given. No *S* was allowed to proceed till he had performed his task thoroughly well. In general there was little trouble except among some of the younger children who were prone to react prematurely. A premature reaction either did not record owing to the wiring arrangement, or if the reaction closely synchronized with the stimulus to react a peculiar floating effect of the dial indicator was observed. In the event of an incorrect reaction or one that was obviously improperly made no record was taken.

The warning signal for both sound and light was the quietly spoken command "Ready" preceding the stimulus from 1–2 sec. Care was taken to vary the warning interval within these limits. When the reactions with warning were given the reactions were taken almost as fast as they could be read and recorded, except for the rest between each group of 10 reactions. When no warning signal was given the reactions were taken much more slowly, the fastest one being given about 6 seconds after the preceding one, and the slowest ones about 20 seconds after the preceding one; the average interval for the reactions was about 10 seconds. In the no warning group care was taken to vary the reaction interval as much as possible.

Variables. The following variables were chosen: sound with warning (SW), sound unprepared, *i.e.* without warning (SU), light with warning (LW), light without warning (LU), the difference between the first pair, called sound difference (SD), and the difference between the second pair, called light difference (LD). The last two variables were to be compared with Woodrow's criterion of attention, which is the reciprocal of the difference between the reactions with warning, and those without warning. Scores on the Otis Intelligence Test were also tabulated (MA).

RESULTS

Practice and fatigue. To ascertain the effects of practice and fatigue the results of the first two classes which took the reactions were used. There were in this group 82 *Ss* (36 boys and 46 girls) all of Grade V standing except 10 boys and 10 girls from Grade VI. The average age of the boys was 11 yr. and 4 mo.; of the girls, 11 yr. and 6 mo. The reactions of each of the 4 types were divided into 3 groups of 10; thus the average of the first 10, the middle 10, and the last 10 for each *S* were separately recorded and averaged. A comparison of the 3 average values would enable one to tell whether practice or fatigue had most effect on the score. By practice is here meant any change in *S*'s reactions which would tend to diminish the reaction-time; by fatigue, any change in *S*'s reaction which would tend to increase his reaction-time. The results are given in Table I; the reactions for boys and for girls are separately tabulated, and the values for all the children are given.

A survey of the table shows that the fatigue effect is greatest in the cases of sound and light with warning, there being a retardation of 11.0 σ and

12.1 σ respectively in the base of the boys, and 8.6 σ and 7.6 σ in the case of the girls, when the averages of the fastest and the slowest groups of 10 are taken. The retardations for sound and light without warning, both for the boys and the girls, are practically the same and amount to about 6 or 7 σ.

TABLE I

Average Reaction-Time in Sigma for Boys and Girls and for All Ss for the Different Groups of 10 Reactions, Together with the Group-Variation from the Total Averages

Condition	Group	Boys (N = 36)		Girls (N = 46)		Total (N = 82)	
		σ	Var.	σ	Var.	σ	Var.
Sound with warning	1st. 10	171.3	−6.8	177.0	−6.2	177.44	−3.46
	2nd. 10	180.5	2.4	187.0	3.8	184.18	3.28
	3rd. 10	182.3	4.2	185.6	2.4	184.34	3.44
	All	178.1	*(11.0)	183.2	(8.6)	180.90	(6.90)
Sound without warning	1st. 10	199.3	−2.5	202.6	.3	201.16	− .87
	2nd. 10	200.3	−1.5	199.5	−2.8	199.88	−2.15
	3rd. 10	205.2	3.4	205.3	3.1	205.27	3.24
	All	201.8	*(5.9)	202.3	(5.9)	202.03	(5.39)
Light with warning	1st. 10	190.5	−8.4	184.8	−3.5	187.26	−5.66
	2nd. 10	203.6	4.7	187.8	− .5	194.74	1.82
	3rd. 10	202.6	3.7	192.4	4.1	196.84	3.92
	All	198.9	*(12.1)	188.3	(7.6)	192.92	(9.58)
Light without warning	1st. 10	237.8	−3.8	229.1	−4.6	232.98	−4.19
	2nd. 10	242.7	1.1	235.9	2.2	238.95	1.78
	3rd. 10	244.3	2.7	235.9	2.2	239.61	2.44
	All	241.6	*(6.5)	233.7	(6.8)	237.17	(6.63)

* Numbers in brackets are the extreme ranges of average reactions in group.

Despite the obviously greater difficulty in maintaining attention when no warning signal is given and the interval is prolonged up to 20 sec., what we have been calling fatigue is not as manifest as in the case when a warning is given. Nevertheless the children showed greater signs of fatigue and when questioned admitted the unprepared reactions were more tiring than the warning ones. The explanation of this anomaly probably lies in the fact that the giving of a warning signal aroused a tendency to anticipate the stimulus, with a consequent unreadiness to react when the actual stimulus came. This anticipatory tendency is built up during the first 10 reactions and thus would be more manifest in the second and third 10's than in the first 10 of the group of 30 reactions.

As a rule the first group of 10 is the only one faster than the mean reaction of the 30; the exceptions are the sound without warning, SU for the boys, and sound without warning, SU, and light with warning, LW, for the girls. In these instances, possibly a tendency to perseveration or inadaptability to the new situation makes itself manifest. Thus in the case of the SU reactions, the *S*s were not thoroughly adapted to the absence of the preparatory signal and consequently their first 10 reactions were slower than they normally would have been. If such is the correct explanation in general the boys were better able than the girls to adapt themselves to the new situations. A comparison of the means for the boys and the girls gives us an indication of a finding that shall later be discussed more thoroughly. The boys of this group are some 5 σ faster for sound with warning than the

TABLE II

Mean Reaction-Time in Sigma

	Age	N	SW	SU	D	LW	LU	D
Boys	9	22	172.94	222.84	49.90	193.50	260.08	66.58
	10	22	172.20	210.86	38.66	186.38	235.94	49.56
	11	22	170.90	197.42	26.52	190.40	234.06	43.66
	12	19	167.28	196.66	29.38	189.82	231.32	41.50
	13	18	160.76	180.34	19.56	180.02	214.96	34.94
	14	22	156.18	174.66	18.50	173.36	203.96	30.60
	15	14	139.50	151.14	11.64	154.82	173.62	18.82
	16	6	136.12	148.46	12.34	145.58	169.66	24.06
Girls	9	20	180.96	215.00	34.02	195.06	241.72	46.66
	10	18	179.82	205.20	25.40	183.50	229.58	46.08
	11	25	167.42	211.98	34.56	192.67	241.68	48.98
	12	30	173.82	201.02	27.20	181.42	228.32	46.90
	13	28	162.72	194.24	31.54	176.72	224.58	48.03
	14	25	153.60	188.10	34.48	165.56	218.26	34.62
	15	12	150.42	181.40	31.16	162.20	212.16	50.00
	16	6	142.90	197.44	54.54	170.60	240.40	69.80
All	9	42	176.76	219.10	42.34	194.24	251.34	57.10
	10	40	175.64	208.32	32.68	185.08	233.08	48.00
	11	47	174.38	205.18	30.78	191.62	238.12	46.48
	12	49	171.28	199.32	28.06	184.68	229.50	44.82
	13	46	161.96	188.80	26.84	178.02	220.94	42.92
	14	47	154.76	181.80	27.00	169.22	211.14	42.22
	15	26	144.54	165.12	20.64	198.20	191.26	33.16
	16	12	139.52	172.96	33.44	158.04	205.04	46.94

TABLE III

Age Norms for Reaction-Time: Smoothed Means

Age	N	SW	SU	SD	LW	LU	LD
9	42	176.76	219.10	42.34	194.24	251.34	57.10
10	40	174.92	210.86	35.26	192.32	240.84	50.52
11	47	173.76	204.28	30.50	187.12	233.56	46.44
12	50	169.20	197.76	28.56	184.78	229.46	44.74
13	46	162.66	189.98	27.30	177.30	220.52	43.32
14	47	153.76	178.58	24.82	168.48	207.98	39.44
15	26	144.54	165.12	20.64	158.20	191.36	33.16

girls; the groups are approximately equal for SU; for both reactions to light the girls are almost 10 σ faster than the boys.

Age norms and statistical values. The age norms for the variables chosen are given for girls, boys, and all *S*s in Tables II and III. It is to be noted that there is a consistent increase in duration from sound and light with warning, to sound and light without warning. In general the tables show the following characteristics: the reaction-times of all the variables except SW show a perceptible drop at age 10 yr. followed by a rise at age 11 yr. This variation can only be accounted for on the basis of inadequate sam-

TABLE IV

Variation of Reaction-Time with Age as Percentages of the 12-Yr. Norms

Age	N	SW	SU	SD	LW	LU	LD
9	42	105.5	110.7	148.2	105.3	109.6	127.7
10	40	103.5	106.8	123.7	103.2	105.1	108.0
11	47	102.8	103.6	107.0	101.4	101.8	104.0
12	50	100.0	100.0	100.0	100.0	100.0	100.0
13	46	96.1	96.0	95.6	96.1	96.1	96.8
14	47	91.0	90.3	87.1	91.2	90.5	88.2
15	26	85.5	83.7	72.3	85.7	83.3	74.0

TABLE V

Mean Score: Percentage of 12 Yr. Norms

	Age	SW	SU	SD	LW	LU	LD
Boys	9	103.2	113.5	170.0	102.0	112.8	160.5
	10	103.0	107.2	131.7	98.2	102.0	119.5
	11	102.0	100.5	90.3	100.5	101.4	105.6
	12	100.0	100.0	100.0	100.0	100.0	100.0
	13	96.0	91.7	66.6	95.0	93.0	80.5
	14	93.4	89.0	63.1	91.5	88.1	70.5
	15	83.5	77.0	39.6	81.5	75.0	43.6
	16	81.5	75.6	42.0	76.8	73.3	55.4
Girls	9	105.1	107.1	125.1	107.3	105.7	99.4
	10	103.2	102.5	93.5	101.1	100.5	98.3
	11	102.0	105.9	127.0	106.1	105.6	104.4
	12	100.0	100.0	100.0	100.0	100.0	100.0
	13	93.6	97.0	116.0	97.3	99.3	102.2
	14	88.4	93.9	127.0	91.3	95.7	116.5
	15	88.6	90.0	114.3	89.5	92.6	106.7
	16	82.3	98.0	201.0	94.0	105.3	148.4
All	9	103.1	109.6	150.9	105.1	109.8	127.3
	10	102.8	104.2	116.4	100.2	101.8	107.1
	11	101.8	102.6	109.5	104.0	104.0	104.0
	12	100.0	100.0	100.0	100.0	100.0	100.0
	13	94.6	94.4	95.7	96.7	96.6	95.7
	14	90.3	90.9	96.2	91.7	92.2	94.1
	15	84.4	82.6	73.5	85.8	83.3	73.9
	16	81.5	86.5	119.2	85.6	89.5	104.6

TABLE VI

Reliability of the Differences

	Girls			Boys						
	Mean	S.D.dis.	S.D.M	Mean	S.D.dis.	S.D.M	Diff.	S.D.diff.	D/S.D.diff.	Chances in 100
SW	168.28	26.01	2.03	162.60	23.96	1.98	5.68	2.83	2.00	98.0
SU	201.20	30.94	2.42	191.10	35.96	2.96	10.10	3.83	2.64	99.6
SD	32.33	25.08	1.95	28.42	26.52	2.13	3.92	2.89	1.35	91.0
LW	180.01	26.66	2.08	180.96	23.76	1.97	−0.96	2.86	−0.34	64.0
LU	228.88	32.48	2.53	222.40	40.84	3.39	6.50	4.23	1.54	93.4
LD	49.78	24.78	1.93	41.38	29.93	2.47	8.40	3.14	2.68	99.7
MA	34.92	16.00	1.25	34.05	15.65	1.30	0.87	1.80	0.48	68.0

TABLE VII

Effect of Adolescence

		Age	SW	SU	SD	LW	LU	LD	MA
Girls	Adolescents	14.11	154.55	197.56	43.01	173.22	232.01	58.79	44.9
	Non-adol.	13.8	150.20	182.49	32.28	163.19	211.86	48.66	51.4
	Diff.	1.3	4.35	15.07	10.73	10.03	20.15	10.13	−6.5
Boys	Adolescents	14.8	145.28	158.68	13.38	159.56	181.58	22.02	49.24
	Non-adol.	13.6	146.71	159.84	13.13	164.31	186.51	22.20	49.28
	Diff.	1.2	−1.43	−1.16	0.25	−4.75	−4.93	−0.18	−0.04

pling. A second characteristic is the rather rapid rise for the girls at 15 yr. of age. It will be shown later that this is probably accounted for by the retardation of reaction-time for some girls at this period due to adolescence. This effect is not noted in boys at the corresponding age.

Possibly the varying rate of reaction-time (RT) can be seen most clearly by calculating the percentages of all the age norms in terms of the 12-yr. means. They are given in Tables IV and V. Reaction-time differences for sound and light show the greatest deceleration, then sound and light without warning, and least of all, sound and light with warning. For SW and LW the deceleration amounts to about 5% from year 9 to year 12, and from year 12 to year 15 it amounts to about 14%. Thus the deceleration in the 3-yr. period 9-12 is only about one third of the deceleration in the following 3-yr. period. Sound and light without warning (SU and LU) show decelerations of about 10% and 16% in these two triennial periods; while SD and LD are more closely alike in the two triennial periods and amount to about 30%.

TABLE VIII

CORRELATION TABLES

		SW	SU	SD	LW	LU	LD	MA
	SU	.625						
	SD	−.243	.597					
Girls	LW	.700	.644	.101				
(N=165)	LU	.523	.796	.468	.655			
	LD	−.078	.418	.524	−.188	.604		
	MA	.115	.251	.008	.236	.196	.011	
	CA	−.448	−.297	.095	−.376	−.206	.177	.523
	SU	.734						
	SD	.056	.738					
Boys	LW	.735	.680	.178				
(N=146)	LU	.629	.834	.577	.680			
	LD	.224	.590	.618	.028	.787		
	MA	.348	.364	.178	.415	.356	.228	
	CA	−.451	−.610	−.408	−.562	−.622	−.420	.605
	SU	.684						
	SD	−.095	.669					
All	LW	.706	.642	.134				
N=311	LU	.569	.817	.527	.656			
	LD	.087	.525	.575	−.088	.711		
	MA	.214	.300	.088	.332	.274	.113	
	CA	−.438	−.463	−.160	−.457	−.435	−.154	.584

An item of no little interest is the fact that the decelerations for sound and light are very closely the same whether one considers the reactions with preparatory signal or without preparatory signal, and there is but slight variation in the case of RT differences for sound and light. It is evident that the psychological factors involved are closely similar when sound and light are the stimuli, but that they vary with the mode of giving the stimuli. Corroboration of this fact is found in Table VIII, which gives the correlation of MA with each of the variables mentioned. The values for sound and light are approximately the same though this value common to sound and to light differs with the mode of giving the stimuli and with sex.

Frequency distributions of the variables. Table XI gives the constants of the frequency distributions for all the variables. The curves were plotted but are not published here. The means and the standard deviations are tabulated as are the constants of the normal curves of best fit. Several facts are worthy of note: there is a tendency for the boys' curves to be flattened, with a consequency greater standard deviation, and the contrary tendency for the girls' distributions to be more frequent at the mean. The boys' curves are skewed positively, that is with increased frequencies toward the smaller scores; hence the boys' scores are in general lower than those of the girls. Later we shall see that there is for some of the variables a statistically significant difference between the sexes. Some of the distributions approximate the normal curve of error so closely as to enable one to use with some degree of confidence statistical devices based upon the probability curve. The curves for light are particularly close to the normal curve. The curves for sound have a tendency to bi-modality due to the significantly faster times of the boys. Some of the values of the RT difference curves, both for sound and for light, are negative. An explanation of these negative values in terms of Woodrow's theory of detractors would be difficult; this will be discussed in another section.

TABLE IX

Decrease in Reaction-Time from 9–15 Yr. of Age

Sex	Age	N	SW	SU	SD	LW	LU	LD
	9	22	172.94	222.84	49.90	193.50	260.08	66.58
Boys	15	14	139.50	151.14	11.64	154.82	173.62	18.82
	Diff.		33.44	71.70	38.26	38.68	86.46	47.76
	9	20	180.96	215.00	34.02	195.06	241.72	46.66
Girls	15	12	150.42	181.40	31.16	162.20	212.16	50.00
	Diff.		30.54	33.60	2.86	32.86	29.56	−3.34

Sex differences. Table VI gives the reliability of the differences between the girls and the boys for the variables. It will be seen that there is a slight, though fairly significant difference between sexes for all the variables except LW and MA. The significance is expressed in terms of the number of chances in 100 that the difference is a true difference. The difference is always in favor of the boys, that is they react faster than do the girls; it is of the order of 3-5% to reactions with and without warning, 20% for RT differences to light (LD), and 14% for SD. These sex differences are calculated from all the scores of the boys and of the girls. In Table I, which gives for Grade V the effects of fatigue and practice, the boys are 2.8% faster than the girls in the case of SW; there is little or no difference for SU. For both the light reactions the girls are faster than the boys, 5.2% for LW and 3.3% for LU. As will be seen in the next section adolescent girls tend to slow up in their reactions though the boys do not; hence when all the children are considered the boys are definitely faster than the girls except for LW.

The differences are so slight that they have hitherto been but little noticed, due to the masking effect of individual variability. They need not necessarily be explained as due to innate sex differences; it may be that the motor training of boys, particularilly from play, tends to develop faster reactions to sound than that of the girls whose games do not have that tendency. A similar explanation would account for the fact that the younger girls are faster in light reactions than are the boys. Such an explanation is of course but tentative and conjectural.[8]

Reaction-time and maturity. Reaction-time being a special type of skill in which the children are unpracticed, all improvement from year to year, save that obtained by transfer from motor activity generally, is the result of maturity. The decrease of RT with age gives us definite evidence that the increased coördination brought about by time is not confined to infancy and to the early years of childhood. It would have been a matter of some interest to obtain the variation of RT with age up to full maturity. As this study was confined to grade children too few *S*s were available beyond 15 yr. of age. Growth in this ability shows no sign of diminution up to that period, as the yearly percentage decrements were increasing from ages 12–15 yr.

There is however a most interesting effect to be noticed at the older ages studied which shows for girls but not for boys a definite slowing up of reaction-time. Table VII brings this out clearly. The tabulations were obtained as follows. The children in Grades VII and VIII were divided into adolescents and non-adolescents by their teachers. In the group chosen there were 20 adolescent girls and 29 adolescent boys, while 38 girls and 18 boys were non-adolescent. The criterion used to determine adolescence was a subjective one, and to that extent the inferences based on the data are invalidated, but as a first approximation the criterion was the best that could be obtained in the circumstances. Though the adolescent girls were on the average 1 yr. 3 mo. older than the non-adolescent girls and hence should have quicker reactions there is a definite retardation for them for every type of reaction varying from 3% in the case of SW to 9% for LU, and from 17% for LD to 25% for SD. The reactions of the adolescent boys are slightly faster than those of the non-adolescent. One would expect this difference to be due chiefly to the fact that the adolescent boys are on the average 1 yr. 2 mo. older than the rest of the group.

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[8] The slowing up of the adolescent girls' reactions which is discussed in the next section, increases the mean scores of the girls. This factor would, of course, account for some of the difference but not for all.

Had a study been made of High School students, where larger numbers of adolescents would be available and where the age range includes *S*s at various stages of adolescence, and had objective criteria of adolescence been used, one would be in a better position to state whether the tendency here indicated had a factual basis or be but the chance result of insufficient data and uncertain criteria.

Correlation of abilities. An inspection of Table VIII reveals the fact that the correlations may be roughly divided into two classes: those that are positive and quite definite, ranging from 0.42 to 0.83, and those that are negative and weak, though they may still be significant. In this latter group are the four correlations SD-LW, SW-LD, LW-LD, SW-SD, which show the relationship between reactions with warning and reaction-time difference. Evidently these abilities are not closely allied—which is pretty much what one would expect for the one implies ability to release nervous energy with facility, while the other implies ability to hold it at constant intensity during small time intervals.

When one compares the correlations for the girls with those of the boys it is found that the correlations of the girls range from —0.243 to 0.796, while those of the boys range from 0.028 to 0.834. Hence there is not a variable to variable parity between the correlations, those of the girls are characterized by a wider range and by negative values in some cases. When, however, the correlations of the girls and those of the boys are tabulated for the same variables, and themselves correlated, the Pearson r found is quite high, ($r = 0.963 \pm .01$). This result is based upon 21 variables including those of MA.[4] If we leave out the intelligence variables the value of r still remains about 0.96. Thus while the individual RT of an *S* shows a variability which is more than 10% of the mean, and while the standard deviations of the distributions of those means are themselves rather high, yet the functions we are considering are so related that a very definite agreement exists between the two sets of correlations.

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[4] The sign of the MA correlations was changed to obtain values for the regression line, $Y = 1.163\ x - 2.06$. The change of sign does not alter the conclusions, as the amount, not the direction, of the correlations was required.

It is to be noted that the girls' correlations are not as high as those of the boys. If we disregard sign, which indicates the direction of the relationship not its amount, the average correlation, excluding those with MA, is, for the girls 0.421, and for the boys 0.501. A partial explanation for this fact is that the girls are not as variable as are the boys and tend to the mean more than do the boys; also the effect of adolescence on the girls and not on the boys is to limit quicker reactions, that is to cut down the relationship at one end of the distribution.

If we examine the mean scores for the boys and girls we find from ages 9–15 yr. a decrease in times of unprepared reactions to sound and to light, roughly twice those of the warning reactions, in the case of the boys, but of equal amount for girls. This is seen in Table IX. The steady decrease with age of SU and LU as compared with SW and LW for the boys, whereas for girls the differences in these values remain more or less unchanged with advancing age, would tend to make the correlations SU-SD and LU-LD relatively high for boys and not for girls. This is what we find in the correlation table. If the correlations for girls are arranged from highest to lowest and likewise those of the boys are arranged from highest to lowest, most of the variables remain in the same order, or suffer a displacement of one rank only. The correlations SU-SD and LU-LD change from ranks 6 and 7 for the girls to ranks 2 and 3 for the boys and gain in value 0.183 and 0.190 respectively.

There are three negative values of correlations for the girls—SW-SD, SW-LD, and LW-LD. A probable explanation of the negative sign is that in general the girls who react fastest to SW and LW have an SU and LU about average—hence their SD and LD would be greater than average. The boys on the other hand who react fastest to SW and LW have also fast SU and LU reactions and hence also low SD and LD.

Reaction-time and chronological age. As was to be expected there is quite a definite correlation with age for most of the abilities. Moreover the correlation ratios obtained for girls differ in some cases from that of the boys, and in general they are considerably lower; in two instances, SD and LD, the sign is changed. The correlations of CA of both boys and girls for SW are very closely the same (—0.451 and —0.448), while MA with CA is 0.605 and 0.523 respectively. The other correlations are considerably lower for girls than the corresponding values for boys. On the basis of what was said in the previous section this is quite understandable. What is not so evident is the equality between the two SW correlations when practically every other one is lower for the girls than for the boys. One possible explanation would be that SW reactions having been taken first, the element of novelty predominates, and the difficulty of the task is the same for both sexes, only in the succeeding reactions does any outstanding sex difference show itself. It is true than even in SW there is a sex difference —the boys' reactions are consistently faster than the girls.

These considerations indicated that it might be of advantage to partial out the effect of age in the various correlations listed. These values are to be found in Table X, the values of which follow very closely as far as rank is concerned those of Table VIII, in which the first order correlations are given. The new values are, however, all smaller for boys, but are more or less the same for girls. Thus the average correlation for the 11 significant positive values changes for boys, when age is partialled out, from 0.691 to 0.586; for girls the corresponding change is from 0.596 to 0.599. Moreover in Table X there is little or no difference between the correlations of the girls and the corresponding ones of the boys.

TABLE X

CORRELATIONS WITH CHRONOLOGICAL AGE CONSTANT

		SW	SU	SD	LW	LU	LD
Girls (N=165)	SU	.577					
	SD	−.225	.658				
	LW	.645	.602	.149			
	LU	.494	.788	.501	.640		
	LD	.000	.501	.518	−.133	.665	
	MA	.156	−.118	−.074	−.045	−.105	−.123
Boys (N=146)	SU	.651					
	SD	−.157	.678				
	LW	.655	.516	−.069			
	LU	.500	.735	.453	.510		
	LD	.043	.465	.539	−.277	.741	
	MA	−.116	.008	.095	−.114	.029	.036
All (N=311)	SU	.604					
	SD	−.186	.680				
	LW	.634	.545	.070			
	LU	.467	.771	.520	.572		
	LD	.021	.519	.565	−.180	.724	
	MA	.058	−.042	.010	−.090	−.027	−.028

When the correlation values, with age constant, of the girls are in turn correlated with those of the boys the r obtained is $0.934 \pm .017$, and the regression line is $Y = 0.977\ x + .006$. When this regression equation is compared with that previously obtained from the first order correlations ($Y = 1.163\ x - 2.06$), it is quite evident that the linear relationship which holds between the raw correlation values of the girls and those of the boys becomes very close to equality when age is held constant.

A study of Table X shows that the correlations of all the *Ss* may be divided into several groups.

(a) Correlations that are valid. Positive and statistically significant correlations were obtained which range from 15–50 times their probable error. They are:

SU-LU	.771	SW-LU	.467
SW-LW	.634	SW-SU	.604
SU-LW	.545	LW-LU	.572

It is to be noted that the correlation for SU-LU is the highest of all and the next highest is SW-LW.

(b) Correlations that are spurious. As the RT differences are obtained by subtracting the warning reactions from the unprepared ones from the same kind of stimuli a spuriously high correlation is obtained.[5] These correlations are SU-SD 0.680 and LU-LD 0.724 in which the spurious element is undoubtedly high. Correlations SU-LD 0.519 and SD-LD 0.565 also contain a spurious element, though in these cases it is an indirect one, as the LD values are partly derived from LU which correlates highly with SU; in the same manner SD-LU 0.520 is partly spurious.

(c) Correlations that are not significant. This group includes all the correlations with MA none of which are much greater than twice their probable error. Also are included the correlations SW-LD 0.021 and SD-LW 0.070.

(d) Correlations that are negative and significant. SW-SD —0.186 and LW-LD —0.180 are about 5 times their probable error. Their spurious element is relatively low and would tend in the opposite direction. The negative sign implies that the higher the RT to warning the lower the RT difference, which is evidently true. A comparison with the values SU-SD 0.680 and LU-LD 0.724 shows that the RT differences are more dependent upon the SU and LU reactions than they are upon the SW and LW reactions, a fact which has already been gleaned from other considerations.

Reaction-time and mental age. While the reaction-time experiment was in progress the *Ss* were all given the Otis Self Administering Test of Intelligence. Form A Intermediate was first given. Those *Ss* who were absent and those who in the opinion of their teachers obtained scores on the first test that for some reason or other were widely different from what these children should have obtained were given form B of the Intermediate examination; the score they made on it was taken as their final score.

In the cases where the Intelligence Test was given some months after reaction-time experiments the scores on the Intelligence Tests were worked out from the tables given by the publishers to an equivalent value for the age at the time of taking the other tests.

It was felt that the Mental Age scores for some of the children in Grade IV were not fair measures of their intelligence, due in some cases to a language handicap, and in others, to the deficiency of the test itself at that low grade. The number of children affected was so small (about 10) a truer result could probably not have been obtained by giving them some other intelligence test and attempting to equate

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[5] Cf. H. E. Garrett, *Statistics in Psychology and Education,* 1926, 261.

TABLE XI

Frequency Distributions of Reactions to Light and to Sound: Constants of the Fitting of These Curves to Normal (Gaussian) Curves

The frequency distributions are based upon the mean reaction-times, measured in sigma, of 311 children. Every *S* gave 30 reactions to sound preceded by a warning, SW; to sound when no warning was given, SU; and to the difference between these two measures for sound, SD. The three values for light, LW, LU, and LD, were obtained in the same manner.

	SW	SU	SD	LW	LU	LD
Mean	165.7	195.9	30.4	180.3	226.0	45.8
S.D.$_{\text{dis.}}$	25.6	35.6	25.8	26.4	36.6	27.5
S.D.M.	1.45	2.02	1.47	1.50	3.66	1.56
β_1 (Skewness)	.154	.082	.433	.138	.078	.133
β_2 (Kurtosis)	3.11	2.77	2.64	3.02	3.64	2.97
†Pearson's Goodness of Fit χ^2	13.92	4.99	33.28	2.71	5.65	9.22
No. intervals	7	8	11	7	9	11
Pearson's P	.031	.702	.001	.842	.686	.512

† Using a Pearson Type III curve, the closeness of fit was considerably improved. Thus for LD, the new values obtained are: (for Skewness = 0.4) $\chi^2 = 2.80$; and P = 0.94.

their scores with those obtained in the Otis test. Accordingly their scores were grouped as found with those of the other children. The actual scores made on the test, corrected in some cases as just stated, were used for the various correlations; they are equivalent to a MA score.

When the correlations between Mental Age and the various reaction-time tests were tabulated for the girls and boys separately and for all *S*s together the correlations were all negative, and ranged for the girls from zero in the case of SD and LD to —0.251 for SU, which is almost 5 times its probable error. For the boys the correlations, all negative, are of greater value, and all are significant. The largest correlations are LW and SU, as in the case of the girls; they are —0.364 and —0.415.

When chronological age is held constant in the correlations between MA and RT a very different result is obtained. All the correlations are within 3 times their probable error except SW-MA for girls, which is 0.156, a value slightly less than 5 times its probable error. It is to be noted that it is positive, while the tendency of all the other larger correlations for girls and for boys is to be negative. It is doubtful if any significance can be attached to this one value. In general the trend of the SW values, particularly for girls, seems to indicate the presence of another factor, possibly novelty or inadaptation to the experiment, which does not hold in other cases.

One may state then quite definitely that there is no significant correlation between MA and RT when CA is held constant.[6]

Reaction-time and attention. Reaction-time has often been used as a measure of attention. For this purpose there have sometimes been used the rapidity of the simple reaction, sometimes the mean variation of a series of reactions, and sometimes a special method devised by Woodrow and based upon the reciprocal of reaction-time difference. A comparison of these three methods has been discussed elsewhere by the writer and by Easley, who fails to find a common factor of attention in the various tests proposed.[7] It might be apropos to discuss this question in the light of the findings of this investigation.

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[6] Easley, *op. cit.*, gives the following correlations: LW-MA — 0.01; LD-MA — 0.00; LW-LD — 0.03.

A study of Table X, giving the correlations of the various factors with CA constant yields several facts which can hardly be interpreted if the reciprocal of RT difference is taken as being a measure of the attention involved. The correlations between SD and LD are, for girls 0.518, for boys 0.539, and for the total group 0.565. These values are considerably lower than the correlations SU-SD and LU-LD for the same groups, they being close to 0.7. If the SD and LD both measure attention it is difficult to see how they would fail to correlate at least as well with themselves as with measures from which they are derived.

The correlation of RT difference either for sound or for light with MA is about zero. Yet evidently attention is involved in intelligence, in fact some are inclined to identify it with intelligence.[8] Must one be satisfied with the explanation that the intelligence tests used involve a different kind of intelligence and hence no correlation between RT difference and MA would be found?

The correlations between SD-SW and LD-LW are low but negative, which seems to imply that the faster an individual reacts the less his power of attention—an absurd conclusion, of course.

The correlations between CA and RT difference are so low as to be insignificant for girls, but are 9 times their probable error for boys. We hardly expect a sex difference as great as this for attention when none has been found for intelligence, memory, etc.

Finally a study of the RT difference frequency distribution curves, discloses several negative SD and LD (8% of the SD and 3% of the LD). If the reciprocal of the RT differences is a measure of attention, these negative values have no meaning. They may however be explained as due to a lack of adaptation to the experiment, and the greater frequency of their occurrence for sound which was taken first confirms this. There is another possible explanation. Individuals need a warning period for their fastest reactions; a preparatory signal given for a shorter or a longer period than the optimum interval yields a slower reaction. Just as one finds quite an individual variability in the length of the reaction, so one finds a large variability in the length of the optimum interval. There is then the possibility that for those individuals with a negative RT difference the interval chosen (1–2 sec.) was quite unsuitable; their optimum interval probably being much longer.

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[7] *Op. cit.*

[8] Cf. C. E. Spearman, *Abilities of Man*, 1927, 88.

One is hence inclined to conclude that the reciprocal of the RT difference is not a satisfactory measure of attention.

SUMMARY

(1) Tentative reaction-time norms to sound and to light have been secured from 165 girls and 146 boys from ages 9 to 16 yr. These norms are based upon 120 reactions from every *S*.

(2) Constants of the curves of the frequency distributions, means and standard deviations are given.

(3) The values of SW, LW, SU, and LU show a steady drop with increasing age, and maintain this order, SW being the fastest reaction.

(4) Fatigue is slightly more apparent than practice in a series of 30 reactions.

(5) The psychological factors involved in reactions to sound and to light are closely similar, but they vary with the mode of giving the stimulus and with sex.

(6) There is a statistically reliable difference between girls and boys for all types of RT except LW. The boys react from 3–5% faster than do the girls.

(7) This difference is probably due to transfer from the greater play activity of boys.

(8) Adolescent girls show a definite retardation of reaction-time which is not noticeable in adolescent boys.

(9) Tables of first order correlations, and correlations with chronological age constant are given for girls, for boys, and for all *S*s.

(10) There is a linear relationship between the correlations of the girls and the corresponding ones of the boys; this relationship approaches equality when CA is held constant.

(11) The correlations from these tables are of several types: those that are (a) valid, positive, and statistically significant; (b) spurious, in whole or part; (c) non-significant; and (d) negative and significant.

(12) There is no correlation between MA and RT.

(13) Woodrow's method of measuring attention by the reciprocal of the RT difference is probably not valid.

THE VOLUME AND INTENSITY OF TONES

By S. S. STEVENS, Harvard University

Experimentation on the problem of tonal volume has tended to yield convincing evidence that pure tones are characterized phenomenally by what has variously been called 'largeness,' 'massiveness,' or 'impressiveness.' Unfortunately, however, the quantitative results obtained by different experimenters have proved less consistent than the introspective results. Each experimenter who has undertaken the task has succeeded in training *Os* to make discriminations of volume with a fair degree of subjective certainty, but the magnitudes of the volumic limens, as determined in various laboratories, have nevertheless varied considerably.

Rich[1] made the first determinations of the volumic limen in 1916. He used the method of constant stimuli to measure the limen for volume in terms of the frequency of the stimulus and found it to be significantly larger than the limen for pitch. In 1919 Rich[2] repeated his experiment and was satisfied that he had confirmed his earlier results, although in this experiment the volumic limens turned out to be smaller in magnitude than those previously obtained. Then, in 1930 Gundlach and Bentley,[3] using the method of constant stimuli, obtained limens for volume which were much smaller than those of either of Rich's determinations and which in some cases approximated the limens for pitch. The variability among the *Os* of this experiment was such as to render definite conclusions hazardous, but, as the authors pointed out, the evidence obtained from this study cast doubt upon the existence of volume as an independent attribute of tones. Recently Zoll[4] has determined volumic limens by Rich's method and found them to be intermediate in size between those reported by Rich and those reported by Gundlach and Bentley.

In addition to the investigation of the relation of volume to pitch, the same method (constant stimuli) has been used to study its relation to phase-differences. Halverson[5] obtained evidence of a peculiar relationship between volume and phase-difference, but the nature of the function was not verified by Gundlach and Bentley.[6] As a matter of fact, Halverson believed that the function which he had obtained

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[1] G. J. Rich, A preliminary study of tonal volume, *J. Exper. Psychol.*, 1, 1916, 13-22.

[2] Rich, A study of tonal attributes, this JOURNAL, 30, 1919, 121-164.

[3] R. Gundlach and M. Bentley, The dependence of tonal attributes upon phase, this JOURNAL, 42, 1930, 519-543.

[4] P. M. Zoll, The relation of tonal volume, intensity, and pitch, this JOURNAL, 46, 1934, 99-106.

[5] H. M. Halverson, Diotic tonal volumes as a function of differences of phase, this JOURNAL, 33, 1922, 526-534.

[6] Gundlach and Bentley, *op. cit.*

was not due entirely to phase-difference in the two ears, but partly, at least, to intensitive differences in the stimulus.

In 1924 Halverson[7] verified his suggestion that volume is dependent on intensity. His *Os*, trained to recognize volume by using frequency as a variable (Rich's criterion), were able to give consistent judgments of volume when the intensity of the tones was varied by changing the amount of resistance in the secondary of an oscillator circuit. Introspections were obtained which tended to confirm those given by Rich's *Os*. One is forced, however, to question the significance of the quantitative results, because the general trend of the intensitive limens, as Halverson determined them, is in marked disagreement with the results of the later studies of Riesz,[8] Knudsen[9] and others. These experimenters found that the size of the relative limen for intensitive discrimination tends to increase as the sensation level decreases. Halverson's results imply an opposite relationship.

Zoll[10] tried to reproduce Halverson's results using his method, but failed. In fact, Zoll's final conclusion is that, owing to the instability of volume under experimental conditions, its phenomenal existence as an independent attribute of tones should be seriously questioned.

That, very briefly, is the story of tonal volume. We have, on the one hand, adequate introspective evidence that volume is phenomenally a separate and distinct attribute of tonal stimuli; but, on the other hand, experiments in which volume has been used as the basis of judgment have not yielded stable quantitative results. Consequently, there remain grounds for the divergence of opinion which exists among theorists as to the existence, the nature, and the possible physiological basis of the volumic attribute.

This state of affairs is due—so the present experiment indicates—to two principal causes: the failure of experimenters adequately to control the physical stimulus, and the use of the method of constant stimuli.

Sabine[11] cautioned those who use sound stimuli that they must "take account of three factors: the effect of reflection by the walls on the increase in the total intensity of sound in the room; the effect of interference in greatly altering the distribution of this intensity; and the effect of the reaction of the sound vibrations in a room upon the source itself." In other words, the neglect of factors affecting the physical behavior of sound waves is likely to vitiate the results of an auditory experiment. Because of the absence of evidence to the contrary, one is forced to assume that in the experiments on tonal volume no serious account was taken of these disturbing physical factors.

With regard to the second reason for psychologists' failure to obtain convincing quantitative results, it now appears that the application of the method of constant stimuli to the problem was not a wise procedure. There are three reasons why volume is difficult to judge. (1) It is a relatively unfamiliar aspect of tonal stimuli: people are not taught from early youth to distinguish it, as they are taught to distinguish pitch and loudness. Consequently, the training of the *O* becomes important. (2) The *O*'s criteria for volume, when once formed, are very instable. They are apt to change in the middle of a series of observations. (3) When two tones are sufficiently unlike in frequency for them to be judged different in volume, the difference in pitch is necessarily larger and more conspicuous than the volumic difference and tends to obscure the latter. These three difficulties render the method of constant stimuli especially unsuitable, because this method provides only for the presentation of tones which differ but slightly in volume and which are heard by the *O* for but a short interval of time. The *O* has no opportunity to manipulate the tones himself, to assure himself as to his criteria by examining large differences in volume, to interrupt a series in order to reaffirm his criteria in case they become uncertain. All this confusion and uncertainty may, as Boring has pointed out,[12] lead to a fourth difficulty: the *O*, without realizing what he is doing, may abandon the judgment of the comparison stimulus with reference to the standard, and may give 'absolute judgments' in which the comparison stimulus is referred, not to the standard, but to the general level of the set of comparison stimuli in use.

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[7] Halverson, Tonal volume as a function of intensity, this JOURNAL, 35, 1924, 360-367.

[8] R. R. Riesz, Differential intensity sensitivity of the ear for pure tones, *Phys. Rev.*, 2nd ser., 31, 1928, 867-875.

[9] V. O. Knudsen, Sensibility of the ear to small differences of intensity and frequency, *ibid.*, 2nd ser., 21, 1923, 84-103.

[10] Zoll, *op. cit.*

[11] W. C. Sabine, *Collected Papers on Acoustics*, 1922, 277 ff.

In order to overcome the difficulties inherent in the judgment of volume, the present study had recourse to the method of average error, under which the *O* makes what may be called a 'transdimensional equation.' The advantage of this method is that it puts volume, so to speak, into the hands of the *O* and gives him a chance to vary and examine it. He can manipulate the tone at will. He can make certain of his criteria by observing large differences in volume; and he can interrupt a series to reassure himself of his criteria whenever they begin to fail. Moreover, this method lessens the chances that volume will be confused with pitch or loudness, and eliminates the tendency for the judgments to become 'absolute.' It is unfortunate that the attempt of Gundlach[13] to use this method should have failed for the reason that, as I shall try to show, his stimuli were wrongly chosen.

APPARATUS

The first consideration in preparing the experimental situation was given to the problem of eliminating to the greatest possible extent the disturbing factors of reverberation and standing waves in the observation room. P. M. Zoll, who had been using the room for his experiments on tonal volume, was convinced that much of the irregularity in his results and in those of other experimenters, should be laid to these factors. Consequently, it was decided to cover the walls and ceiling with a 2-in. thickness of rock wool, and the floor with celotex, 1 in. thick. The coefficients of absorption for these two materials at a frequency of 1024 cycles are 0.82 and 0.72 respectively. The intensity of the standing waves in the room was further diminished by hanging sheets of muslin a few inches apart throughout the room. About 100 sq. yd. of muslin were used in this way. The room measures 7 x 17 x 10 ft.

The generating apparatus was situated outside the observation room. It consisted of a General Radio 508-A oscillator to which was connected the necessary external capacitance to give any frequency desired in the middle auditory range. By means of a switch, the *E* could readily change the amount of the external capacitance so that two tones of different pitch could be presented alternately to the *O*. This switch[14] was so constructed that it served to eliminate the click from the loud speaker. The two tones were presented to the *O* at the rate of about 40 per min. One of the tones was of a fixed intensity, and will be referred to as the standard tone. The other tone could be varied in intensity by means of a rheostat connected across the output of the oscillator and held in the hands of the *O*. It was possible, then, for the *O* to equate the two alternating tones either in respect of loudness or of volume simply by changing the intensity of one of them. The voltage of each tone was read directly from a General Radio output-meter.

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[12] E. G. Boring, *The Physical Dimensions of Consciousness*, 1933, 117.

[13] R. Gundlach, Tonal attributes and frequency theories of hearing, *J. Exper. Psychol.*, 12, 1929, 187-196.

Three different sources of sound were used in the course of the experimentation. Since the results from all three of them are in close agreement, the results are not an artifact of the source used.

One source was a Western Electric earphone, type 509 W. It was mounted about 1 m. behind the *O*'s head in a horizontal position. A small mirror, 5 mm. x 2.5 mm., was suspended in a split silk thread fastened to the prongs of a heavy brass fork which could be lowered over the face of the diaphragm of the earphone. The torsional force of the thread when twisted held one edge of the mirror firmly against the diaphragm at an angle of about 30°. Rotation of the mirror due to the vibration of the diaphragm served, then, to deflect a beam of light reflected by the mirror; and the amount of this deflection served as a measure of the amplitude of vibration of the diaphragm. The purpose of this arrangement was to provide a basis for showing the relationship of volume to the amplitude of the sound-wave.

The second source of sound was a Western Electric loud speaking telephone, No. 560 type, located about 1 m. behind the *O;* and the third source consisted of two Jensen "concert junior" dynamic speaker, 6 in. in diam., mounted so as to fit immediately over the ears of the *O*. This arrangement was utilized for the more extensive series of observation, because it reduced the effect of interference to a negligible amount, and at the same time was free from the troublesome characteristics of earphones, such as the dependence of intensity on the manner in which the phone is held to the ear. It also permits the operation of the speakers at a very low level of intensity with a consequent improvement in the purity of the tones. Furthermore, the sensation level of the tones could be conveniently altered by changing only the potential impressed on the field coils of the speakers.

A signal system between the *O* and the *E* completed the set-up.

PROCEDURE AND RESULTS

The *O*s in the experiment were Professor E. G. Boring (*B*), Dr. J. Volkmann (*V*), Dr. C. Goldthwait (*G*), Mr. E. B. Newman (*N*), and Miss S. V. Fladeland (*F*). *B* and *V* had observed in Zoll's experiment, and were consequently trained in the recognition of volume. *G* and *N* were familiar with the concept of volume, but had never tried to observe it under experimental conditions. *F* was a congenitally blind instructor from the Perkins Institute, who had never heard of volume as a tonal attribute.

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[14] For a description of this switch see S. S. Stevens, A switch for the presentation of electrically generated tonal stimuli, this JOURNAL, 46, 1934, 138-139.

In the preliminary experiment, *B, V,* and *N* were each given, in continuous alternation, eight pairs of tones of different frequency with the instructions, in one series, to match them for volume, and in another series for loudness, by changing in each case, the intensity of one of the tones. The tones in each pair were separated in frequency by approximately 50 cycles. The results were,[15] without exception, that the *O* found the two tones equal in volume after the tone of higher frequency had been made more intense than the lower tone. In other words, it was clear that a loud high tone can be experienced as equal in volume to a soft low tone. Although *G* had reported that at first the tones were separated too far in frequency for him to make them equal in volume, after some training with pairs of a lesser difference, he found the match easy to make.

Following this series, *V, G,* and *N* were given a series of eight different tones, each paired with a standard tone of 512 cycles at an intensity of 5.0 volts. The *O*s matched the two tones of each pair in both volume and intensity. The Western Electric earphone was used in this series, so that the relative amplitude of the tones could be determined. The *O*s again matched the tones in volume by increasing the intensity of the higher tone, or by decreasing the intensity of the lower tone, as compared with the value at which the variable tone was judged equal to the standard in intensity. It is this difference, then, in which we are primarily interested, for it shows the amount of intensitive difference which will just compensate for a given difference in pitch when the task is to match two tones for volume. The following are the averages for the three *O*s, when each *O* made 10 intensitive and 10 volumic equations:

Frequency (cycles)	470	480	490	500	512	525	540	565	603
Difference (volts)	—2.00	—1.52	—1.36	—.98	0.00	1.06	2.20	2.86	3.15
Av. amplitude (mm.)	9.0	12.6	15.3	16.5	19.9	22.0	26.3	27.7	29.7

From these data it is clear that the amount of intensitive difference which will compensate for a given difference in pitch varies as the size of the latter difference. From the measures of amplitude it is also evident that the amplitudes of tones of equal volume are not equal, as Boring suggested might be the case.[16] Volume is not at all a simple function of amplitude.

With the dynamic speakers as the source of sound, six tones were matched

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[15] The complete data are contained in the author's doctoral dissertation on file in the library of Harvard University under the title of this article.

[16] E. G. Boring, Auditory theory with special reference to intensity, volume, and localization, this JOURNAL, 37, 1926, 164.

TABLE I

RESULTS OF MATCHING 6 TONES TO THE STANDARD FREQUENCY 900 CYCLES AT DIFFERENT SENSATION LEVELS BY *G* AND *N*

The values in db. are computed from the thresholds 0.0020, 0.00063, and 0.00016 volts for the 55, 65, and 75 db. levels respectively. Each entry is the average of 10 observations.

	Frequency	G						N					
		55 db.		65 db.		75 db.		55 db.		65 db.		75 db.	
		volts	db.	volts	db.	volts	db.	volts	db.	volts	db.	volts	db.
Intensity	800	1.22	55.71	1.24	65.88	1.16	77.21	1.21	57.15	1.38	66.81	1.44	77.57
	850	1.25	55.92	1.21	65.67	1.08	76.59	1.13	56.06	1.20	65.60	1.27	76.98
	875	1.27	56.05	1.21	65.67	1.08	76.59	1.02	54.73	1.10	64.84	1.09	76.09
	925	1.26	55.99	1.25	65.95	1.13	76.98	1.02	54.15	1.10	64.84	1.02	76.17
	950	1.31	56.32	1.27	66.09	1.13	76.98	1.10	55.49	1.20	65.60	1.19	76.25
	1000	1.25	55.92	1.33	66.49	1.18	77.36	1.20	56.19	1.33	66.49	1.29	77.50
Volume	800	.42	46.44	.59	59.43	.79	73.97	.97	54.24	1.04	64.35	1.03	75.65
	850	.75	51.48	.77	61.74	.81	74.09	.98	54.24	1.00	64.01	1.03	75.74
	875	.96	53.62	.95	63.77	.92	75.19	.93	53.53	.99	63.93	.95	75.29
	925	2.08	60.14	1.65	68.36	1.28	78.06	1.14	55.34	1.23	65.81	1.17	77.06
	950	2.80	62.92	1.94	69.77	1.39	78.77	1.30	57.79	1.47	67.36	1.55	78.20
	1000	4.00	66.02	2.78	72.89	1.61	80.05	1.50	59.51	1.82	69.21	1.89	79.44
Int. minus Vol. (in db.)	800		9.26		6.45		3.24		2.91		2.46		1.92
	850		4.44		3.93		2.50		1.82		1.59		1.24
	875		2.43		1.90		1.39		1.20		.91		.80
	925		−4.15		−2.41		−1.08		−1.19		−.97		−.89
	950		−6.60		−3.68		−1.79		−2.30		−1.76		−1.45
	1000		−10.10		−6.40		−2.70		−3.32		−2.72		−1.94

in intensity and volume to a standard frequency of 900 cycles. The matches were made by *G* and *N* at three different sensation levels. The numerical results are recorded in Table I.

In Fig. 1 the results obtained from *G* are plotted as equal-volume con-

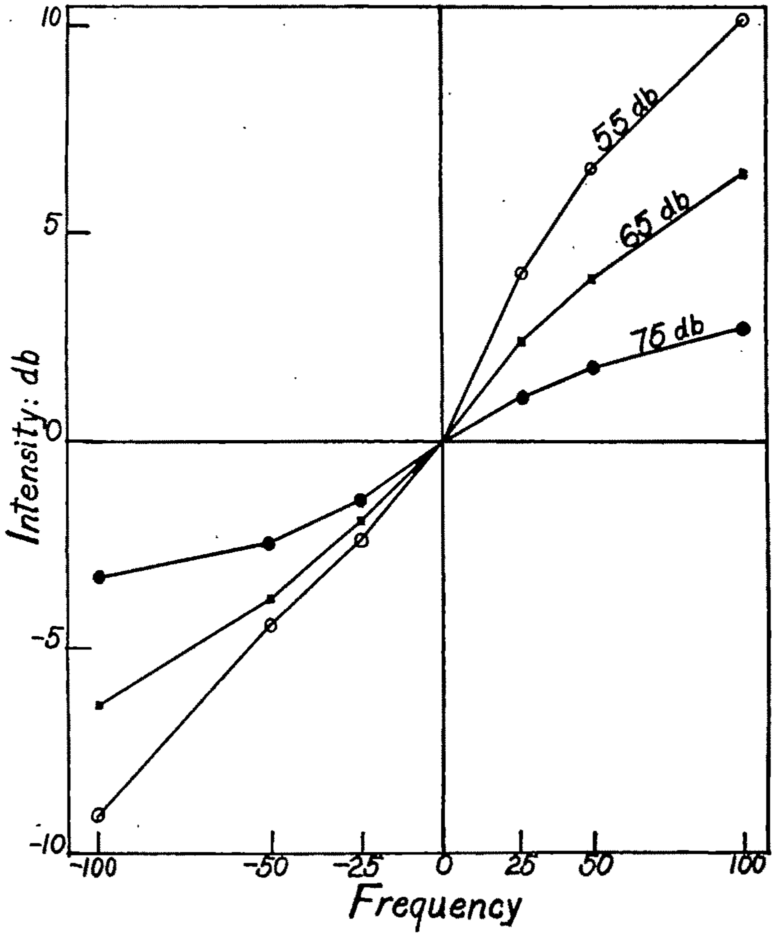

FIG. 1. SHOWING THE MANNER IN WHICH THE AMOUNT OF INTENSITIVE DIFFERENCE NEEDED TO COMPENSATE FOR A DIFFERENCE IN FREQUENCY DEPENDS UPON THE MAGNITUDE OF THE DIFFERENCE IN FREQUENCY AND UPON THE SENSATION LEVEL

The values from which these curves are drawn were obtained by subtracting the values (in db.), at which the *O* reported the respective tones equal to the standard tone (900 cycles) in volume, from the values at which he reported them equal in intensity.

tours with sensation levels as parameters. *N*'s results yield similar functions except that the absolute values are different, as can be seen from an inspection of the data in Table I. The manner in which the slope of the curves decreases as the sensation level increases suggests that at very high sensation levels the volume of all tones would be equal. This possibility has not been thoroughly investigated as yet. Listening to such loud tones is a very unpleasant occupation.

Fig. 1 shows that at low sensation levels the amount of intensive difference necessary to compensate for a given difference in frequency is greater than at high sensation levels. This function means that the difference in volume between two given tones is greater at lower sensation levels. From this fact it follows that the higher tones increase in volume more rapidly than low tones when sensation level increases. Both low and high tones increase in volume when their intensity is increased, but the high tones increase in volume more rapidly. For instance, at 40 db. a tone of 800 cycles is phenomenally larger than a tone of 1000 cycles, but at 100 db. they have the same, or nearly the same, volume. Hence, the 1000-cycle tone increases more in volume than the 800-cycle tone as the sensation level is increased from 40 to 100 db.

It is interesting to note that the curves of Fig. 1 indicate the probable form that equal-volume contours would have in relation to the familiar diagram of the auditory area as drawn by Fletcher.[17] Equal loudness contours have been determined by Kingsbury[18] and are reproduced on Fletcher's diagram, along with equal sensation level contours. From *G*'s data it appears that equal-volume contours would be formed as follows. At the sensation level of 100 db. the equal-volume contour would approximately coincide with the contour representing equal sensation level. Then, as the sensation level decreases, the slope of the equal-volume contours would increase relative to the slope of the equal-loudness contours, until at points near the threshold of hearing the slope would reach a maximum. The precise mapping of these contours would involve the exploration of the entire "auditory area" by the method used in this experiment. The above observations serve only to indicate that such a mapping is possible and the probable form which it would take.

The coefficients of variation of the results shown in Table I are presented in Table II. In every case the average variability is greater among the judgments of volume than among those of intensity for the same sensation level. In the case of *G* there is a pronounced tendency for the lower sensation levels to yield larger average deviations, both for volume and intensity, but the large variability in the results of *N* at the level of 65 db. makes this tendency less clear-cut for him. If we take these coefficients to be indicative of the relative sizes of the relative differential limens, it becomes apparent that the results are in agreement with those of Riesz,[19] who showed that the relative differential limen for intensity increases as the sensation level is lowered. Shower and Biddulph[20] found an analogous situation to hold for pitch. Then, since judgments of both pitch and intensity are made with a larger probable error at the lower sensation levels, and since the judgment of volume is a function of both the frequency and the intensity of the stimulus, it seems reasonable to expect the variability of the volumic judgments to vary as it does in the case of *G*. In short, the relative differential limens of pitch, loudness, and volume all increase in size as the sensation level decreases.

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[17] H. Fletcher, *Speech and Hearing,* 1929, 162.

[18] B. A. Kingsbury, A direct comparison of the loudness of pure tones, *Phys. Rev.,* 2nd ser., 29, 1927, 588-600.

TABLE II

Coefficients of Variation of the Results Obtained from G and N at 3 Different Sensation Levels

Each entry represents the variation in 10 equations made at each frequency and sensation level.

		Sensation level					
	Frequency	Intensity			Volume		
		55 db.	65 db.	75 db.	55 db.	65 db.	75 db.
G	800	9.59	5.97	6.03	29.52	22.88	8.22
	850	7.91	5.12	5.09	19.87	12.86	6.54
	875	7.87	4.13	4.44	14.48	7.16	6.85
	925	7.62	3.92	2.65	12.39	9.76	7.11
	950	7.02	5.51	2.30	7.75	10.88	5.76
	1000	9.20	6.24	7.37	7.25	8.06	7.45
	Av.	8.20	5.15	4.65	15.30	11.93	6.99
N	800	4.31	8.62	5.29	4.56	10.38	5.88
	850	2.99	2.42	3.01	7.67	1.59	7.04
	875	7.61	8.32	2.25	4.32	7.17	6.77
	925	4.11	7.18	3.30	3.08	9.02	3.77
	950	6.72	7.33	5.55	8.84	7.00	4.62
	1000	1.29	10.53	4.92	10.42	11.32	4.20
	Av.	4.51	7.33	4.05	6.48	7.74	5.38

It will be noted that the contour curves of Fig. 1 tend to be sigmoid in form. This tendency is present in every one of twelve different curves determined during the course of this experiment. The reason is not known. Perhaps the *O*s tend to over-compensate for the volumic difference caused by a difference in pitch when that difference is small.

With regard to the question as to whether volume is more dependent upon the frequency of the stimulus or upon its intensity, an interesting calculation can be made from the data of Table I. Let us take the case of *N*. At a sensation level of 65 db. this *O* used about 2.5 db. of intensity to compensate for a difference of 100 cycles. Now, from Riesz's results[21] it is seen that 2.5 db. at this sensation level is equivalent to about four j.n.d. of intensity, and from the data of Shower and Biddulph[22] it appears that 100 cycles at the frequency-level of 1000 cycles represents about 30 j.n.d. of pitch. It seems, therefore, that one j.n.d. of intensity is equivalent to almost 8 j.n.d. of pitch in altering the volume of a tone for this *O*. The ratio for *G* would be only about half as large. In general, however, one can conclude that a j.n. change in the intensity of a tone has a greater effect on its volume than a j.n. change in pitch.

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[19] Riesz, *op. cit.*

[20] E. G. Shower and R. Biddulph, Differential pitch sensitivity of the ear. *J. Acoust. Soc.*, 3, 1931-32, 275-287.

It now seems probable that negative results were obtained by Gundlach[23] because the tones were too widely separated in frequency. In the present experiments points were found (usually about 300 cycles removed from the standard stimulus) at which the *O*s had difficulty in matching the tone to the standard tone in volume. It was unfortunate that Gundlach gave his *O*s frequencies of such wide separation as 100-400, 400-800, and 800-2000 cycles, for when the *O*s tried to equate the two tones in volume the intensity of the higher tone had to be made so great that it was accompanied by "violent starts and mutterings on the part of all the observers." Even under these conditions a few equations were "forced out of the subjects," as Gundlach phrased it; but the separation was obviously too great.

The case of *F*, the congenitally blind *O*, is of special interest because it shows that, contrary to the implication of Banister,[24] blind persons can be trained to judge volume. It also shows that the judgment is not necessarily made in terms of visual imagery. The same procedure was used as was employed with the other *O*s, and the results, although limited to fewer observations, were substantially similar to those already described. A function similar to those of Fig. 1 was obtained. At the beginning of the experiment *F* reported that concrete kinaesthetic imagery was being used, but at the end of a series of 32 judgments she stated that it had become possible for her to abstract the 'size' of the tones from their pitch and loudness without the aid of concrete imagery.

The foregoing experimental results show that what has been called ***volume*** is a phenomenal dimension of pure tones and that this dimension is a complex function of both the frequency and the amplitude of the stimulus. Since, in fact, Kingsbury[25] showed that loudness is a complex function of the two dimensions of the stimulus, and in view of the suggestions of Troland[26] and Ogden[27] that pitch itself depends upon *both* frequency and amplitude, it may be stated that no psychological dimension need be a simple correlate of a single dimension of the stimulus.

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[21] Riesz, *op. cit.*

[22] Shower and Biddulph, *op. cit.*

[23] R. Gundlach, Tonal attributes and frequency theories of hearing, *J. Exper. Psychol.*, 12, 1929, 187-196.

[24] H. Banister, Auditory theory: A criticism of Professor Boring's hypothesis, this JOURNAL, 38, 1927, 436-440.

The relation of volume to the localizability of tones seems, on the basis of introspective evidence, to be fairly intimate. The low tones are reported as ill-defined and indefinite as to position in space, whereas the high tones are usually more definite as to form and localization. Boring[28] suggested that volume may determine localization, but Banister[29] reversed the argument and said that the notion that tones have volume "may be only tantamount to a confession of failure of discrimination [of localization]." An adequate study of the relation of localizability to pitch and intensity would probably clear up this issue.

Finally, we might ask what tonal volume is. We are justified in saying that volume is a phenomenal dimension of tones. It is a dimension in terms of which an *O* is able consistently to make discriminable judgments, and in terms of which he is able to equate two tones which differ in respect of every other phenomenal dimension. It is better to call it a *dimension* than an *attribute,* because the word *attribute* has historically more specific connotations. The word *dimension* signifies that a tone can have any number of 'sizes;' that is to say, volume is one of the aspects in terms of which a tone can undergo continuous variation. *Volumic dimension* means a continuum, the entire range of specific values of volume that tones might have. The term *variable* can be applied to the specific volumic values, V, that a tone may have. Consequently, we can say that V varies in the volumic dimension and is a function of the frequency and the intensity of the stimulus.

CONCLUSIONS

(*1*) The previous investigations of the facts of tonal volume suffer from the fact that stimulus-conditions were not adequately controlled, and from the use of the method of constant stimuli which seems not to have been adequate to the problem. As a result, the quantitative data of the earlier experiments show little agreement and reliability. All of these experiments, however, have left us with introspective evidence that volume is a discriminable aspect of tonal stimuli.

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[25] Kingsbury, *op. cit.*

[26] L. T. Troland, *Psychophysiology,* II, 1930, 222.

[27] R. M. Ogden, *Hearing,* 1924, 258.

[28] Boring, *op. cit.,* this JOURNAL, 37, 1926, 168.

[29] Banister, *op. cit.*

(*2*) The method of average error has shown itself to be suitable to the study of the volumic dimension of tones, largely because it provides an opportunity for the *O* to manipulate the tones, and to stabilize his criteria by voluntary reference to large differences of volume.

(*3*) Under this method the *O* makes an equation of volume by varying the relative intensity of two tones separated by a constant difference of pitch.

(*4*) Tones of different pitch can be equated in volume by trained *O*s. The variability of these equations is greater when the tones are equated in volume, than when they are equated for intensity.

(*5*) The amount of intensitive difference which is required to compensate for a given difference of pitch, in order to make two tones equal in volume, depends upon the sensation level at which the equation is made. More compensatory intensitive difference is required at the lower sensation levels.

(*6*) The congenitally blind can be trained to observe volume.

(*7*) Pitch, loudness, and volume may each be a complex function of the two variables of the stimulus, frequency, and amplitude.

(*8*) The relation of the volume of tones to their localizability and the relation of volume to psychological space remain undetermined.

EMOTIONAL REACTIONS CONNECTED WITH DIFFERENCES IN CEPHALIC INDEX, SHADE OF HAIR, AND COLOR OF EYES IN CAUCASIANS

By GEORGE M. STRATTON, University of California

The present part of a more extended study[1] deals with the question whether the emotions of persons will be found to differ when we pass from those who have certain physical features to those who have other physical features. The study may prove to be the more significant because the bodily features chosen for our attention are among the signs often used to distinguish certain chief strains within the Caucasian race. The features selected are the cephalic index, the shade of the hair, and the color of the eyes.

The investigation in the course of several years involved somewhat over 1000 persons as subjects, both women and men, all of them Caucasian students in the University of California.[2] Measurements of each person were made from which the cephalic index could be computed; and there were observations which allowed each person to be classed as of light, medium, or dark hair, using for each observation a scale of actual samples of hair, to escape some of the errors of a purely subjective judgment.[3] There were observations, finally, to determine whether the eyes were

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* Accepted for publication May 15, 1933.

[1] G. M. Stratton, Emotion and the incidence of disease, *J. Abn. & Soc. Psychol.*, 21, 1926, 19-23; Anger and fear: Their probable relation to each other, to intellectual work, and to primogeniture, this JOURNAL, 39, 1927, 125-140; Emotion and the incidence of disease. The influence of the number of diseases, and the age at which they occur, *Psychol. Rev.*, 36, 1929, 242-253.

[2] Of students not Caucasian, it is hoped that a report may be made at some later time.

[3] In the scale here used, the lower limit of 'light' hair is approximately the shade of sample No. 9 of the Fischer Scale; the upper limit of 'dark' hair is approximately the shade of No. 6 of the Fischer Scale. 'Medium' hair includes all hair judged to lie between these limits. These limits were determined by combining the judgments of three or more members of the psychology department in regard to samples of the hair of 40 students chosen at random. The samples were arranged in order from lightest to darkest (the extremes of shade we may designate as a and d respectively). The observer was then requested to select two other shades (b and c) in the array of samples, such that their relation to the extremes would be judged by him to accord with the formula $a{:}b{::}b{:}c{::}c{:}d$. The aim thus was to have equal steps of apparent difference in shade. There was of course no intention to have the array fall into groups having equal numbers of samples. Indeed when the 40 samples were grouped by the method just described, there were 9 samples of 'light' hair, 13 samples of 'medium,' and 18 samples of 'dark.' In Table I it will be seen that by this scale there were in our student population about equal numbers of persons having 'light' and 'medium' hair, while a far larger group is of those with 'dark' hair.

blue or brown or a mixture of blue and brown. These three classes of eyes will hereafter, for brevity, be called light, dark, and medium. The emotional reactions were recorded by a method whose full description has appeared elsewhere.[4] In essence it was that directly after having found himself in one of certain designated situations, *O* recorded the degree of his reaction according to a definite scale of such reactions, each degree of which was briefly indicated by definite characteristics, in printed instructions given him. From the 1000 and more persons already mentioned, a crude measure was thus obtained of about 9000 particular emotional reactions of fear or anger, immediately after their occurrence in carefully specified situations of real life. From the crude measures made by the *O*s, more refined measures were derived by a computation described in the article just cited.

TABLE I

MEAN EMOTIONAL RESPONSES WITH P.E., OF *O*S DISTINGUISHED BY HAIR SHADE

Shade of hair	Fear			Anger		
	No. *O*s	No. responses	Mean	No. *O*s	No. responses	Mean
Light	302	863	98.21 ± .57	326	1092	98.74 ± .51
Medium	331	1085	97.67 ± .48	347	1468	101.59 ± .41
Dark	484	1789	99.89 ± .39	553	2702	100.84 ± .30

TABLE II

DIFFERENCES, WITH P.E., IN EMOTIONAL RESPONSES OF *O*S DISTINGUISHED BY HAIR SHADE

Shades of hair of groups compared	Fear			Anger		
	Diff.	$P.E._{diff.}$	$Diff./P.E._{diff.}$	Diff.	$P.E._{diff.}$	$Diff./P.E._{diff.}$
Light, Medium	0.54	.75	0.72	2.85	.65	4.36
Light, Dark	1.68	.69	2.43	2.10	.59	3.55
Medium, Dark	2.22	.62	3.59	0.75	.51	1.47

Coming now to the findings by this procedure, there appear only small emotional differences between the average of any one group and the average of any other. Many of the differences are indeed so small, both in themselves and in relation to their probable errors, that they may most suitably be ascribed to chance. But some of them though small, are statistically trustworthy. In many cases the difference exceeds 3 times its probable error, occasionally running as high as 6 or 8 times. Such differences may be regarded as probably due, not to accident, but to some real although slight contrast in the emotional tendencies of the groups compared.

It may be well to look, first, at what appears in the emotional reactions connected with each of the three physical variables separately; that is, with variations in shade of hair, disregarding eye-color and shape of head; and similarly with these other features, each in its turn.

Thus, taking the shade of hair, there appears to be a difference, with respect to fear, between the group composed of the medium-haired and the group composed of the dark-haired, a difference which is more than thrice as large as its P.E. (Table II). With respect to anger, there appear differences between the light-haired and the dark-haired, and between the light-haired and the medium-haired, which are from 3 to 4 times their P.E. (Table II). The medium-haired have a slightly less intense fear than have the dark-haired; the medium-haired and the dark-haired have a slightly more intense anger than have the light-haired (Table I).

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[4] *Op. cit.*, this JOURNAL, 39, 1927, 125-140.

TABLE III

MEAN EMOTIONAL RESPONSES WITH P.E., OF Os DISTINGUISHED BY EYE COLOR

Color of eyes	Fear			Anger		
	No. Os	No. responses	Mean	No. Os	No. responses	Mean
Light	454	1489	98.91 ± .42	509	2106	101.71 ± .37
Medium	478	1605	99.18 ± .43	510	2312	99.84 ± .31
Dark	185	643	97.97 ± .62	207	844	99.97 ± .58

TABLE IV

DIFFERENCES WITH P.E., IN EMOTIONAL RESPONSES OF Os DISTINGUISHED BY EYE COLOR

Color of eye of groups compared	Fear			Anger		
	Diff.	P.E.$_{diff.}$	Diff./P.E.$_{diff.}$	Diff.	P.E.$_{diff.}$	Diff./P.E.$_{diff}$
Light, Medium	.27	.60	.45	1.87	.48	3.87
Light, Dark	.94	.75	1.26	1.74	.69	2.53
Medium, Dark	1.21	.75	1.60	.13	.66	.20

TABLE V

MEAN EMOTIONAL RESPONSES WITH P.E., OF Os DISTINGUISHED BY CEPHALIC INDEX

Cephalic index	Fear			Anger		
	No. Os	No. responses	Mean	No. Os	No. responses	Mean
69–77*	333	1065	101.73 ± .53	382	1683	102.87 ± .38
78–80†	411	1423	96.92 ± .41	446	1914	98.60 ± .36
81–89‡	373	1249	98.62 ± .47	398	1665	100.64 ± .40

* Here and later, this expression means the entire group of Os with a cephalic index of 77.49 or less.

† Here and later, this expression means the entire group of Os with a cephalic index of 77.50 to 80.49, inclusive.

‡ Here and later, this expression means the entire group of persons with a cephalic index of 80.50 or more.

Next, taking eye-color, the light-eyed are slightly more intense in their anger than are the medium-eyed (Table III), the difference between the average responses of the two classes being above thrice its P.E. (Table IV). No other statistically trustworthy difference appears.

As regards the shape of the head, the narrow-headed are more intense both in their anger and in their fear than are either of the other groups (Table V): in like situations the narrow-headed become angrier or more afraid than do those whose heads are of medium proportions or broad. The medium heads show the least intense fear and anger, of our three groups; the broad heads fall between the medium and the narrow. The difference, in all cases but one, is statistically reliable (Table VI).

TABLE VI

DIFFERENCES, WITH P.E., IN EMOTIONAL RESPONSES OF Os DISTINGUISHED BY CEPHALIC INDEX

Indices of groups compared	Fear			Anger		
	Diff.	$P.E._{diff.}$	$Diff./P.E._{diff.}$	Diff.	$P.E._{diff.}$	$Diff./P.E._{diff.}$
69–77, 78–80	4.81	.67	7.18	4.27	.52	8.16
69–77, 81–89	3.11	.71	4.39	2.23	.55	4.04
78–80, 81–89	1.70	.62	2.72	2.04	.54	3.79

If these results should be supported by other studies, and they of course await such confirmation, several features would attract one's attention.

First, the three bodily features, at least as here used, are not equally important as indicators of the average emotional reactions to be expected from a large number of persons. If one reviews the tables given thus far it will be seen that the average reaction in fear or anger when we pass from one head-shape to another head-shape usually varies by a greater amount than when we pass from one hair-shade to another, or from one eye-color to another. As between these latter features, the emotional differences between our groups distinguished by shade of hair are greater than the emotional differences between our groups distinguished by color of eyes. It is of course possible that by some other classification based on hair-shade or by some other classification based on eye-color or shape of head the outcome would be different. But considering the differences here chosen, the bodily features vary in importance as indices of the emotional responses to be expected, and in the following order from great to less: (1) cephalic index; (2) shade of hair; (3) color of eyes.

As between fear and anger, there appears little, if any, constant difference in the power of the different bodily features to indicate what is to be expected. The amount of difference in *fear* connected, say, with contrasts in cephalic index, is, on the whole, not strikingly different from the amount of difference in *anger* connected with such head-contrasts. Similarly for the other two bodily features. The average difference in fear connected with difference of cephalic index is very slightly more than that in anger. The average difference in fear connected with difference in shade of hair or color of eyes is slightly less than that in anger.

Furthermore, it is of interest that the emotional differences are most pronounced, not between groups whose physical features stand in strongest contrast, but between groups physically closer together. Thus with respect to shade of hair, the largest and statistically most trustworthy emotional difference is not between the light-haired and the dark-haired, but between the medium-haired and one or other of the extremes (Table II). Or again, with respect to eyes, the most pronounced emotional difference is not between the light-eyed and the dark-eyed, but between the medium-eyed and one or other of the extremes (Table IV). Or taking our third basis of classification, shape of head, the clearest emotional difference is not between the polar opposites, the narrow-headed and the broad-headed, but between the narrow-headed and those whose heads are of medium cephalic index (Table VI).

TABLE VII

Mean Emotional Responses, with P.E., of Os Distinguished by Combinations of Cephalic Index, Hair Shade, and Eye Color

Index	Hair	Eye	Fear			Anger		
			No. Os	No. responses	Mean	No. Os	No. responses	Mean
69–77	Light	Light	62	197	99.53 ± 1.09	69	250	101.94 ± 1.08
81–89	Medium	Medium	48	158	95.13 ± 1.10	49	224	105.08 ± .91
69–77	Dark	Dark	35	102	101.27 ± 1.43	45	193	104.66 ± .93

TABLE VIII

Differences, with P.E., in Emotional Response of Os Distinguished by Combinations of Cephalic Index, Hair Shade, and Eye Color

Combinations in the groups compared						Fear			Anger		
Index	Hair	Eye	Index	Hair	Eye	Diff.	$\text{P.E.}_{\text{diff.}}$	Diff./$\text{P.E.}_{\text{diff.}}$	Diff.	$\text{P.E.}_{\text{diff.}}$	Diff./$\text{P.E.}_{\text{diff.}}$
69–77	Light	Light	81–89	Medium	Medium	4.40	1.55	2.84	3.14	1.41	2.22
69–77	Light	Light	69–77	Dark	Dark	1.74	1.80	.97	2.72	1.46	1.86
81–89	Medium	Medium	69–77	Dark	Dark	6.14	1.80	3.40	.42	1.30	.32

One may next ask whether the present findings point to a difference in the emotional disposition of the three more frequently recognized strains within the Caucasian race; namely, the Nordic, Alpine, and Mediterranean. There are indications of a difference.

In the Nordic stock, it will be recalled, a relatively small cephalic index (that is, a narrow head) is joined with light hair and light eyes; in the Alpine, a relatively large cephalic index (a broad head) is joined with medium hair and medium eyes; and in the Mediterranean stock, a small cephalic index (again, a narrow head) is joined with dark hair and dark eyes. Among the persons here studied, the number of individuals who combine all three of the features in any one of the ways just described is relatively small, ranging from 35 to 69 persons; and the indication from groups so small is of course unsatisfactory. But such as the numbers are, they suggest a difference with respect to fear between our 'Alpines' and the others; the 'Alpines' having a mean score for fear that is lower than that of our 'Nordics' or our 'Mediterraneans' (Table VII). But only between 'Alpine' and 'Mediterranean' is the difference over thrice its P.E. (Table VIII).

When the grouping is based on two features instead of three, the groups of course become somewhat larger, and there appears a larger number of emotional differences that are statistically satisfactory. Thus taking only head-shape and color of eyes as indicators of the different stocks among the Caucasians;[5] our 'Alpines' have a lower emotion score for both fear and anger than either our 'Nordics' or our 'Mediterraneans' (Table IX); and for the comparisons between 'Alpine' and 'Nordic,' and between 'Alpine' and 'Mediterranean' there is, with respect to both fear and anger, a fairly satisfactory relation between the differences and their P.E. (Table X). The indications from these larger groups based on head-shape and color of eyes thus strengthen and extend the indications given by the smaller groups based on three features.

TABLE IX

Mean Emotional Responses, with P.E., of Os Distinguished by Certain Combinations of Cephalic Index and Eye Color

Combination		Fear			Anger		
Index	Eye	No. Os	No. responses	Mean	No. Os	No. responses	Mean
69–77	Light	142	453	101.23 ± .75	166	716	105.63 ± .61
81–89	Medium	160	541	97.85 ± .68	170	795	101.58 ± .54
69–77	Dark	47	128	103.88 ± 1.23	54	224	104.99 ± .93

When, further, we base our 'racial' classification on a different combination of two features; namely, on shape of head and shade of hair,[6] instead of head-shape and color of eyes, the group in every case comprises more than 100 persons. Some of the former emotional differences among our groups now reappear dependably, and additional differences appear. Again our 'Alpines' have lower fear scores than our 'Mediterraneans' and 'Nordics;' and the 'Alpines' have lower anger scores than the 'Mediterraneans,' but not lower than the 'Nordics' (Table X). Now the 'Nordics' and the 'Mediterraneans' are different from each other by relatively large and statistically satisfactory amounts, the 'Nordics' showing less intense fear and less intense anger than our 'Mediterraneans' (Table XI). That our 'Nordics' are less intense than our 'Mediterraneans' in their responses of fear was faintly indicated in Table IX; and that the 'Nordics' are less intense than the 'Mediterraneans' in both fear and anger was faintly indicated in Table VII. These faint indications now become clear.

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[5] A combination of cephalic index 69-77 and light eyes is here assumed to come closer to a 'Nordic' strain than would any other combination of head-shape and color of eyes here used; of cephalic index 81-89 and medium eyes, to an 'Alpine' strain; and of cephalic index 69-77 and dark eyes, to a 'Mediterranean' strain.

[6] A combination of cephalic index 69-77 and light eyes is here assumed to come closer to a 'Nordic' strain than would any other combination of the head-shapes

Briefly, then, our samples of the Caucasian stocks, as here defined, might, if we be guided solely by such differences as seem trustworthy statistically, be ranked as follows with respect to their fear and anger, beginning with the stock showing the most intense reactions: (1) 'Mediterranean;' (2) 'Nordic;' (3) 'Alpine.'

In some respects this order is in accord with the frequent opinion that the South European (for example, the Italian or the Spaniard) is more emotional than the European of the Center or of the Northwest; and with the opinion, perhaps less frequent, that the Alpine is the most phlegmatic of the three European stocks. The present indications lend no support, however, to the opinion that the race of the long-headed, light-haired, blue-eyed folk of Northwestern Europe are the most courageous of all the Caucasians, and the most terrible in their wrath. But if the present indications should prove correct, the true believer in the mission of the Nordic people to rule the world may easily shift his ground, and find the superiority of the Nordic, emotionally, to lie in the very fact of his mid-place, in his possessing the virtues of the mean, being neither phlegmatic like the Alpine nor gusty like your Mediterranean.

But in connection with these indications of an emotional contrast in the three European stocks more commonly recognized, another indication should be noted. It is that in the population here studied, an equal if not greater emotional contrast is found between some groups as just defined and another group whose physical features do not fit the usual description of European stocks. I have in mind a group marked by dark hair and eyes and a medium cephalic index, 78-80. In its dark hair and dark eyes this nameless group seems to be Mediterranean, and in its head-shape also it seems to be within the border of the Mediterranean stock as often defined. Yet this nameless group is emotionally at the opposite pole from the 'Mediterranean' as defined in the present study. Whereas the 'Mediterraneans,' in general, display the intensest emotions of all the three groups named, yet the nameless group (Table XIII) reveals a lower average degree of fear and of anger than does even the lowest of the three; namely, the 'Alpine.' The nameless group therefore stands in extreme contrast, emotionally, to the very group to which it is nearest in its physical marks, the 'Mediterranean.' Emotionally this group marked off from the others by mesocephaly alone, or by mesocephaly combined with either dark eyes

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and shades of hair here used; of cephalic index 81-89 and medium hair to an 'Alpine' strain; and of cephalic index 69-77 and dark hair to a 'Mediterranean' strain.

TABLE X

DIFFERENCES, WITH P.E., IN EMOTIONAL RESPONSES OF Os DISTINGUISHED BY CERTAIN COMBINATIONS OF CEPHALIC INDEX AND EYE COLOR

Combinations				Fear			Anger		
Index	Eye	Index	Eye	Diff.	$P.E._{\text{diff.}}$	$\text{Diff.}/P.E._{\text{diff.}}$	Diff.	$P.E._{\text{diff.}}$	$\text{Diff.}/P.E._{\text{diff.}}$
69–77	Light	81–89	Medium	3.38	1.01	3.34	4.05	.81	4.97
69–77	Light	69–77	Dark	2.65	1.29	2.05	.64	1.11	.58
81–89	Medium	69–77	Dark	6.03	1.41	4.29	3.41	1.08	3.17

TABLE XI

DIFFERENCES, WITH P.E., IN EMOTIONAL RESPONSES OF Os DISTINGUISHED BY CERTAIN COMBINATIONS OF CEPHALIC INDEX AND HAIR SHADE

Combinations				Fear			Anger		
Index	Hair	Index	Hair	Diff.	$P.E._{\text{diff.}}$	$\text{Diff.}/P.E._{\text{diff.}}$	Diff.	$P.E._{\text{diff.}}$	$\text{Diff.}/P.E._{\text{diff.}}$
69–77	Light	81–89	Medium	4.09	1.29	3.16	2.03	1.17	1.74
69–77	Light	69–77	Dark	5.04	1.27	3.96	6.34	1.02	6.22
81–89	Medium	69–77	Dark	9.13	1.14	8.02	4.31	.91	4.73

TABLE XII

MEAN EMOTIONAL RESPONSES, WITH P.E., OF *Os* DISTINGUISHED BY CERTAIN COMBINATIONS OF CEPHALIC INDEX AND HAIR SHADE

Combination		Fear			Anger		
Index	Hair	No. *Os*	No. responses	Mean	No. *Os*	No. responses	Mean
69–77	Light	101	288	99.67 ± 1.00	105	359	97.98 ± .87
81–89	Medium	107	371	95.58 ± .82	107	428	100.01 ± .78
69–77	Dark	132	486	104.71 ± .79	161	790	104.32 ± .53

TABLE XIII

MEAN EMOTIONAL RESPONSES, WITH P.E., OF *Os* DISTINGUISHED BY A CEPHALIC INDEX 78–80, ALONE, AND IN COMBINATION WITH DARK HAIR AND DARK EYES

Distinguished by	Fear			Anger		
	No. *Os*	No. responses	Mean	No. *Os*	No. responses	Mean
Index 78–80	411	1423	96.92 ± .41	446	1914	98.60 ± .36
Index 78–80 and Dark Hair	190	712	95.39 ± .56	209	1055	97.69 ± .46
Index 78–80 and Dark Eyes	69	294	93.99 ± .80	76	317	96.65 ± .94
Index 78–80 and Dark Hair and Dark Eyes	50	220	92.09 ± .88	54	239	97.18 ± 1.06

or dark hair or both, seems to call for the special attention of investigators in other fields. Some difference in racial strain not yet clearly recognized may lie at the root of such a psychologically marked group. Or the darkness at present encompassing this group may be dispelled at least in part by further studies in the different temperaments which go with different 'types' of bodily structure.[7] Or race and bodily structure may unite to give us light.

But whether the explanation of the facts indicated be racial or not, the main facts themselves seem fairly clear, that differences in emotion are connected with differences in shape of head, in shade of hair, in color of eye. It seems probable, further, that the main connection between the two orders of difference is a native connection. In the view taken by the present writer, the differences in body-structure here considered are the visible signs of deeper somatic differences, presumably in nervous structure and in nervous functioning. These deeper bodily differences, in their turn, are connected with differences in psychic disposition, of which the emotional reactions here observed are the modes of display.

Some persons will of course reject outright this bare outline of an explanation in so far as it looks toward an innate variation in psycho-somatic disposition. They will prefer to regard the variation not as innate but as acquired. No evidence is yet at hand to prove them wrong, since the cause of the observed connection between anger and, say, a particular shape of head, may conceivably lie in some environmental difference acting upon those who in the present study had this particular shape of head. If one prefers to look first of all to individual experience or social training or to climate or food, or to other peculiarities in the individual's environment, psychic or physical, as the principal if not sole source of a man's difference from his fellows, then one can follow that course here. But in the present case I find this a hard course to follow. For it means that among the university students in the present study, the hundreds of persons with, say, a cephalic index of 78-80, although they had lived scattered through communities in many states, yet they had been subject to an environment importantly and systematically the same, while importantly and systematically different from the environment of those whose cephalic index is 81-89 or 69-77. Similarly of the students whose hair or eyes differ from those of their fellows. Students with hair of medium shade may, it is true, have lived in surroundings importantly different from the surroundings of students with lighter or darker hair. But the probability of it seems to the present writer small indeed.

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[7] Cf. E. Kretschmer, *Körperbau und Charakter*, 1921.

On the contrary the present evidence invites one to recognize that the innate emotional equipment in any large number of persons is connected with certain differences in the features of the head and hair and eyes. The interest in these connections is heightened because the variations in bodily features here used are, in some cases, although not in all, the variations which mark off certain stocks of the Caucasians.

SUMMARY

(*1*) About 9000 particular instances of either fear or anger were systematically observed in prescribed situations by 1000 and more students all of whom were of the Caucasian race.

(*2*) Head-measurements of these persons were made, from which were derived their cephalic indices; and the persons were observed and classified as to their shade of hair and color of eyes.

(*3*) Significant differences both in fear and in anger were found to be connected with differences in each of the three bodily features named above.

(*4*) The most pronounced differences in emotion occur, in general, not between groups whose physical features stand in strongest contrast, but between groups physically closer together, for example, between the medium-haired and the dark-haired, or between those with medium and those with small cephalic index.

(*5*) The three bodily features as used in the present study are not of equal importance as indices of emotional disposition. Their rank in importance is: first, cephalic index; second, shade of hair; and third, color of eyes.

(*6*) Significant differences of emotion were found to be connected with

those combinations of bodily features which have been thought to point, respectively, to Nordic, Alpine, and Mediterranean strains of the Caucasian race. The persons in the present study with physical marks suggesting a kinship with the Mediterranean stock are, in general, most intense in their reactions of fear and anger; below them come those with Nordic marks; while still less intense emotional reactions were displayed by persons whose bodily marks are Alpine.

(7) Even less intense reactions of fear and anger than those of our 'Alpines,' however, occurred in a group whose bodily marks do not connect them clearly with any of the three strains more commonly recognized. This nameless group is marked by a cephalic index of 78-80 alone, or by this mesocephaly conjoined with dark hair or dark eyes or both. This group in its physical marks is closest to the 'Mediterraneans' of the present study. And yet in the intensity of emotional response it is farthest from these very 'Mediterraneans.'

(8) The difference in emotion here connected with variation in shape of head, color of eye, and shade of hair is probably not due to an accidental concurrence of the bodily variation with some systematic difference in the physical or social environment of the persons here studied. The present writer inclines to regard the emotional differences not as acquired, but as innate; as due probably to an unobserved difference in the psychophysical constitution of the groups here compared.

THE INFLUENCE OF THE REPETITION OF A SITUATION

By EDWARD L. THORNDIKE, Columbia University

In the *Fundamentals of Learning* we presented evidence from a variety of experiments that the mere repetition of a situation did not result in the strengthening of those responses which were made to it frequently at the expense of those which were made rarely. Except for some force other than differences in frequency among the responses to it, these responses will remain the same at the end of a thousand repetitions as at the beginning. This is not because the connections $S \rightarrow R_1$, $S \rightarrow R_2$, $S \rightarrow R_3$, etc., are unmodifiable and maintain rigidly the exact relative strengths which they initially display. On the contrary they may be, and in some of our experiments were, influenced notably by satisfying after-effects. A connection with an initial frequency of 1 in 10 may thus quickly attain a frequency of 9 in 10. Also there were notable shifts in relative frequency apart from any obvious differences in after-effects, which showed that the connections in question were sensitive enough. Not their fixedness, but the impotence of differences in relative frequency to strengthen the more frequent at the expense of the less frequent was to blame.

The fact seems of great importance, first because, if generally true, it makes allowance for relative frequencies unnecessary in many experiments on learning, permitting us to assume that, except for some force other than the relative frequencies themselves, these will remain unchanged. In the second place, it amounts to a disproof of hypotheses of inhibition by drainage wherever these rely on the greater strength of the drainer over the drainee. In the third place, it makes all theories that explain adaptive modifiability as a consequence of the displacement of infrequent or weak by frequent or strong tendencies unsound at their very base.

An experiment devised primarily for an entirely different purpose provides material for a useful study of the influence (rather, of the lack of influence) of the relative frequencies of occurrence of the connections $S \rightarrow R_1$, $S \rightarrow R_2$, $S \rightarrow R_3$, etc.

In this experiment, *E* says a word, and *S* responds by saying any number from 1 to 6, and also any letter of his choice. *E* then says "Right" or "Wrong," these

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TABLE I

The Frequencies of Choice of the Letters in the First (F) 800 and Last (L) 800 Choices of 4800 by 24 Ss. Also the Frequencies of the Four Letters Chosen Most Frequently in the First 800 (4a), Those of the Four Chosen Next Most Frequently (4b), Those of the Four Chosen Next Most Frequently (4c), Those of the Four Chosen Next Most Frequently (4d), and Those of the Ten Letters Chosen Least Frequently in the First 800 Choices (10).

Letter	1		2		3		4		5		6		7		8	
	F	L	F	L	F	L	F	L	F	L	F	L	F	L	F	L
a	111	37	51	21	92	49	52	55	41	70	146	105	75	80	38	29
b	27	166	55	57	35	14	28	75	56	35	82	97	34	50	64	56
c	14	4	38	18	36	66	81	11	87	75	60	15	19	35	82	85
d	29	0	25	36	91	60	68	19	66	44	60	46	57	31	52	47
e	60	94	14	2	53	98	43	8	48	100	7	1	45	32	69	47
f	7	0	41	14	29	22	28	5	26	28	28	15	26	25	10	34
g	6	2	51	24	36	63	57	55	13	42	17	1	28	27	37	21
h	6	2	31	4	4	0	24	34	9	22	4	1	17	53	7	7
i	58	1	18	0	13	4	6	0	7	13	2	0	32	18	23	4
j	30	71	15	0	9	2	0	0	0	18	9	3	2	12	4	0
k	50	22	90	109	12	23	3	0	11	9	0	0	65	7	21	5
l	57	64	53	3	49	52	70	100	49	4	17	13	48	51	30	5
m	13	1	91	62	57	37	30	99	36	32	35	5	25	31	22	83
n	72	81	6	0	27	9	66	66	34	16	7	126	24	11	29	34
o	78	102	1	1	13	56	5	1	31	28	5	3	40	27	42	36
p	13	3	12	20	19	22	5	0	52	39	48	12	26	65	59	81
q	2	10	3	30	1	9	1	1	2	6	18	16	9	17	18	2
r	76	4	62	124	45	52	43	88	36	17	95	29	91	8	50	84
s	16	0	36	72	45	70	64	48	80	75	6	3	53	39	27	48
t	56	123	24	45	30	65	12	1	33	15	4	3	17	47	51	16
u	17	2	2	0	20	3	24	1	8	27	0	3	12	10	18	10
v	0	1	5	68	31	20	2	0	32	5	0	3	19	12	6	8
w	0	0	4	0	11	0	1	0	8	10	1	1	2	17	13	1
x	0	10	56	85	12	0	41	88	6	41	64	109	18	49	2	3
y	2	0	8	2	7	0	44	44	26	26	70	158	4	22	24	54
z	0	0	8	3	23	4	2	1	3	3	15	32	12	16	2	0
4a	337	224	299	380	293	244	285	196	289	229	393	389	288	134	274	269
4b	231	282	210	105	175	237	217	202	190	213	232	182	180	172	195	183
4c	136	259	146	108	132	165	157	283	138	70	98	37	126	97	134	89
4d	60	7	82	81	99	38	104	115	116	97	48	49	94	142	96	189
10	36	28	63	126	101	116	37	4	67	191	29	143	112	255	101	70

announcements applying to the *numbers only*. *S* is told that no letter is ever right or wrong, that they have nothing whatever to do with the learning, but are for use in a different problem, and that, whereas he receives a money payment for each right number given by him, he will receive just the same no matter what letters he says. He is asked, however, not to repeat the same letter, nor to follow any system (such as saying a, b, c, d, a, b, c, d, a, b, c, d, etc., in order). The words said by *E* are in 24 sets of 40, each set being repeated 5 times before going on to the next set.

We thus have for each *S* 4800 choices of a letter in response to the situation whose

TABLE I (*Continued*)

Letter	9		10		11		12		13		14		15		16	
	F	L	F	L	F	L	F	L	F	L	F	L	F	L	F	L
a	162	152	125	46	67	25	0	69	123	68	7	143	119	202	62	34
b	150	89	82	69	14	28	37	51	24	0	66	77	18	17	43	53
c	16	40	142	32	49	4	18	5	89	115	32	104	22	6	54	42
d	1	1	124	77	48	32	19	44	16	8	11	72	47	11	37	28
e	4	95	82	50	21	25	4	21	85	108	2	85	53	74	28	12
f	91	123	19	87	12	7	85	64	3	0	30	38	16	4	22	21
g	138	112	75	62	19	26	13	45	23	23	101	56	27	4	20	14
h	33	2	74	42	8	13	11	20	15	0	7	88	33	1	39	7
i	9	1	0	0	58	18	1	0	9	1	2	10	17	83	0	14
j	0	0	0	66	1	45	11	3	1	0	36	0	5	1	4	0
k	15	1	0	13	3	37	54	53	0	0	1	0	19	11	30	10
l	22	130	0	45	67	31	168	67	68	107	5	0	71	30	70	62
m	26	4	4	2	14	2	130	64	18	6	122	117	45	10	31	45
n	13	12	12	11	79	119	24	81	15	2	23	4	56	27	18	57
o	5	4	9	21	61	49	35	28	95	186	6	0	24	90	56	34
p	3	0	12	3	10	8	22	14	7	0	132	3	22	53	38	70
q	17	0	1	16	32	1	11	1	0	0	45	0	5	0	43	6
r	25	33	0	0	51	44	45	22	99	58	106	1	38	39	22	29
s	5	0	0	53	40	84	42	21	11	7	6	0	49	32	23	74
t	8	0	0	0	77	78	53	84	9	0	3	0	35	6	22	39
u	1	0	0	0	1	72	1	3	2	0	1	0	25	65	1	14
v	1	0	2	4	1	6	8	28	1	0	0	0	6	1	13	13
w	33	1	0	0	0	2	0	8	1	0	0	0	5	0	1	16
x	8	0	6	0	55	17	2	2	32	71	49	2	2	28	83	75
y	0	0	1	1	7	21	5	2	52	40	5	0	20	5	5	9
z	4	0	30	100	5	6	1	0	2	0	2	0	21	0	35	22
4a	551	476	473	224	290	253	437	248	402	420	461	177	299	333	271	205
4b	117	40	261	254	225	128	177	178	241	333	196	79	179	92	179	108
4c	70	171	52	122	169	121	100	167	81	37	96	218	117	162	140	130
4d	38	13	13	22	68	81	53	73	50	10	26	231	92	63	104	152
10	24	100	1	178	48	217	33	134	26	0	21	95	113	150	106	205

main component is "Say a letter" the minor components being the different words said by the *E* and the numbers said by *S* and whatever other stimuli may be acting.

Table I records the frequencies of choice of each letter by each of the 24 *S*s in their first 800 choices and in their last 800 choices of the 4800. Table I also records for each *S* the total frequencies in the first 800 choices for the four most frequent, the four next most frequent, the four next most frequent, and the four next most frequent, and for the ten least frequent; and the same facts for the last 800 choices. There is no waxing of the initially strong tendencies at the expense of the initially weak. On the contrary, 20 of the 24 *S*s show the opposite. In 19 of the *S*s the ten letters of least initial frequency gain.

TABLE I (*Concluded*)

Letter	17		18		19		20		21		22		23		24	
	F	L	F	L	F	L	F	L	F	L	F	L	F	L	F	L
a	15	65	40	0	90	82	170	20	136	32	192	31	46	54	27	42
b	91	104	35	121	87	64	58	47	135	76	136	72	51	71	88	115
c	32	49	62	1	87	53	25	23	44	11	57	5	30	87	23	106
d	17	15	59	106	21	95	37	34	98	34	11	7	131	57	20	22
e	15	53	0	0	6	16	148	77	104	47	8	7	54	70	7	1
f	16	12	83	63	16	20	33	19	18	5	17	1	29	62	28	31
g	16	10	1	0	2	3	14	30	69	102	6	11	22	43	16	0
h	38	7	0	1	28	1	9	68	43	34	11	72	55	30	26	3
i	18	38	0	0	34	9	0	11	36	2	0	0	20	48	11	1
j	9	0	0	0	20	17	0	11	4	11	0	0	1	0	68	74
k	11	0	0	0	7	20	5	53	0	73	1	85	96	45	8	2
l	125	64	104	125	0	0	81	79	7	66	16	6	65	47	72	123
m	9	19	75	99	34	8	49	66	11	71	29	55	31	45	43	82
n	9	44	28	110	104	151	0	1	7	14	7	27	28	10	22	5
o	21	13	1	0	97	68	0	2	10	41	2	64	29	29	8	33
p	22	3	0	0	4	7	12	68	3	1	16	144	9	4	16	0
q	3	1	0	0	6	3	12	5	5	9	0	0	3	2	68	12
r	20	22	46	1	16	1	3	3	16	68	125	50	24	26	51	31
s	44	37	73	145	27	122	2	60	8	42	117	112	5	3	42	0
t	44	22	133	20	7	10	39	27	13	2	8	9	7	38	14	6
u	1	75	1	0	0	0	0	2	1	3	0	0	7	4	9	0
v	6	3	1	2	0	0	1	12	0	6	0	1	1	7	31	46
w	29	8	0	1	9	11	0	40	0	1	0	0	3	5	8	2
x	52	77	1	0	74	30	59	7	9	28	8	41	30	4	46	60
y	80	59	0	0	17	0	43	5	8	15	1	0	8	11	10	0
z	57	0	57	5	7	9	0	30	9	6	32	0	18	0	38	3
4a	353	227	395	207	378	365	458	183	473	189	570	265	347	179	296	324
4b	178	143	251	257	229	100	187	152	192	149	135	61	182	240	182	173
4c	104	73	149	232	96	235	111	99	61	185	54	229	118	182	124	122
4d	71	87	4	2	58	32	38	194	39	78	31	84	94	127	91	216
10	90	270	1	2	39	68	6	172	35	199	10	161	59	74	107	45

Inspection of the table will convince the reader that the failure of the frequent to drain off strength from the infrequent is not due to the differences in frequency being small and due to mere chance. They are very large. In every *S* the four initially most frequent averaged over four times as frequent as the initially least frequent fourteen. In 17 *S*s the average initial frequency of the initially most frequent four was over ten times that of the ten initially least frequent.

We may, subject to the validity of one assumption, use the changes in the frequencies of the numbers 1 to 6 just as we have used the changes in the frequencies of the letters. The assumption is that the amount of influence of an after-effect in the form of announcements of "Right" and "Wrong" is proportional to the number of times it occurs. The number of rewards (and of punishments) associated with any number is proportional to the frequency with which that number is chosen, since the six numbers were equally often "Rights." The six numbers will then have, except for chance, the same percentage of rewards, and, if the assumption is valid, the strengthening by "Right" (and weakening by "Wrong," if such occurred) would, except for chance, have the same proportions for 1, 2, 3, 4, 5, and 6 as their relative frequencies had. There is no reason to doubt this, though it is conceivable that a "Right" might have more influence upon the general tendency to choose the number in the first n occurrences (say, five) than 1/K times its influence in Kn occurrences (say fifteen).

TABLE II

THE FREQUENCY OF CHOICE OF EACH NUMBER BY EVERY S IN THE FIRST 400 RESPONSES (F) AND IN THE LAST 400 (L) OF 4800

Number	1		2		3		4		5		6		7		8	
	F	L	F	L	F	L	F	L	F	L	F	L	F	L	F	L
1	22	59	59	49	70	65	101	62	79	49	41	64	164	108	42	97
2	47	59	66	65	61	77	65	88	102	90	71	92	139	56	103	91
3	63	58	90	90	71	82	115	78	73	84	127	80	31	67	125	58
4	110	104	60	57	76	74	65	60	105	82	49	76	32	63	44	43
5	80	87	80	77	68	55	27	46	30	63	76	44	21	40	55	79
6	78	33	45	62	54	47	27	66	11	32	36	44	13	65	31	32

	9		10		11		12		13		14		15		16	
	F	L	F	L	F	L	F	L	F	L	F	L	F	L	F	L
1	46	81	61	99	72	98	22	91	26	73	48	64	39	46	47	75
2	68	62	63	83	118	51	125	83	120	79	93	92	58	64	90	62
3	75	83	85	50	83	58	109	64	93	93	96	74	99	82	70	70
4	62	60	99	85	58	61	87	55	81	66	71	65	67	66	78	85
5	80	80	31	48	48	65	31	54	29	52	54	59	37	77	67	55
6	69	34	61	35	21	67	26	53	51	37	38	46	100	65	48	53

	17		18		19		20		21		22		23		24	
	F	L	F	L	F	L	F	L	F	L	F	L	F	L	F	L
1	48	59	89	49	11	72	75	100	48	52	28	65	57	109	47	77
2	95	66	48	93	49	64	80	105	32	65	61	70	84	116	102	70
3	86	65	78	78	156	90	59	36	71	82	126	101	80	78	71	54
4	73	73	35	71	117	75	80	38	86	71	78	51	73	77	97	68
5	49	75	64	56	41	56	65	55	107	88	70	67	52	7	54	71
6	49	62	86	53	26	43	41	66	56	42	37	46	54	3	29	60

It is also conceivable, and somewhat more probable, that the gross number of rewards had undue impressiveness, swamping that of the rewards for less frequently chosen numbers. This would work in favor of the waxing of the more frequent at the expense of the less frequent.

Table II shows the frequency of choices of each number by every *S* in his first 400 and last 400 choices. The initially frequent do not gain. Taking for each person the sum of the two numbers most frequently chosen in the first 400, we find a loss in 20 of the 24 *S*s. The two that were least frequently chosen initially gain in 22 of the 24 *S*s. There is naturally not the enormous difference in frequency here that was found for the choices of letters, but the ratio of the average of the two initially most frequent to the average of the two initially least frequent is over 3.0 in 9 *S*s and over 2.0 in 17. These differences in frequency are large enough to cause substantial drainage from the weak to the strong in the intervening 4000 choices if any differences could cause such a drainage.

In the case of both the letters and the numbers, the initial tendencies do not remain stable (within the limits of chance) but the weak (or infrequent) tend significantly to gain at the expense of the strong. This is a matter worth investigating and I shall return to it later.

The next set of data are derived from the choices amongst four positions on a line (1 = left end; 2 = next to left end; 3 = next to right end; 4 = right end), among the choices neither rewarded nor punished in an experiment of Lorge and Thorndike in which the responses to some lines were rewarded when right and punished when wrong, whereas the responses to other lines were never rewarded or punished. Among the lines subject to reward and punishment, each position was equally likely to be rewarded.

For each *S* we have 2548 choices which, as recorded, fall into five groups of 512, 512, 512, 504, 508 respectively in order of time. For convenience we have raised the last two groups to equivalence with the former by multiplying their entries by 1.013 and 1.008.

Table III records the results for the 9 *S*s of the experiment. The most frequent in choices 1 to 512 lost in every set of 512 thereafter in 8 *S*s and in three out of four sets in the ninth. The most frequent in choices 1 to 1024, gained in the next 1024 in 5 *S*s (just barely in one of them) and lost in 4. In the last 1524 (raised in Table III to 1536) following, the most frequent in choices 1 to 1024 gained in 2 *S*s, lost in 6 and just barely lost in one.

In a similar experiment, but with records involving 1760 choices by each of 10 *S*s, the most frequent choice in the first 176, or the first 352, or the first 704 did not show a gain in the last 176, or 352 or 704. On the contrary it showed a loss in 9, 9, and 8 *S*s, respectively, by these three computations.

In all these experiments there is not an iota of evidence that the mere frequent connection by its frequency (or the stronger connection by its

TABLE III

THE FREQUENCIES OF CHOICES OF 1, 2, 3, AND 4 IN SUCCESSIVE FIFTHS OF 2548 CHOICES

S	Choice	Frequencies					
		1–512	513–1024	1025–1536	1537 to 2044 × 1.013	2045 to 2548 × 1.008	1–1024
I	1	145	166	140	184	195	311
	2	131	125	136	123	120	256
	3	157	137	139	128	125	294
	4	79	84	97	77	72	163
II	1	89	90	104	105	98	179
	2	169	149	161	148	152	318
	3	187	165	139	156	157	352
	4	67	108	108	103	115	175
III	1	130	138	154	165	140	268
	2	115	141	101	121	129	256
	3	160	134	141	124	122	294
	4	97	99	116	102	131	196
IV	1	125	161	157	131	136	286
	2	100	102	104	112	114	202
	3	146	114	110	137	128	260
	4	141	135	141	132	134	276
V	1	84	65	126	122	76	149
	2	199	161	200	183	140	360
	3	146	179	111	128	190	325
	4	83	107	75	79	106	190
VI	1	122	147	138	143	121	269
	2	132	93	97	109	106	225
	3	156	107	113	105	160	263
	4	102	165	164	155	123	267
VII	1	118	183	172	122	118	301
	2	111	131	153	147	137	242
	3	136	112	117	124	147	248
	4	147	86	70	119	110	233
VIII	1	156	100	116	124	120	256
	2	120	131	119	110	104	251
	3	148	152	165	165	155	300
	4	88	129	112	113	133	217
IX	1	90	124	117	136	138	214
	2	112	138	163	162	129	250
	3	154	136	133	123	151	290
	4	154	114	99	91	93	268

strength) waxes at the expense of the less frequent (or weaker). The departures from the status quo are all in the opposite direction of strengthening of the less frequent at the expense of the more frequent.

Although it is not the purpose of this article to explain the shifts in choices, and additional experiments would be required to do so with surety, certain facts may be noted.

A certain fraction of them is, of course, statistical, due to the fact that the initially most frequent are so to some extent by mere chance, and will by chance regress toward the average status. This fraction is very small in cases where the number of choices taken as a sample of initial status is 400 or more.

A certain fraction of them may be due to a refractory period or phase developing from very frequent operation of a connection. This can hardly explain all, however, since the early and late choices were days apart.

A certain fraction of them may be due to a slight satisfaction in the form of relief when a less used response is given in the place of one that has become slightly annoying by its monotony.

A certain fraction of them, especially in the choices among the 26 letters, is due to the fact that the choices of letters are partly determined by ideas or short-lived habits of one sort or another and that these ideas and habits change from time to time in various ways. It is interesting to observe that the choices among the 26 letters diverged significantly from chance in all of the 24 *S*s. Four of the 26 letters had almost or quite 10 twenty-sixths of the first 800 choices in every *S*. In every *S* but one,[1] some letter had twice its chance frequency (31-) in the first 800 choices, and some letter less than one-tenth of its chance frequency.

Among the causes which made *S* have habits of preference, some may, and probably were, more or less temporary in their action. For example, some *S*s doubtless were influenced in their choices of letters by some characteristic of the word, such as its initial letter, or final letter, or dominant sound, or whatever in it caught attention. An acquisition or abandonment of such a habit or a change in it, could obviously cause notable shifts in the frequency of choices.

In the case of the choices of numbers, if, for any reason, an *S* got the notion that he was more often right when he chose a certain number, there would be satisfaction in choosing it which could strengthen the tendency. If he came to distrust this notion, there would be cause for a new shift.

In ordinary life, mental connections are under a powerful and pervasive control from their after-effects: Satisfactory after-effects tend to cause continuance of a connection if the situation continues. If it vanishes, they still cause a strengthening of it, *i.e.* an increased probability of recurrence of it when the situation recurs. Annoying after-effects often cause the subject to change to a different connection if the situation continues. So there is progress toward a status of satisfaction, and stability when it is reached. When any one connection is as satisfactory as any other the brain's choice amongst them depends upon causes about which little is known, because such choices have been so little studied. They are well worth study.

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[1] In that *S*, the frequencies in the *last* 800 choices did diverge enormously from chance.

Two things we know about them. They are sensitive and shift notably; their shifts are not in the direction of strengthening the more frequent or strong at the expense of the less frequent or weak.

THE SUSCEPTIBILITY OF PROSE TO RETROACTIVE INHIBITION

By JOHN A. McGEOCH and FRED McKINNEY, University of Missouri

The generality of the hypothesis that the phenomena of forgetting are to a large degree functions of interpolated events needs to be tested upon a wide sampling of materials.[1] Analysis of results from different materials may, moreover, be expected to shed light upon the conditions of retroactive inhibition. The present paper reports an experiment designed to measure the susceptibility of two different sections of meaningful prose to inhibition from the interpolated learning of other meaningful prose. In an effort to extend our knowledge of the character of the phenomenon of retroactive inhibition, itself a second condition of interpolation, the learning of a list of nonsense syllables, has also been included.

The prose paragraphs used in both of the experiments to be reported were written specifically for the purposes of these experiments and were based upon the information provided in Chapters 1 and 2 in Garrett's *Great Experiments in Psychology.* Two long sections, one on types of tests and the other on individual differences in intelligence, were prepared in such a way that, when divided in halves in terms of number of lines, each half contained 20 specific facts or ideas. The first half was used in each case as the material for original learning and the second half as the material for interpolated learning. The general theme is, thus, the same in both original and interpolated materials. The *S*s were instructed to learn the facts and ideas of each material as completely as possible in the time given. The amount learned was tested by the answers to 20 questions, each of which was worded in a way to avoid any suggestion of the answer and at the same time to demand specific recall of one of the 20 specific items in the material studied.[2] The scoring was, save for a few answers which seemed to warrant partial credit, either right or wrong. The total score possible is, thus, 20 in each case. In the condition which employed nonsense syllables as interpolated material lists of 20 syllables of mixed associative value were studied for the same length of time as was the prose, but were recalled, without questions, by the method of retained members.

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* Accepted for publication April 3, 1933. This is the fifth of a series of Studies in Retroactive Inhibition. The others are: I. The temporal course of the inhibitory effects of interpolated learning, *J. Gen. Psychol.,* 9, 1933, 24-41; II. Relationships between temporal point of interpolation, length of interval, and amount of retroactive inhibition, *ibid.,* 44-55; III. Retroactive inhibition in the learning of poetry, this JOURNAL, 46, 1934, 19-33; IV. Temporal point of interpolation and degree of retroactive inhibition, *J. Comp. Psychol.,* 15, 1933, 407-417.

[1] One of the writers has elsewhere brought together evidence in support of the major importance of interpolated events as determiners of forgetting. See J. A. McGeoch, Forgetting and the law of disuse, *Psychol. Rev.,* 39, 1932, 352-370.

[2] The two following questions are representative: (A) What percentage of the population is feebleminded? (B) What was the average Alpha score made by white enlisted men?

Both experiments have been conducted with groups of *Ss* and, in order to prevent the *Ss* from copying from each other, one section of prose was used with those in the odd-numbered seats and another with those in the even-numbered seats.[3] This precaution, together with the careful watch maintained by *E* during the writing of the recalls, has reduced the variable of copying very nearly to zero. Each section was presented to *S* mimeographed on a single sheet and 5 min. were allowed for study. Immediately after this period *S* wrote his answers to the 20 questions for which 5 min. were allowed. After an interval of 15 min. from the end of this recall *S* wrote a second set of answers to the same questions for which 5 min. were allowed. One week later, at the same hour, a third 5-min. recall was written in answer to the same questions. Exactly the same procedure was followed when the nonsense syllables were employed as the materials for interpolated learning, except that learning was tested by the method of retained members under instruction to write down as many of the syllables as could be recalled. The general procedure was a constant for all groups and under all conditions.

The variables under experimental control are, of course, the interpolated events which fill the first 15-min. interval. Under the rest or control condition the *Ss* took the Seashore pitch discrimination test for 15 min., writing their judgments on a previously prepared record sheet. This is far from a "rest" condition in the sense of freedom from mental activity, but it requires no learning activity, the variable which we wish to eliminate during this interval, and some device must be used to prevent untrained *Ss* from rehearsing. Under the first work condition the *Ss* began to study the interpolated section of prose, either II or IV, immediately following the completion of the recall of the original section. They studied this interpolated section for 5 min., immediately wrote during the next 5 min. their answers to the 20 questions about it, and then spent the remaining 5 min. taking the Seashore pitch discrimination test. Under the second work condition, the *Ss* began to study 20 nonsense syllables in two lists of 10 each immediately after the first recall of the original material, under instruction to study the first list for 2 min., then at a signal from *E* to begin the second list, study it for 2 min., and finally at a second signal to spend 1 min. reading over the two lists together. After 5 min. of study, he spent 5 min. in recall and then 5 min. taking the Seashore test. These two conditions differ from the rest condition only in the interpolated learning and recall, while they differ from each other only in the character of the interpolated material.

Three different groups of *Ss* were employed, one upon each condition, and the alternate members of each group learned different sections of the material under otherwise identical conditions. At the beginning of the experiment each *S* was given all of the material required by the experiment, including in the case of the second work condition blank sheets for the written recalls of the interpolated nonsense syllables. The materials were clipped together in order, with the proper designation written on the back of each. At each successive step in the experiment *S* had only to place the sheet at which he had been working face down at the bottom of his packet of material, turn over the next sheet at the proper signal and proceed. It was possible in this way to adhere exactly to the time schedule described. At the end of the second recall the *Ss* were given detailed and rigid instructions against rehearsal. All *Ss* were students in General Psychology. The experiment was conducted early in the semester and the relatively technical material of the prose sections was unknown to them.

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[3] The sections on types of tests are numbered I and II, those on individual differences are numbered III and IV. Sections I and III were learned as original materials and Sections II and IV as interpolated materials.

The procedure just described was followed in both experiments except for one difference which distinguishes the two from each other. In Experiment I recalls were taken of the original material, as described, immediately, after 15 min., and after 7 days. In Experiment II the 15-min. recall was omitted and only the recalls immediately and after 7 days were taken. It is, thus, possible to discover whether the distributed practice afforded by the recall after 15 min. has affected the results after 7 days. The procedure used in these experiments parallels exactly, except for the difference in material, a prior experiment upon the susceptibility of poetry to retroactive inhibition.[4] The report of the present experiments is presented in as nearly as possible the same form in order to facilitate comparison of the results from these two kinds of meaningful material.

TABLE I

ITEMS CORRECTLY RECALLED: MEANS, SIGMAS, PERCENTAGES OF RECALL AND OF RETROACTIVE INHIBITION

		Time of recall						
Condition	N	Immed.	After 15 min.			After 7 days		
		Mean	Mean	%Rec.	%R.I.	Mean	%Rec.	%R.I.
			Section I					
Rest	37	13.77 ± .49	15.47 ± .47	112.3		13.70 ± .58	99.4	
Work (Prose)	29	14.03 ± .42	14.88 ± .42	106.0	05.6	11.79 ± .62	84.0	15.4
Work (N.S.)	36	14.80 ± .56	15.80 ± .48	106.7	04.9	13.76 ± .50	92.9	06.5
			Section III					
Rest	37	17.51 ± .38	19.00 ± .26	108.5		18.70 ± .30	106.7	
Work (Prose)	24	17.83 ± .41	18.19 ± .34	102.0	05.9	17.85 ± .46	100.1	06.1
Work (N.S.)	35	17.37 ± .30	18.65 ± .18	107.3	01.1	17.97 ± .27	103.4	03.0

Retroactive inhibition has been computed in terms of the percentages of recall after the two intervals. The percentage of recall is found by dividing the mean raw score at immediate recall into the corresponding score at each delayed recall. The percentage under each work condition is then subtracted from the percentage under rest and the difference divided by the percentage under rest to give the relative amount of retroactive inhibition.

RESULTS

Experiment I. The data for the two sections of prose will be presented separately. This permits a comparison of the results from two different samplings of the material and will lend increased significance to the conclusions. In Table I are given the mean numbers of items correctly recalled (or questions correctly answered), the sigmas of the means, the percentages of recall and of retroactive inhibition. Percentage of recall has been found after each interval with the mean at immediate recall, which is the measure of original learning, in the denominator.

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[4] J. A. McGeoch and F. McKinney, Retroactive inhibition in the learning of poetry, this JOURNAL, 46, 1934, 19-33.

Each set of three groups has immediate recalls which are as close together in magnitude as could be expected. Section I is somewhat more difficult than Section III. Both, however, yield recall scores after 15 min. which are higher than 100%, a fact which is readily understandable when one remembers that the recalls were in response to the same questions each time and that a single recall is an incomplete measure.[5] Each section of prose shows a small percentage of inhibition from the interpolated learning of an equal section of similar prose.[6]

The differences involved are not satisfactorily reliable, but the fact that the same difference appears with each section of prose and that differences of similar magnitude appear consistently in other parts of the experiment support the conclusion that the recall of the ideational content of a prose passage is susceptible to a small amount of inhibition from the learning of the content of a similar passage. This amount of inhibition becomes larger in the next recall after an interval of 7 days, and the increase is a function of the prose section, being 9.8% in Section I and but 0.2% in Section III.[7] The latter amount is inconsequential, and the former, while much larger, by no means represents certain evidence of an increase in amount of inhibition with increasing time.

When we turn to the results from the interpolated learning of 20 nonsense syllables, it can be seen at once that very nearly the same statements apply to them. The percentages of inhibition are less in every comparison from the syllables than from the prose, but the differences are not large. There is the same increase with time, although it is less pronounced in the case of Section I, so that the difference between the inhibition produced by the two conditions is here the largest of all. The interrelationships are highly complex and an assessment of their statistical validity would rest upon assumptions of equal complexity. If, however, we depend for assurance upon the consistency of the direction of the differences following each interpolation, we may conclude that the nonsense syllables have a small amount of inhibitory effect but one which is only a little less than that produced by the learning of the prose and one which parallels the variations of the latter with material and with interval.

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[5] Cf. W. Brown, To what extent is memory measured by a single recall? *J. Exper. Psychol.*, 6, 1923, 377-382.

[6] The interpolated sections of prose were learned slightly better in each case than were the original sections. The immediate recalls of the interpolated syllable lists varied between 6 and 8.

[7] The implications of the data are not greatly affected if we use the recall scores after 15 min. as the basis for computation of percentages of recall after 7 days.

Experiment II. This experiment is an exact reproduction of the first except that the second recall, after 15 min., has been omitted and that three new groups have been employed with each section of prose. It permits an estimate of the influence of this second recall upon the amount of inhibition after 7 days and provides an independent statement of the susceptibility of the two passages to inhibition. The results (Table II) show once more a consistently appearing but small amount of inhibition, although here the interpolated syllables produce a larger decrement in one case than does the interpolated prose. In conjunction with the other data this probably means only that the two interpolations are not far apart in the amount of their influence. Three of these percentages of retroaction are slightly larger than those at recall after 15 min. in Experiment I, and three are slightly smaller than the corresponding percentages after the 7-day interval of the first experiment. The two experiments yield evidence of a slight but statistically unimportant tendency for retroaction to increase in amount from 15 min. to 7 days and no evidence that the second recall, after 15 min., has had any marked influence upon the amounts of inhibition after 7 days.

TABLE II

ITEMS CORRECTLY RECALLED: MEANS, SIGMAS, PERCENTAGES OF RECALL AND OF RETROACTIVE INHIBITION

Condition	N	Time of recall			
		Immed.	After 7 days		
		Mean	Mean	% Rec.	% R.I.
			Section I		
Rest	27	14.44 ± .61	11.81 ± .68	81.8	
Work (Prose)	33	13.04 ± .47	9.80 ± .61	75.1	08.1
Work (N.S.)	33	13.69 ± .53	9.33 ± .66	68.1	16.7
			Section III		
Rest	21	16.11 ± .53	16.35 ± .42	101.4	
Work (Prose)	26	16.67 ± .45	16.14 ± .53	96.8	04.5
Work (N.S.)	32	16.62 ± .38	16.46 ± .41	99.0	02.3

DISCUSSION

The results of two experiments have uniformly shown that prose content is susceptible to a small degree of inhibition from the learning of a second section of prose of the same kind, that there is a slight but somewhat uniform tendency for inhibition to be larger after 7 days than after 15 min., and that there is no uniform variation in results following the omission of the recall after 15 min. The interpolated learning of 20 nonsense syllables is followed by a decrement which is only a little less than that which follows the interpolated prose. These results present three general questions for discussion. (*1*) How do these results compare with those from our earlier experiments upon the susceptibility of poetry to retroactive inhibition? (*2*) What is the importance of the fact that prose is susceptible, even though slightly, to a decrement from interpolated learning? (*3*) What is the significance of the fact that the interpolated learning of nonsense syllables has almost as large an inhibitory effect as does the learning of very similar prose content? We shall consider these questions in order.

(*1*) The present experiments have been performed under conditions which directly parallel those employed in the experiments with poetry and the results may, therefore, be directly compared. In the earlier experiments percentages of inhibition were found in terms of number of lines correctly recalled, number of which the meaning was correct, and number of which the meaning was only partially correct. The present experiment yields only a single score, the number of questions correctly answered. This score is not the direct analogue of any of the three employed with the poetry, but it is most closely comparable to the number of lines correct in meaning, since the answers were not required, except when the nature of the question obviously demanded it, to be recalls of the wording of the passage but only of its meaning. The amount of inhibition found with the prose, when other prose is interpolated, is less than that found with poetry, when other poetry is interpolated and when the scoring is in terms of meaning correct or of meaning partially correct. The prose is roughly equal to the poetry in susceptibility to inhibition when the poetry is scored in terms of lines verbally correct. Since all scores must be considered in evaluating the results with the poetry and since the retroactive effect is in one sense the total of all, it must be concluded that prose is less susceptible to inhibition than is poetry under the conditions of these experiments. This is especially true after the 7-day interval and still more so when the recall after 15 min. has been omitted. The increase in retroaction following the omission of this recall with the poetry does not appear with the prose. It may also be concluded that the interpolated learning of nonsense syllables is followed by a smaller decrement in the recall of the prose than of the poetry.

It is probable that the differences between the results with the two different materials are in large part a function of the method of recall. The number of associative cues presented at the recall of the prose was much greater than the number offered in the case of the poetry. The prose recalls were in response to 20 individual questions, each one of which, although framed to avoid suggestion of the answer, would necessarily be connected with it. The poetry recalls were, on the other hand, given in response to the single stimulus of the name of the poem and the words of the instructions and, although each line recalled would be a cue to the next, each line partially forgotten would introduce a break in the direct sequence of cues and would to that extent render recall more difficult. This circumstance would account for the increase in retroaction with time interval as well as for its greater amount after 15 min., since the forgetting which followed the non-experimental interpolations of everyday experience and which appeared under the rest condition meant a progressive diminution of the correct sequential cues to the recall of the poetry, while such forgetting could have no influence upon the cues (questions) to the recall of the prose. The results of the two experiments are in complete agreement upon the question of the susceptibility of meaningful material to a decrement from interpolated learning. The differences in amount and in change of amount with interval are, according to the interpretation given, functions of differences intrinsic to the method of measuring the recalls of the two materials.

(2) We shall now turn to the second question, that of the meaning of the small amounts of inhibition found with the prose. They must be interpreted in the light of their general setting. The conditions of these experiments approach the conditions under which human beings actually learn and recall. The method of complete presentation with continuous reading is the one by which students usually study and by which meaningful material is read by anyone. The testing of learning and of delayed recall by answers to questions upon the material approximates the method of the examination or any situation in which one is asked to reproduce piece-meal the specific content of previously read material. We have sought in this way to test further the generality of the hypothesis that inhibition from interpolated learning is a basic condition of forgetting. If retroactive inhibition is a phenomenon which is found only in the verbatim reproductions and with the serially learned materials of the usual laboratory investigations, its significance is narrowly limited. If, on the other hand, it appears under conditions such as we have employed in these experiments, its generality is increased and the hypothesis that it is a major determiner of forgetting in extra-laboratory as well as in laboratory situations is supported. The fact of the smallness of the amount of this inhibition should not be allowed to weigh against its importance. The uniformity of the inhibition with two different sections of the material and in two separate experiments is strong evidence of its validity. That the amount should be small is but to be expected from the fact of the relatively gentle decline in curves of retention for prose as compared with those for disparate serial material.

THE INFLUENCE OF INSTRUCTION AND CONTEXT UPON PERCEPTIVE SEARCH

By HOWARD L. KINGSLEY, Boston University

When the *Os* of a previously reported experiment on search[1] were instructed to find an obscure object in a presented picture, it was found that they frequently looked for something quite unlike the object named in the instruction. Here the object as derived from verbal instructions did not coincide with the perceived object. This inadequate formulation of the search-task occasioned delays, confusion, annoyance, and even failure to consummate the search, for it left *O* unprepared to recognize, when he saw it, the thing he had been instructed to find. Because of this discrepancy between the task as understood and the object actually perceived, further experiments were arranged in which the obscure object was to be visually presented with the instruction.[2] In order to secure comparable results, pictures were mounted (as before) on gray cardboard (9 x 12 in.). From an extra copy of each picture a small part was cut and mounted on a cardboard (2 in. sq.), bearing, without words or other context, an exact duplicate of some object to be found in the larger picture.

Experiment I. O was instructed as follows:

> At the 'ready' signal you are to fixate the center of the screen. At 'now' a small picture will be presented. Observe it carefully. It will be quickly withdrawn and then a large picture will replace it. You are to find in the large picture the object shown in the small picture. When you have found this object, press the button, and immediately report the total experience of the exposure-period.

The small picture was exposed for 2 sec. When the picture was seen by *O*, significant forms usually stood out at once, though sometimes the impression was at first confused and then "the meaning emerged gradually" (*B*), or "verbal kinaesthesis gave meaning to the visual experience" (*Br*). Verbal kinaesthesis was abundant. It served to name the object and sometimes it carried descriptive comment (*e.g.* "fish—under water"), or antici-

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[1] H. L. Kingsley, An experimental study of search, this JOURNAL, 44, 1932, 314-319.

[2] The writer acknowledges indebtedness to Dr. Ethel Bowman (*B*) and Miss Elizabeth Rutherford (*R*), members of the psychological staff, and to Theresa Berney (*Br*), Charlotte Towson (*T*) and Anna R. Meeks (*M*), major students in the psychological laboratory of Goucher College.

patory comments (*e.g.* "flag—easy to find," "must be a beach scene," "this picture will be in black and white").

Since the first perception served as an occasional instruction directing anticipation, self-instruction also was frequent; (*B*: "pay close attention," "hold the image if you can," and "look for it in any position;" *T*: "look for characteristics in this picture that will help identify it in the text;" *M*: "try to take in all details of the small picture").

During the interval (2 sec.) there was always for *R* a definite visual image of the object just seen. For the other *O*s there was sometimes a visual image, sometimes perception of the gray screen, apprehension of the experimental situation, or silent repetition of the name of the object perceived. For *T* the visual image faded quickly and the object was carried by kinaesthesis. Anticipation of the setting of the object in the large picture was frequently reported; *e.g.* "I thought she must be sitting on a beach. I expected to see a beach" (*Br*), "I tried to place the object in its natural surroundings before the exposure of the second picture" (*M*). Self-instruction frequently appeared in anticipation of the impending search. When a house was shown in the small picture, *Br* instructed herself to "Look in the distance; it will be in the distance."

As soon as the large picture was exposed *O* began to search for the duplicate of the insert. The reports generally confirm the results of the previous experiments with regard to visual exploration and discovery; but the visual preview was responsible for certain differences. These we must discuss.

(*1*) *Exploration.* While large or brightly colored parts were occasionally perceived at once, there were, as a rule, few clear perceptions attained prior to the discovery of the sought insert. "I did not notice anything else in the picture. All was a confused mass until I clearly saw the cow. Nothing in the foreground was clearly apprehended" (*Br*). Strain and pressures, particularly about the eyes, and the shifting visual patterns carried the searching. "The harder the search, the greater the strain" (*Br*). Verbal kinaesthesis frequently implied "no flag there," "can this be it?", and other comments.

Visual images frequently referred to the sought object. With *R* they were replicas of the perceptive patterns of the small picture, persisting throughout the search and disappearing on discovery. *B* and *Br* often reported no visual surrogates. *M* used auditory-visual imagery during the interval but lost it when she looked at the details of the large picture. *T* "tried to hold the visual image, which was clear at first, but faded," giving place to kinaesthesis.

The visual designation proved superior to the oral method for giving *O* a correct and usable anticipation. But perceptions are not wholly determined by stimulus pattern,[8] and an object which is a part of a complex situation is not quite the same thing as it is in isolation.

In the initial perceiving of the isolated figure, *O* frequently supplied an imaginal context. A fish hanging on a line was "vaguely filled in by supplying a man with a rod and reel" (*R*), and a woman in a bathing suit with a striped parasol was supplied with a "visual background which meant *beach*" (*B*). Since the object sought was thus in part determined by imaginal factors contributed by the individual *O* it did not always match the object sought in the large picture. When it was at variance with the realized context it tended to delay or hinder discovery. (*E.g.* a cut-out of a light fixture was completed in imaginal terms to become a wall fixture. *O* was then "set to discover a fixture on a wall." But in the larger picture *O* found the wall covered with filled bookcases and only later discovered the fixture suspended from the ceiling.) The effect of this context was the same whether the supplementing came with the cut-out or was later supplied by search during the brief interval or while exploring the large picture. (*E.g. Br,* looking for a trowel, sought it in the hand of some person in the picture, but finally, after misdirection, found it lying on the ground.)

(2) *Discovery.* When finally apprehended, the sought object is clear and focal and is viewed with relief. "The clearness of the thing sought brings relaxation," and "there was a feeling of relief and relaxation when I found it" (*Br*).

All *O*s declared it easier to find the objects in this experiment than in the experiment where they were designated orally. "It is easier and more definite to have two visual things to match than to have a visual and a verbal experience" (*B*). The following comparison was made by *M*; "When the object was merely named you had to create your own image. Here you know just what it will look like, in color, shape, and size. This gives one a different attitude toward the task. It requires less effort. The search-performances with verbal designation were somewhat more interesting than these because you didn't know just what you were going to see. You had then to take some distinguishing feature of the class to which the object belonged, such as the beak of a bird or the wattles of a turkey, and watch for it. In this experiment you look over the picture as a whole, concentrating on some such aspect as the size, color, or form, of what you have seen in the preliminary exposure." *R* commented that "discovery is more immediate and more easily accomplished in this experiment. There is no conflict between the visual image and the perception, as often happened in the previous experiment. Here just a small part of the object in the periphery of the field of vision leads one to glance at once to the object. There is less aimless wandering of the eyes."

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[8] M. Bentley, Environment and context, this JOURNAL, 39, 1927, 54-61.

Experiment II. The context. In Experiment I the thing sought was always an integral part of the larger picture, definitely related to the other parts. Because of these contextual relationships, *O* was able to anticipate to some extent the location of the object sought, or at least to eliminate certain sections of the picture as altogether unpromising. The task was somewhat like that of the huntsman looking for a deer in the forest. He does not waste his time looking for game in the tree-tops. The purpose of the present experiment was to study search where no clues could be obtained from the arrangement of the constituent parts of the field of search.

From 50 to 60 small pictures of miscellaneous objects were pasted in irregular order on each of 12 large cards (9 x 12 in.). Each small picture was a separate unit which bore no relation to the others, except that of spatial proximity. A duplicate of one of the 50 or 60 pictures appearing on each of these cards was pasted on a small card (2 x 2 in.). The instructions were the same as before. Search was now limited to the place of the previewed small picture in the large heterogeneous group. It is marked, when the large group is presented, by peripheral vision, quick shifts of fixation, pressures about the eyes and shoulders, persistence of the visual image of the object sought, scanty perception of other objects, clarity of the object when found, and announcement of discovery in verbal terms (usually "there"). The following is a typical report by *R* (small picture of a chicken). "Visual image. Upon large exposure the center was first fixated. Rapid shift of fixation to the left. The chick appeared in peripheral vision. Image disappeared. Further shift made it foveal. Clear perception. The eye-movements were rapid, with pressures about eyes and shoulders. The other objects were not separately apprehended as figures."

B substituted verbal tags for visual images. Sometimes fixation halted on a group of lines while verbal kinaesthesis labeled the clarified object. Sometimes an attitude "not there" or a verbal labeling and rejecting came.

The distinctive feature of search under these conditions was the rapid running over of many "rejected" objects with no clear perception but with the realization that they were not the object sought.

Experiment III. Search for numbers. In the two preceding experiments *O* knew the appearance of the sought object because she had seen a duplicate of it. Here recognition was immediate, certain, and satisfying. Now if the visual object was so standardized and familiar as to leave no doubt that it was the visual counterpart of the verbal symbol, it would seem that the oral designation should be quite as adequate for immediate and certain recognition as the visual designation. To test this supposition printed numbers were now used for search-objects, and the one to be found was given orally by the experimenter.

Six exposure cards (9 x 10 in.) were used. On each were pasted from 50 to 55 numbers. These numbers ranged from 1 to 30, were ¼-in. in height, and were printed on small squares and scattered about on the large card in an irregular way. Some were upright, others tipped, and a few were lying on the side. The instructions were the same as before except that *O* was instructed that she would be asked to find on the large card a certain number to be spoken (*e.g.* "7" or "27") just before the exposure. Thirty-six numbers were used.

The auditory perception of the number was followed either by a visual image of the number, printed in black, and verbal (throat) kinaesthesis (*R*), or by a combined vocomotor-auditory image (*B*). With *B* verbal kinaesthesis played the dominant rôle in defining and sustaining the task. Search was carried on here with the same general plan as in Experiment II. But peripheral vision was less effective, and *O* made more use of self-instructions and of comments (*e.g.* "neglect all but one-digit numbers," "this number is not quite right"). The same promptness and certainty in recognition appeared as with the visual designation of the earlier experiments.

The context was the same here as in Experiment II. The large card gave no clues of the sought number until this number was apprehended.

Experiment IV. The time element.[4] This experiment was undertaken to determine whether the visual method of designating the search-object used in Experiments I and II makes for quicker discovery than the verbal descriptions used in the earlier experiments. The same materials and procedures were used. Ten untrained students, divided into two groups, served as subjects. Both the visual and the oral methods of designating the search-object were used. One group was given even-numbered cards and the other the odd-numbered. The oral method was the same as before save that here *S* was instructed what to find just before, instead of just after, the picture was exposed. Haste in announcing the discovery was cautioned. By means of a stop-watch, *E* measured the time which elapsed between the beginning of the exposure-period and the announcement of discovery. Each *S* had six trials under the oral method and six under the visual method in each of two series A and B which corresponded to Experiments I and II—24 trials, or 240 for the 10 *S*s.

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[4] This experiment was conducted in the psychological laboratory of The College of Practical Arts and Letters of Boston University. The writer was assisted by Dorothy L. McAlister and is indebted to her for the time records.

The search-times for all *S*s under the oral method in the A-series is 3.1 sec., while for the visual method in this series it is 2.8 sec. with significant m.v. The median time for the oral method, B-series, is 3.8 sec., while for the visual method, B-series, it is 3.4 sec. Thus it appears that the visual method made for somewhat quicker discovery in both series.

Moreover, search required more time where the context was irrelevant than in the series where clues were obtained from the context. Under the oral method the median time was 3.1 sec. for the A-series and 3.8 for the B-series. Under the visual method the median time for the A-series was 2.8 sec., while for the B-series it was 3.4 sec.

Conclusions. These experiments have confirmed the previous series relative to visual exploration in a search-performance. Search and apprehension follow the same general course where the object to be found was verbally described or was visually presented. But the visual method of designating the search-object gives *O* a more adequate preparation for discovery than does the verbal method, it affords a greater degree of confidence, makes the task easier, and insures prompt, complete, and certain recognition of the object. It leads also to a quicker discovery.

Printed numbers, designated orally, were recognized quite as immediately and with as much certainty as were the miscellaneous objects designated by the visual method.

When *O* is precisely shown what to find, there is a tendency to anticipate the forthcoming context. If this anticipation is approximately correct it facilitates discovery; if in error it retards.

Where the search-field is so structured that the search-object sustains natural relations to its surroundings, the context usually affords helpful suggestions concerning the probable place of the object; but where the context is a mass of unrelated items, no clues are provided by it and the search becomes a systematic survey of the field in which only objects similar to the object sought are clearly perceived. More time is required to find an object when its context is irrelevant.

THE INFLUENCE OF VISUAL AND AUDITORY PATTERNS ON TACTUAL RECOGNITION

By THEODORA M. ABEL, Sarah Lawrence College

The object of this experiment was to study the influence of visual and auditory modes of presenting stimuli on tactual discrimination, thus employing a different type of learning condition from the more usual requirements of memorizing nonsense or meaningful words or sentences.

In investigations on verbal learning the results have often been conflicting, some favoring visual, others auditory presentation methods.[1] Differences in procedure and in experimental task-conditions produce differences in results.[2] But interpretations of the results of a given experimenter frequently do not account for and often conflict with the explanations of another experimenter. For instance, the class-room lecture method favors auditory presentation of learning material for one group of undergraduates,[3] but apparently has no such influence on the learning ability of undergraduates in another experiment.[4] In the latter investigation, visual presentation facilitates the learning of nonsense syllables because of its longer stimulation time for the syllable as a whole.[5] This explanation, however, cannot account for experiments where oral presentation of stimuli has been preferred.

By introducing a task of a less verbal character, in which the possibilities of organization of meaning are more remote than in nonsense syllables, and which require of the *S* direct sensory cues under quite unfamiliar learning conditions, we hoped to reach a better comprehension of this problem, particularly in the direction of interpreting results.

The task was to learn to recognize by touch 12 letters of an alphabet arbitrarily set up but similar to the Braille point system for the blind. Various combinations of 2 to 6 dots were used. The dots were punched on heavy manila paper cards (1.5 in. x 5 in.) by the regular procedure of writing board, frame and stylus employed for writing in Braille, and were shellacked in order to preserve uniformity in size. The stimuli were placed in a covered cardboard box, and were felt through an aperture at the side of the box facing the *S*, large enough for the index finger of the right hand. The left side of the box was open where the cards protruded

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* Accepted for publication April 20, 1933.

[1] For a recent bibliography on the influence of sensory modality, see John A. McGeoch, The psychology of human learning: A bibliography, ref. nos. 672-705, *Psychol. Bull.*, 30, 1933, 1-62.

[2] *E.g.* in employing the anticipation method of memorizing nonsense syllables, Koch found combined visual-auditory and visual presentations superior to auditory presentation (this JOURNAL, 42, 1930, 370-388). The stimuli were presented both visually and orally. On the other hand, Reed found auditory presentation superior to visual presentation in an experiment on paired associates (*J. Exper. Psychol.*, 14, 1931, 632-649). Here the nonsense syllable to be associated with a meaningful syllable was always presented visually.

[3] Reed, *op. cit.*, 643. [4] Koch, *op. cit.*, 374. [5] Koch, *op. cit.*, 384.

(3/4 in.), so that the *S* could slide a card to the back of the box when through with it, without seeing the dots.

Experiment I. The *Ss* were 170 college women, divided into groups of 85 each: a visual and an auditory group. The groups were equivalent in age and on the Thorndike aptitude ratings.[6] Before being presented with a tactual stimulus, the visual group (VP) was shown for 3 sec. a large placard, on the left of which was printed in India ink the name of the tactual stimulus to be felt, and on the right of which was indicated the pattern of the dots to be felt. Before being presented with the tactual stimulus, the auditory group (AP) listened to an auditory pattern of the arrangement of the dots, played by *E* on a wooden block xylophone (concealed from the *Ss*). Three notes, high, medium and low, an octave and the fifth between, were used. The *Ss* were uniformly trained to understand that a sequence from high to low meant a vertical sequence of dots, a sequence of two identical notes meant an horizontal sequence, and a sequence from low to high note meant a diagonal sequence of the dots from lower on the left to higher on the right. The notes were played at the rate of 1.5 per sec.

Two letters not in the series were practiced before the learning began. The learning procedure consisted of two learning trials and a recognition trial for the first 6 letters, followed by two learning trials and a recognition trial for the next 6 letters. Then came a learning trial of all 12 letters, a rest of 5 min., and a final recognition trial of the 12 letters. Five seconds were allowed for feeling each stimulus-card under both learning and recognition conditions. In the first learning trial for each of the sets of 6 letters, the order of presentation of the names of the stimuli was known by the *Ss* (the names being the first 12 odd letters of the alphabet, A-C-E, etc., through W). In all other trials the sequence of presentation was worked out in a chance order and was not known by the *Ss*. The experiments were performed in groups ranging from 5 to 15 *Ss* in number. Scoring was made on the basis of the number of correct recognitions; immediate recognition-scores comprising the results of the first two recognition-trials taken together, and mediate recognition scores comprising the results of the final recognition-trial.

Experiment II. Previously we had performed a similar experiment on another group of *Ss* with a different object in view. We found we could use some of the results of this experiment for comparative purposes and to serve as a control to Experiment I. Ninety-nine *Ss* learned to recognize another set of 12 tactual letters comparable in difficulty to the set in Experiment I.[7] Fifty of these *Ss*, designated as the auditory control group (AC), were told what letter they would feel, and 49 *Ss*, the visual control group (VC), were shown on a large cardboard the name of the stimulus they would feel.[8] Neither group was presented with a pattern, visual or auditory, of the arrangement of the dots. Other conditions for the procedure of the experiment were the same as in Experiment I. As a longer interval had been allowed before the final recognition of the letters, only the scores of immediate recognition could be employed for comparative purposes.

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[6] The ages ranged from 16 to 21 yr. with the average for the visual group at 18.42 ± .679 and for the auditory group at 18.73 ± .659. Thorndike ratings for 59 of the visual group averaged 65.431 ± 7.996 and for 56 of the auditory group averaged 63.227 ± 7.281. We did not have Thorndike ratings for the 55 students from Vassar College. They were equally distributed in the visual and auditory groups, however.

[7] Comparability in difficulty was estimated on the basis of the work of P. Villey, Psychologie de la lecture tactile, *J. de psychol.*, 28, 1931, 213-249, in which he indicated the tactual letters that were remembered more easily.

In Experiment II the names of the stimuli were the first 12 letters of the alphabet (A-L). This might possibly have made a slightly easier task in the recall of the names of the tactual stimuli. As only 2 *Ss* in Experiment I and 1 *S* in Experiment II made the error of assigning a name not in the series, we can assume that this difference was negligible.

Experiment II was conducted during the spring term of 1932 at Sarah Lawrence College. Experiment I was conducted during the fall term of 1933 at Sarah Lawrence College and at Vassar College.[9] The author acted as *E* in all experiments.

Table I gives the results of immediate recognition in Experiments I and II and for mediate recognition in Experiment I, and indicates the reliability of the differences between the averages of the various groups.

TABLE I

AVERAGE SCORES AND DIFFERENCES, EXPERIMENTS I AND II

Group	No. of *Ss*	Av. no. correct scores	
		Immed. recog.	Mediat. recog
AP	85	6.785 ± 1.949	5.529 ± 2.217
VP	85	8.317 ± 1.719	7.788 ± 1.831
AC	50	7.255 ± 2.051	
VC	49	7.019 ± 1.859	

Groups compared		Actual diff.	$P.E._{diff.}$	Chances in 100 for true diff. greater than zero
Immed. recog.	Mediat. recog.			
AP–VP		1.532	.282	100
	AP–VP	2.259	.313	100
AC–VC		.236	.979	55
AC–AP		.470	.366	81
VC–VP		1.298	.317	100

There was no reliable difference in the scores of the AP and of the two control groups. The number of letters correctly recognized for all 3 groups ranged from 6.8 to 7.2. The VP group did better, averaging slightly over 8 letters correct, or 17% better than the AP group. The actual difference between the average of the VP group and of the other 3 groups was reliable.[10]

Experiment III. In another experiment, following Experiment I by exactly 3 weeks to the hour for all *Ss* participating, 38 of the former VP group learned a new set of letters, comparable in difficulty to the first set by the auditory pattern method;[11] and 34 of the former AP group learned these letters by the visual pattern method. The names of the stimuli were the first 12 even letters of the alphabet (B-D-F through X).

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[8] The age ranges were the same as those of the groups in Experiment I. The Thorndike ratings were also equivalent, the average for group VC was 65.789 ± 7.234 and for group AC 63.864 ± 7.1665.

[9] Acknowledgment is due Professor Josephine M. Gleason of the Psychology Department of Vassar College for allowing us to use her students in the introductory laboratory course as *Ss* in this experiment, and to Miss Mary Elizabeth Stanwood for making the stimulus-cards.

[10] An attempt at measuring the reliability of the task was made in correlating the first half with the second half of the mediate scores of VP and AP. The reliability *r* for VP was +0.64 ± .05 and for AP +0.48 ± .06 (product-moments method).

[11] See footnote 7.

As can be seen from Table II, both groups improved by approximately 2 letters in efficiency, but again the VP group did 17% better than did the AP group, the difference being reliable.[12]

TABLE II

AVERAGE SCORES AND DIFFERENCES, EXPERIMENT III

Group	No. of Ss	Av. no. correct	
		Immed. recog.	Mediat. recog.
AP	38	8.184 ± 1.756	7.605 ± 2.282
VP	34	10.114 ± 1.551	9.286 ± 2.2099

Groups compared	Actual diff.	P.E.$_{diff.}$	Chances in 100 for true diff. greater than zero
AP–VP (Immed. recog.)	1.93	.387	100
AP–VP (Mediat. recog.)	1.68	.512	98.7

There is indication that *Ss* who did well in Experiment III did well also in Experiment I. Using the rank correlation method, with conversion into Pearson coefficients, we found the correlation between AP_I and VP_{III} to be + 0.45 ± .09 and between VP_I and AP_{III} to be + 0.64 ± .07. This is in accord with the results of other experimenters which show that individuals who learn best by one mode of presentation of stimuli, learn best by another mode as well.[13]

The *Ss* who acted in Experiments I and II were asked to comment on the visual and auditory methods and to give any reasons that occurred to them for the success of one method over the other (in case one method was found easier). Of these *Ss*, 74% stated that the visual method was easiest: 57% gave positive reasons for their judgments, such as influence of training and habit on eye-hand manipulations, more meaning, better connections and more unification in the visual patterns for the tactual patterns; 17% gave negative reasons for their judgments of the superiority of the visual mode of presentation, such as "sounds too confusing or distracting," "the meaning of the sounds had to be created and required too much effort," or "the sound had to be translated into visual imagery before it made sense in terms of the touch patterns." On the other hand, 17% of the *Ss* expressed the opinion that the auditory method was the easiest because of the fact that they were trained musically and used "to getting" rhythms. Actually, only one-half of these *Ss* had a better score with the auditory method.

DISCUSSION OF RESULTS

In the specific task of learning to recognize by touch small dot patterns, the aid of a visual presentation of the pattern was more effective than the presentation of an auditory pattern. The presentation of the auditory pattern was neither more nor less effective than a no-pattern presentation, where the name of the tactual stimulus was announced either visually or orally. The sense modality through which the name alone, without the pattern of the stimulus, was presented played only an insignificant rôle in the total learning process.

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[12] The actual difference between the average median scores is only 3 times the P.E. of the difference, but with the small number of cases this difference can be considered quite reliable.

[13] G. Bennett, The correlation between different memories, *J. Exper. Psychol.*, 1, 1916, 404-418.

It seems fairly obvious that the more immediate coherence and significance of the visual patterns for the *S*s determined the superior part played by vision in the comprehension and recognition of the tactual patterns. We have seen that the comments of the *S*s indicate the nature of the help visual patterns gave them in interpreting tactual patterns. In fact, 34% of the *S*s used the terms, "more meaning," "better connections," or "more unification" to characterize the influence of the visual patterns.

Since beginning this experiment we have found an instance in which auditory patterns were more immediately significant than visual patterns for learning in a particular task situation. In a report on visual *versus* auditory methods of teaching bi-manual production of rhythmic patterns, Schick[14] stated that one of the reasons the auditory method was superior to the visual in her learning problem, was because of the more immediate comprehension of the auditory rhythms, whereas the visual rhythms required the added difficulty of reading and interpreting notations.

Two factors, it would seem, contribute to the greater significance of the visual patterns in our experiment. The first factor is the influence of the constant integrations being made between touch and vision in the contacts an individual has with the world around him. As the individual develops from childhood to adulthood his tactual experiences become more and more dominated by vision, and he depends more and more on his keener visual discriminations to render more acute his tactual discriminations.

The second factor is the influence of the similarity between visual and tactual sensory organizations. In both fields the "primitive and natural" organizations have in common the quality of spatiality or extensity, so that a visual space pattern can more readily be translated into a tactual space pattern than can an auditory temporal pattern. Possibly we can carry over Lashley's interpretation of the "equivalence of stimuli" within a given sensory field to explain the similar pattern perceptions of two different sensory fields.[15] We may then say that between vision and touch, the patterns of stimuli are more nearly equivalent than between hearing and touch, in the sense that they have in common ratios of proportion in the spatial distribution of excited points.[16]

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[14] H. F. Schick, Proceed. 40th ann. meet. Amer. Psychol. Assoc., *Psychol. Bull.*, 29, 1932, 682-683.

[15] K. M. Lashley, Basic neural mechanisms in behavior, *Psychol. Rev.*, 37, 1930, 1-14.

[16] Lashley, *op. cit.*, 5.

THE COLOR PREFERENCES OF 1032 YOUNG CHILDREN

By THOMAS R. GARTH and ELECTA PENINA PORTER, University of Denver

Previous inquiries into the color preferences of young children have been made, but the groups were usually small so that generalizations are on that account somewhat precarious. Another inadequacy is found in the lack of numerical expressions of the relative "pulls" of the different colors. What is needed is a study of a group large enough to allow for individual differences in so far as it is possible, and a statement of the results in such form that comparisons can be made of a more or less quantitative character.

Studies of small groups have been made by Shinn[1] in 1907, Myers[2] in 1908, McDougall[3] in 1908, and Woolley[4] in 1909, but the results, here listed, are cramped by the above mentioned inadequacies. However, Shinn has shown that a color preference is exhibited in the second half year of the life of a child, lying at the red end of the spectrum. McDougall, working with children from the fifth month on, and using the method of paired comparisons, found that red, green, and blue are preferred to white. Myers found results coinciding with those of McDougall. Woolley experimented upon a girl infant almost 7 mo. old, and found, using the method of paired comparisons, the child's preference for red and an indifference to green. Blue and yellow held an intermediate place, with yellow slightly in the lead. The preference does not coincide with the brightness scale.

The largest group of young people afforded by the literature is that of 212 kindergartners. This is reported on by Dashiell[5] who in the same report gives comparative results between these kindergartners and a group of college sophomores. He found a striking similarity of preference between the groups of young and old Ss.

In the present experiment it is sought to obviate the errors which may come of using small groups and the lack of definiteness resulting from inadequate quantitative measures of the results. Results from a previous study[6] of 1000 older children are here used for purposes of comparison.[7]

Subjects. The Ss of this experiment were 1032 young children. These were divided into 3 groups: (I) 439 pre-school children; (II) 381 kindergarten children; and (III) 188 first-grade children. The ages of the children in Group I ranged from 1 to 4.9 yr.; in Group II from 4 to 6.9 yr.; and in Group III from 6 to 6.9 yr. These children were found in urban communities and rural sections in Colorado

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* Accepted for publication January 2, 1933.

[1] M. Shinn, The development of the senses in childhood, *Univ. Calif., Pub. in Educ.*, 4, 1907.

[2] C. S. Myers, Some observations on the development of the colour sense, *Brit. Jour. Psychol.*, 2, 1908, 353-362.

[3] W. McDougall, An investigation of the colour sense of two infants, *Brit. Jour. Psychol.*, 2, 1908, 338-352.

[4] H. T. Woolley, Some experiments on the color perception of an infant and their interpretation, *Psychol. Rev.*, 16, 1909, 363-376.

[5] J. F. Dashiell, Children's senses of harmonies in colors and tones, *J. Exper. Psychol.*, 2, 1917, 366-475.

[6] T. R. Garth, A color preference scale for one thousand white children, *J. Exper. Psychol.*, 7, 1924, 233-241.

[7] Cf. also W. E. Walton, R. B. Guilford, and J. P. Guilford, Color preferences of 1279 university students, this JOURNAL, 45, 1933, 322-328.

and Arizona. The children of Group I and of some of the other groups were found in various settings, as in the home, Sunday School, and on playgrounds. All the rest were found in public and private schools.

Materials of experiment. The materials of the experiment were 7 color stimuli. Six of these were saturated colors as red, green, orange, violet, blue, yellow. The seventh stimulus was the achromatic white. All these were on the Milton Bradley coated papers in the form of disks ½ in. in diam., which were mounted on small white cards. The 7 stimuli made a set and the sets were contained in small envelopes. The method of administration was the simultaneous presentation of the colors to a child with intent of having him pick out from the set the one he liked best, then from the 6 left, the one he liked best, and so on until he had only one left as the one he liked least of all. The process was more directly supervised in the case of the very young children, where the child was persuaded to make at least 3 arrangements of order of preference so as to insure that his real order of preference was obtained.

TABLE I

SHOWING RANKS, SCALES, AND COEFFICIENT OF VARIABILITY

(Numbers in parentheses give the number in the group)

	Larger groups				Pre-school					
Colors	Rank	A Younger (1032)	Rank	B Older (1000)	Rank	C 1–2.9 yr. (62)	Rank	D 3–3.9 yr. (124)	Rank	E 4–4.9 yr. (128)
Red	1	185	3	122	3	298	1	138	1	208
Blue	2	174	1	204	6	136	1	138	2	204
Green	4	151	2	126	2	309	3	131	3	197
Orange	3	155	5	100	1	316	3	131	4	185
Violet	5	127	4	107	4	272	6	120	5	178
Yellow	6	105	6	69	5	147	5	124	6	125
White	7	0	7	0	7	0	7	0	7	0
C.V.		.15		.24				.03		.05

	Kindergarten				First Grade		Sex Groups			
Colors	Rank	F 5–5.9 yr. (102)	Rank	G 6–6.9 yr. (102)	Rank	H 6–6.9 yr. (126)	Rank	I Boys (495)	Rank	J Girls (537)
Red	2	109	1	308	3	179	1	199	2	189
Blue	5	141	2	207	1	262	2	169	1	193
Green	3	175	3	224	5	153	3	147	4	107
Orange	1	201	4	177	2	219	4	136	2	189
Violet	4	145	5	170	5	127	5	110	5	136
Yellow	6	100	6	120	6	105	6	91	6	114
White	7	0	7	0	7	0	7	0	7	0
C.V.		.12		.03		.24		.22		.16

The scales. The rankings of the 1032 children were recorded in blocks with reference to age, sex, and school experience. In order to find some numerical value for the affective reaction to a color, scales were obtained in terms of D/MD, built upon an arbitrary zero value, *i.e.* white. (See Hillegas[8] for discussion of the method of obtaining a scale.) If a color is preferred to another color 25% of the time, the scale difference between them is 1.00; if 35%, it is 0.57. Table I gives the ranks of the colors, various scales, and coefficients of variability for values of the 6 colors above white. (See Table I for the scales and Fig. 1 for this graphic representation.) It will be seen that the scale for the 1032 young white children (Group A) is slightly different from that for 1000 white children, whose ages run from 6 to 19 yr., and who were in school grades 1 to 10 (Group B). In the case of the children having more wide-spread ages and grade experience, 1000 cases, the value of yellow is seen to be 69 units. It might be permissible to say that all colors had a stronger pull for the first group than for the second group. The ratio for yellow is 105 to 69, or 1.52:1.00. In the first group, red has a scale value of 185, and for the second group a value of 122. Here the ratio (185:122) is 1.50:1.00, or about the same as it was for yellow.

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[8] M. B. Hillegas, *A Scale for the Measurement of Quality of English Composition of Young People*, 1913, 18-25.

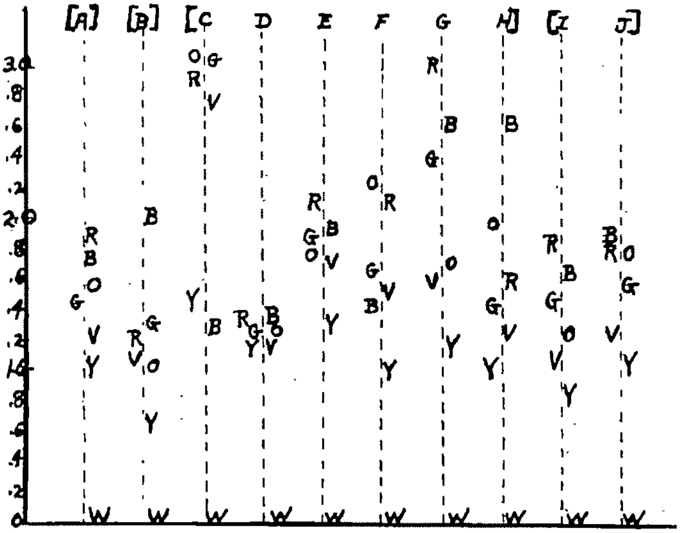

FIG. 1. GRAPHIC REPRESENTATION OF THE SCALES OF THE VARIOUS GROUPS

In the case of Group A, R is the most preferred color, being 11 points above B, while, in the case of Group B, R stands third from the most preferred, being transcended by B, 70 units, and by G, 4 units. In the younger group O and V taken as complementary, and R and its complement G have respective ratios of (155:167) 1.20:1.00 and (185:151) 1.22:1.00. In the older group these complementary colors stand each to each so closely that it may be said that the judges experience practically no discrimination of feeling (122:126) between the members of the respective pairs. The writers offer no explanation for this disposition of the difference between B and Y to increase and for that of the others, as R, G, O, and V to decrease.

Analysis of scale for young children. Two questions arise: (1) What are the scales for children for various ages? (2) What differences are found due to sex? Table I gives the scales and Fig. 1 their graphic representation. Scale C is for children of 1.5 to 2.9 yr., pre-school children. Since this group is small the results are not quite reliable; Scale D is for pre-school children from 3 to 3.9 yr.; Scale E is for pre-school children from 4 to 4.9 yr.; Scale F is for kindergarten children from 5 to 5.9 yr.; Scale G is for kindergarten children from 6 to 6.9 yr.; Scale H is for first-grade children of 6 to 6.9 yr.

On examination of the scales and the graphic presentations of the same, Fig. I, it will be observed that all these children have preferences as indicated between the achromatic white and any color; *i.e.* all place white last, and that they place yellow next to white (with the exception of Groups C and D, the youngest ones, who, however, prefer it definitely to white.) If we ignore Scale C we may say that with increase of age (Scales D to G) these children experience a more definite feeling of preference for the several colors. (Compare the coefficients of variability.) Red is always high in the scales, and is somewhat preferred to blue in most instances. As to Scale H, we would suggest the influence of adult bias.

Sex differences. Among young children Scales I and J are given for 495 boys and 537 girls. Here we find in the case of the boys a tendency for red to have the greatest affective value, and in the case of girls a tendency for blue to have the greatest affective value, with red running a close second, so close that there is hardly a perceptible difference. The scale of the boys is more clean-cut than that for the girls. We cannot say, however, that there are indicated satisfactory sex differences excepting for absence of confusion in discrimination on the part of the boys. Garth[9] found the same situation when he compared older boys and girls, that boys feel more clean-cut differences between the affective values of the several colors.

SUMMARY

(*1*) The older children (1000) have a more definite discrimination of feeling of difference between the colors than the younger children (1032).

(*2*) From 3 to 6 yr. of age in the Kindergarten the coefficient of variability increases with each age group, though there is a slight set-back with 6-yr. olds in the first grade. The discrimination of feeling for colors tends to increase with age.

(*3*) The boys on the whole discriminate the feeling values of colors somewhat better than the girls, even among children of 6 yr. and less.

(*4*) Even children of 1 to 2.9 yr. like any color better than white. This is shown by the incomplete scale, Scale C. The Scale (D) for children 3 to 3.9 yr. is more representative for the youngest group and indicates the same tendency, but no one color has any decided preference with these children.

(*5*) If we ignore the results for the youngest group because of the inadequate number tested, we may say that yellow tends to be the least esteemed of the colors. Red is held in high esteem by all age-groups for children through the Kindergarten. It drops in the first grade, and this suggests adult influence.

(*6*) On the whole blue, with the exception of the 5 to 5.9 yr. group, tends to be more esteemed with increasing age, and the divergence between it and yellow tends to increase with age, with the exception mentioned.

(*7*) There is a tendency for the ratios between the "pulls" of red and green, and orange and violet, to decrease with age.

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[9] T. R. Garth, *Race Psychology,* 1931, 115-136.

THE PITCH OF TONAL MASSES

By ADOLPH G. EKDAHL, University of New Hampshire,
and EDWIN G. BORING, Harvard University

Twenty years ago Baley reported that a number of tones, sounding together and differing from each other by small equal increments, tend to fuse (zusammenfliessen) into a single resultant tone, the pitch of which corresponds to the arithmetic mean of the frequencies of the stimuli.[1] Baley presented data for only four cases: (1) 6 tones in the range 610-625 cycles, (2) 6 tones in the range 600-620 cycles, (3) 8 tones in the range 600-628 cycles, and (4) 10 tones in the range 600-636. In the first case the increment between successive tones of the series was about 3 cycles, in the other cases it was about 4 cycles. The tone resulting from the mixture was described in the four cases as lying respectively in the ranges 616-619, 608-612, 612-618, and 616-620. It is obvious that this work is not quantitatively exact. There is no evidence of a psychophysical procedure and the subjective equivalents were not exactly determined. Baley's use of the phrase "arithmetic mean" does not signify that the equivalence is not for the geometric mean. The arithmetic mean of the six tones in his first case is 617.60 cycles, the geometric mean is 617.57 cycles, and he fixed the subjective equivalent merely as lying between 616 and 619 cycles. Baley, however, stressed more than the quantitative results the intimate fusion of the tones into a single resultant tone.

In 1925 the junior author, working in the Harvard Psychological Laboratory, undertook to determine the subjective equivalents in pitch of the tonal masses described in Table I. The points of subjective equality were computed by the method of constant stimuli with five comparison stimuli and (except for two cases, see legend to Table II) 100 series. The six sets of notes defined in Table I were given by an Appunn reed tonometer and were employed as the standard stimuli. The standard was given first. The stops were pulled out, the air was turned on and the tonal mass allowed to build up, then the stops were all pushed in at once by a hinged felt-covered board, and the single stop for the comparison stimulus was pulled out. The *O* made comparative judgments of the pitch of the single note in respect of the preceding tonal mass.

There was no evidence at all of the fusion of any of these tonal masses to yield a single resultant tone or note. The mixtures always sounded complex, and the psychophysical judgment was a judgment of equivalence of pitch but by no means a judgment of identity. This resultant is not a direct contradiction of Baley's finding. The minimal increment in the present experiment was 8 cycles in the region of 264 cycles, where 8 cycles is equivalent to about 52 musical cents (100 cents = 1 semi-tone in equal temperament). Baley used steps of about 4 cycles, but they were in the region of 620 cycles where every step is only about

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* Accepted for publication March 1, 1933.

[1] S. Baley, Über den Zusammenklang einer grösseren Zahl wenig verschiedener Töne, *Zscb. f. Psychol.*, 67, 1913, 261-276.

11 cents. However, the minimal range in the present experiment ("4 by 8," Table I) was 157 cents, and for it there was no good fusion; whereas the maximal range for fusion reported by Baley—case (4) cited above—was 102 cents, a quarter-tone less. Nevertheless it seems doubtful that Baley's tones could have fused in the way that colors fuse in color mixture to give identity in perception. It is much more likely that his "resultant tone" was merely a tone judged as equivalent to a perceived tonal mass.

It was, however, not the primary purpose of the present experiment to test Baley's results. Rather we sought to examine the nature of the pitch of the noises which are generated as tonal masses. The complexes were chosen to yield results for different numbers of notes combined (from two to eight) and different ranges of pitch (from about three-fourths of a whole tone to almost an octave). Thus in the stimulus '2 by 24' we have an ordinary interval that is something less than a major second, and in the stimulus '2 by 48' we have an interval a little more than a minor third. These pairs are simple fusions. However, the stimulus '8 by 8,' extending over an interval slightly less than a major third, yields a confused mass that can readily be described as a noise, and the stimulus '8 by 24,' covering nearly an octave, is also a noisy complex.

TABLE I

DESCRIPTION OF STANDARD STIMULI

The tonal masses defined in the table served as standards to which single notes were equated in pitch by the method of constant stimuli. The range is given in musical cents (1200 cents = 1 octave)

Name	No. notes	Int. betw. notes	Notes in tonal mass (cycles)	Geom. mean	Arith. mean	Range in musical cents
8 by 8	8	8	236–244–252–260–268–276–284–292	263.36	264	362
4 by 8	4	8	252–260–268–276	263.85	264	157
8 by 24	8	24	180–204–228–252–276–300–324–348	258.10	264	1141
4 by 24	4	24	228–252–276–300	262.63	264	475
2 by 24	2	24	252–276	263.72	264	147
2 by 48	2	48	240–288	262.91	264	315

The results are shown in Table II.[2] Here the small size and variability of the intervals of uncertainty show that the judgment was difficult and that the *Os* tended to avoid the category *equal,* as they might not have done had it been possible for *equal* to mean *identical* instead of merely *equivalent.* Hence we may dismiss the intervals of uncertainty as unimportant.

The first line of the table shows that *Ds* with the '8 by 8' stimulus gave the point of subjective equality at 261.46 cycles. This value is quite close to the mean of the stimulus (range 236–292 cycles) which is 264 cycles. It will be observed from the values of *h* that the point of subjective equality, 261.46, is quite precisely established. The interquartile range in this case is about 6 cycles, *i.e.* with a change of 6 cycles one passes from the stimulus that yields 25% judgments of *higher* to a stimulus that yields 75% of *higher* (or conversely for *lower*).

Examination of Table II shows that, with the exception of the case of '8 by 8'

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[2] The observers in this experiment were H. R. DeSilva, A. M. Brues, R. A. McFarland, K. E. Zener, and T. F. Karwoski, then research students in the Harvard Psychological Laboratory.

TABLE II

Subjective Equality of Single Notes to Tonal Masses

The first two columns show the number of notes and the interval (cycles) between successive notes in the tonal masses making up the standard (cf. Table I). The body of the table gives, in cycles, the lower and upper limens, the values of *h* for each of the limens, the interval of uncertainty, and the point of subjective equality (intersection of the two psychometric functions). A value of $h = 0.2$ corresponds to an interquartile range on the psychometric function of about 4.8 cycles, since $\text{P.E.} = 0.4769\ h$. All frequencies in the psychometric functions are based on 100 observations, except that there were only 75 observations for *Ze* and 85 for *Ka*.

	Standard		Limens		*h*		Interval of Uncertainty	Point of Subjective Equality
O	No. notes	Int. betw. notes	Lower	Upper	Lower	Upper		
Ds	8	8	260.30	262.51	.1502	.1648	2.21	261.46
Br	8	8	254.57	255.19	.2331	.2426	0.62	254.89
Mc	8	8	251.63	251.95	.1845	.1893	0.32	251.79
Ze	8	8	256.67	260.16	.1185	.1612	3.49	258.68
Ka	8	8	284.81	276.37	.0720	.0895	−8.44	280.13
Ds	4	8	261.75	264.70	.2492	.2051	2.95	263.08
Br	4	8	259.64	259.72	.4514	.4426	0.08	259.68
Mc	4	8	260.47	263.46	.2061	.2218	2.99	262.02
Ds	8	24	262.91	265.11	.2384	.2222	2.20	263.97
Br	8	24	271.47	271.45	.1398	.2656	−0.02	271.46
Mc	8	24	285.38	285.38	.1971	.1971	0	285.38
Ds	4	24	266.07	267.75	.1983	.1534	1.68	266.80
Br	4	24	252.16	254.27	.0940	.2509	2.11	258.69
Ds	2	24	266.21	269.12	.2077	.1307	2.91	267.33
Ds	2	48	238.45	242.24	.2238	.1643	3.79	240.05
Br	2	48	240.04	241.90	.4121	.3649	1.86	240.91

for *Ka* and the two cases of '2 by 48,' all the points of subjective equality are near the mean, 264, and that nearly all of them are placed within interquartile ranges of less than 6 cycles. This degree of precision is high, although it is not great enough to give significance to the question as to whether it is the arithmetic or the geometric mean that is approximated (cf. Table I). What the Table II shows is that an accurate judgment of pitch can be made for tonal masses where as many as eight tones, distributed within a musical third or over an entire octave, are sounded together. In such cases the pitch of the mass is taken by *O* as equivalent to a mid-pitch of the mass. The same principle held in this experiment for masses of four tones, and for a two-tone combination when the two tones were separated by an interval less than a major second ('2 by 24'). It did not hold for an interval approximating a minor third ('2 by 48').

There were three exceptions to the general rule. In the case of '2 by 48' both *Ds* and *Br* equated the pitch of the pair to the pitch of the lower member of the pair. We probably have here the limiting case where the number of components is so few (two) and the separation is so great (minor third) that the *O*, in making a judgment of pitch, could not avoid discriminating the two pitches and selecting one as the equivalent. It is apparent that a complex tonal mass or one within a small range of pitches is localized as to pitch at its mid-pitch, but that simpler complexes may be characterized by one of the actual pitches in the complex. Thus we know that clangs are located as to pitch by their fundamentals, the lowest pitch of the complex, but that in musical harmony the upper note of a chord often characterizes the fusion as to pitch.

The other exception is the case of '8 by 8' with *Ka.* He found the judgment very difficult and his subjective assurance was low. His psychometric functions show erratic frequencies, as the negative interval of uncertainty and the low values of *h* indicate. The point of subjective equality (280.13) is well above the average, but not as high as upper note of the stimulus (292). Presumably this case can be ignored, as dependent upon an instable attitude.

In brief we may say that, if we start with a single tone and add successively to it other tones that bear no particular musical relation to one another, we pass through musical fusions to confused tonal masses which are noisy and which may be regarded as noises approximately located as to pitch. The nature of these complexes as noises is most obvious when the masses are given briefly and explosively by an abrupt quick pressure on the tonometer bellows. The fact that they have location in the scale of pitches is shown by the capacity of *Os* to equate them to simple pitches, usually a pitch approximating the pitch that corresponds to the mean of the frequencies of the tones in the mass. However, the mass is still perceived as a mass, and its equation to a single pitch is only a rough localization that resembles the manner in which a large cutaneous impression is localized, when its exact position is required, approximately at its mid-point.

In this manner we come to see the relation of noises to tone. Complexes of tones are noises, and a noise is tonal when pitches within it are continued and when the range of pitches allows of some relatively definite location of the complex noise in the scale of pitches.

CERTAIN QUANTITATIVE ASPECTS OF CHICKENS' BEHAVIOR TOWARDS FOOD

By W. A. Bousfield, Harvard University

In an earlier paper, the writer has attempted to show that graphical records of the amount of food eaten by cats during a period of feeding possess a high degree of regularity.[1] The analysis of these records demonstrated that they could be described adequately by means of the exponential equation,

$$f = c\,(1 - e^{-mt})\ldots\ldots\ldots\ldots\ldots\ldots\ldots[1]$$

in which f = amount of food eaten, c = constant, e = base e of natural logarithms, m = constant, and t = time.[2] The experiment reported here was undertaken with the purpose of applying a parallel quantitative method to a study of the behavior changes of a different animal during the eating of a different food.[3] These data not only furnish an index of the variability of a certain type of behavior, but also a partial check on the generality of the exponential equation as a descriptive formula.

TABLE I

Values of Constants c and m

	Chicken A		*Chicken B*		*Chicken C*		*Chicken D*	
Record	c	m	c	m	c	m	c	m
1	180	.0329	180	.0366	180	.0478	180	.0413
2	180	.0468	180	.0410	200	.0470	180	.0426
3	180	.0512	180	.0563	200	.0476	180	.0408
4	180	.0512	180	.0413	180	.0444	180	.0393

Average c = 182.50 Average m = .0442

An experimental technique was devised to obtain kymograph records of the amounts of whole-wheat eaten by chickens during regular periods of feeding. Four Rhode Island Red roosters were used for *Ss*. At a regular hour each day an *S* was admitted to a pen containing a trough 2 in. deep and 18 in. square, from which the animal pecked its food as it was dropped through a piece of brass tubing extending at an angle of 30° from the side of the pen to the edge of the trough. Portions of 2 grm. were presented at a time. As soon as all but about 6 kernels of the wheat had been eaten, *E* dropped another 2-grm. portion into the tube, and at the same time depressed a telegraph key which was wired to a recording de-

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* Accepted for publication January 20, 1933.

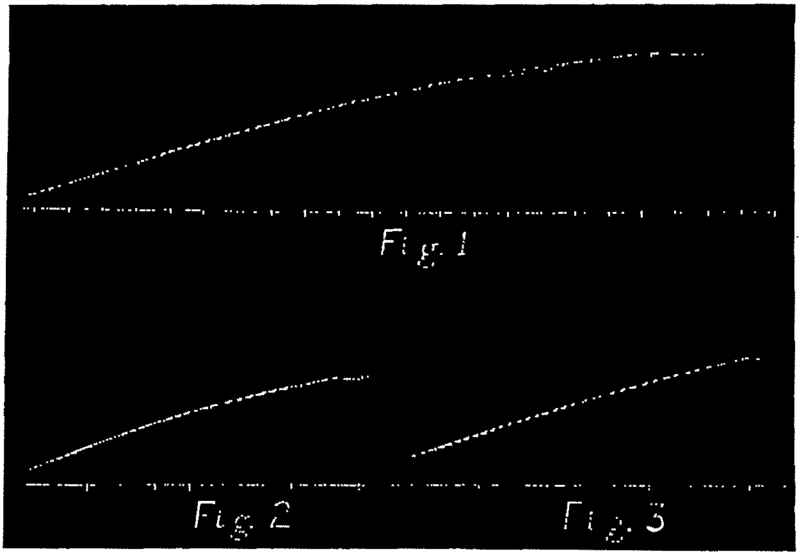
[1] W. A. Bousfield, Certain quantitative aspects of the food-behavior of cats, *J. Gen. Psychol.*, 8, 1933, 446-454.

[2] The exponent, mt, was originally presented as positive. The negative exponent provides a simpler mathematical significance.

[3] The writer wishes to thank Dr. M. H. Elliott of the Harvard Psychological Laboratory for his help in the undertaking of this experiment, and Professor G. D. Birkhoff of Harvard Mathematics Department for his assistance in the interpretation of the exponential equation.

vice arranged to lift a writing-point vertically through a unit-distance. The process was continued until the animal definitely ceased to respond to the food, *i.e.* sat down and closed its eyes, began preening its feathers, or walked to the opposite side of the pen. A screen concealed all movements of *E.* The recording apparatus which was somewhat noisy in its operation was placed in an adjoining semi-sound-proof room.

In all, four records were obtained from each chicken. Typical samples are presented in Figs. 1, 2, and 3. The method of treatment of the records was similar to that employed in the earlier experiment with the cats. Again, the exponential equation [1] was found to give curves closely approximating the original records. The values of constants c and m of all the records are presented in Table I. These constants have been calculated so that t represents time in mins. and the f represents grm. of food eaten. It should be noted that the mathematical treatment consists in first selecting a c to give the best alignment of the plotted points, and then in calculating m on the basis of the slope of a straight line drawn through the plotted points.[4] In Figs. 1, 2, and 3, the theoretical curves have been superimposed upon the original records.



FIGS. 1, 2, AND 3. ORIGINAL RECORDS WITH THEORETICAL CURVES SUPERIMPOSED
Each vertical step on the records represents the consumption of two grm. of whole-wheat. Time has been recorded horizontally in minutes.

The examination of Equation [1] shows that it is really a descriptive formula. The constant c is the horizontal asymptote of the function, and thus represents the quantity of food which is approached as a limit by the function and theoretically never completely realized. If we call c the ideal physiological limit of the amount of food consumable by the animal, we shall not be so much interpreting c as stating what the formula shows c actually to be. It may be that c will turn out to depend upon the capacity of the animal, for in the data of this experiment c shows little individual difference and the four roosters, all belonging to the same brood, were similar in physical development. On the other hand, m varies with the rapidity with which the function approaches its asymptote; the more rapidly the animal consumes food the larger is m. Accordingly, m may be described by calling it the *coefficient of voracity* (for m is a coefficient in some expressions).[5] Thus m shows larger individual differences than c and less consistency for the individual, as might be expected for a characteristic which depends on a higher order of reflex integration. Still further assumptions, however, are justified. The rate of eating at any time may be determined by differentiating Equation [1],

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[4] The accuracy of c is limited to ± 5 units, with the longer records furnishing more accurate determinations. The method of treatment may slightly exaggerate both the constancy of c and the variability of m.

$$\frac{df}{dt} = cme^{-mt} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots [2]$$

But from Equation [1], it may be seen that

$$ce^{-mt} = (c - f) \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots [3]$$

Equation [2], accordingly, can be rewritten,

$$\frac{df}{dt} = m\ (c - f) \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots [4]$$

In other words, the rate of eating, at any time, varies directly with the difference between what the animal has already eaten, f, and its ideal physiological limit, c.

In spite of the differences in the rates of eating, the theoretical curves are approximated regardless of the durations of the times represented by the graphical records. A shorter record may, for this reason, be considered as a segment of a longer record which might have been obtained under more ideal conditions. With respect to the theoretical rate, the behavior tends to be all-or-none. Cessation of eating thus implies a definite shift in the behavior-pattern. It is not surprising to find that this change in behavior takes place progressively more readily as the animal continues to eat, since distension of the digestive apparatus must be paralleled by some sort of reflex inhibition. In proportion as the amount of this inhibition increases, the effectiveness of competing stimuli is enhanced.

It is evident from the foregoing analysis that the exponential equation is fundamentally reasonable as a descriptive formula when applied to certain quantitative aspects of an animal's behavior towards food. The constants of the equation are significant in their implications since they may be related to the meaningful concepts of an ideal physiological limit of food-consumption and a coefficient of voracity. The determination of the numerous factors influencing these constants offers an attractive field for further research.

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[5] The concept of a coefficient of voracity was suggested to the writer by Dr. G. D. Birkhoff. The term possesses the advantage of psychological significance while at the same time the relation to the mathematical description is immediately implied.

A COMPARISON OF SIMULTANEOUS AND SUCCESSIVE METHODS OF MOTOR LEARNING

By LELAND W. CRAFTS and R. MORRIS ALLEN, New York University

The object of the experiment was to determine whether an activity requiring ultimately the simultaneous use of the two hands—in this case to trace two identical designs seen in a mirror—would be better learned by practicing it first with each hand separately (the 'part' or 'successive' method), or by practicing with both hands together from the beginning (the 'whole' or 'simultaneous' method).

The only previous experiment of this type was performed by Koch,[1] who had her *Ss* learn to make with each hand simultaneously nine finger reactions on five keys of each of two typewriters in a fixed sequence and tempo. Her results show the successive group to have been reliably superior to the simultaneous. The present investigation was undertaken with the aim of finding out whether similar results would be obtained with an act different from that which she employed.

METHOD AND PROCEDURE

Subjects. The *Ss* were 40 undergraduate students in psychology, assigned alternately as they presented themselves for the experiment to the successive and the simultaneous groups. The former contained 15 men and 5 women, the latter 17 men and 3 women.

Apparatus. The apparatus consisted of a mirror-drawing set-up as follows. A mirror 25.5 x 19.5 in. was held by clamps and wire in a vertical position on a table. The design to be traced (a four-pointed star having eight 'alleys' each 2¼ in. long and 5/16 in. wide) was fastened to a drawing board in front of it. A curtain of black cloth prevented *S* from seeing the designs in any way save in the mirror. Two electric lights, one on each side of the mirror, provided the necessary uniform illumination. All the *Ss* used hard lead pencils furnished by *E*.

Procedure. The successive group practiced with each hand separately, using the preferred hand first (which was by chance always the right), until they could trace the design with each hand within 1 min. and with not more than one error. They then practiced tracing two stars with both hands simultaneously until they could do so in 2 min., and with not more than two errors with each hand. For this work the two designs were placed as close together as possible. The *Ss* were told that the movement of the hands must be coördinated. While a literally perfect simultaneity was not obtained, no *S* was allowed to have his hands farther apart in the course of the tracings than a distance of one half an alley on the star. If *S* failed to maintain this degree of coördination he was warned by *E* and any *S* for whom three such warnings were required was eliminated from the experiment. Altogether the records of 6 *Ss* were for this reason discarded.

The simultaneous group practiced with both hands simultaneously from the beginning until they achieved the criterion stated above. Coördination of the two hands was insisted upon to a similar degree throughout.

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[1] H. L. Koch, A neglected phase of the part-whole problem, *J. Exper. Psychol.*, 6, 1923, 366-376.

All *Ss* were instructed to stress accuracy rather than speed. They were also told, when they made an error, to return as nearly as possible to the place of departure from the alley without lifting their pencil from the paper. The practice may be termed massed, since the interval between trials was only about 0.5 min. Scoring was in trials, errors and time. An error was defined as actually crossing with the pencil either boundary line of the star. Touching such a line was not scored as an error, nor was the recrossing which necessarily occurred when *S*, having made an error, had to return to a point within the design.

TABLE I

GIVING THE AVERAGE SCORES FOR BOTH GROUPS AND ALL CRITERIA

	Right hand			Left hand			Both hands together		
	Trs.	Errs.	Time	Trs.	Errs.	Time	Trs.	Errs.	Time
Simultaneous		61.35			58.40		7.35	119.75	1157.60
Successive:									
Each hand separately	1.68*	9.20	252.80	1.25	3.95	105.40			
Both hands together							3.15	21.95	448.65
Total, hands separately and together							6.08	35.10	806.85
D./S.D.$_{\text{diff.}}$							1.71	4.59	3.04

* In computing the above averages a trial with one hand separately was counted as 1/2 a trial only. Hence the average number of actual tracings with the right hand alone was 3.36 and with the left hand 2.50.

RESULTS

The results, given in Table I, show that the successive group of *Ss* was superior to the simultaneous group in performance. The differences are marked according to all three criteria, and are reliable for errors and for time. To find the successive method superior to the simultaneous for learning an activity of this type confirms the conclusions of Koch before mentioned.[2] Her group differences were least for errors, ours are least for trials. Otherwise the results of the two experiments are in complete agreement.

In explanation of this superiority of the successive method the following factors are probably significant.

(*1*) There was presumably in the successive group a considerable transfer from the initial practice with the preferred hand to the later practice with the nonpreferred. While our data do not prove this point—a preliminary test with the left hand was not made—Ewert has shown that marked bilateral transfer does occur in mirror-drawing.[3] Furthermore, as McGeoch's review of the experimental literature on the whole-part problem shows,[4] it is generally agreed that, in maze learning at least, the possibility of positive transfer between one part and another constitutes a condition favoring a part method. It is very probable that the successive method for the learning of two separate acts which must later be integrated would also be favored thereby, and that this is therefore one cause for its superiority to the simultaneous method in the present experiment.

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[2] Koch, *op. cit.*

[3] P. H. Ewert, Bilateral transfer in mirror drawing, *Ped. Sem.*, 33, 1926, 235-249.

[4] G. O. McGeoch, Whole-part problem, *Psychol. Bull.*, 28, 1931, 713-739.

(2) Koch notes that in her experiment the difference in favor of the part method was less with those *Ss* who had had the most experience in playing the piano. Since these were also the *Ss* for whom her task was the easiest, she concludes: "This suggests that the advantage of the part method over the whole is greatest when the task is most unfamiliar and difficult."[5] Pechstein holds similar views.[6] While this factor of difficulty can scarcely be expected of itself to determine the relative values of part and whole methods to the exclusion of all other conditions—that it does not always do so has been demonstrated by Crafts with card-sorting[7]—it may plausibly be regarded as having some significance. In the present experiment mirror-drawing was for all *Ss* a totally unfamiliar task and to trace the designs with both hands simultaneously seemed to be difficult and, if not preceded by practice with each hand separately, very confusing as well. Hence in so far as this factor is important it may perhaps have favored the successive method.

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[5] Koch, *op. cit.*, 376.

[6] L. A. Pechstein, Massed vs. distributed effort in learning, *J. Educ. Psychol.*, 1921, 12, 92-97.

[7] L. W. Crafts, Whole and part methods with non-serial reactions, this JOURNAL, 41, 1929, 543-563.

WORK-SCORES *VS.* TIME-SCORES

By QUINN MCNEMAR, Stanford University

In order to decide upon the relative merits of work-scores as compared with time-scores, it seems necessary to give consideration to three phases of the problem. In the first place, it would be better to use that score which gives distributions more nearly normal, thereby permitting the use of ordinary statistical techniques. It often happens that time-scores give skewed distribution curves, whereas the same performance scored in terms of work per unit of time will result in distributions approximately normal. In the second place, the question of the relative speed of work is of theoretical importance as regards the effect of two different mental sets upon performance. This may manifest itself in group trends or may result in wide individual differences. In the third place, the accuracy with which one score can be predicted from the other is of fundamental importance in the theory of measurement and in the practical situation wherein variables are being intercorrelated. Although it would seem safe to assume that the two types of scores would correlate with each other as highly as their reliabilities would permit, such an assumption should be checked in order to satisfy the skeptical investigator.

The study of Paterson and Tinker demonstrates that for a speed of reading test,[1] the correlation of work-limit with time-limit scores was unity when corrected for attenuation. That such a finding may not hold for types of performances requiring skill was brought to the present writer's attention in connection with a study of the effect of practice upon twin resemblances in motor skills. In this study, the assumption was made that the reciprocals of the work-scores would be proportional to the corresponding time-scores, but when twin resemblances were computed on the basis of these reciprocal scores, the coefficients of resemblance were found to vary considerably from those obtained when the work-scores were used. It is the purpose of this paper to present the results of an experiment so designed as to permit an empirical check on the interchangeability of time- and work-scores for two of the performances used in the twin study.

Fifty men (elementary psychology students) were given 12 trials on each of two tasks, spool packing and card sorting, the odd trials being scored in terms of work done in a given time (60 sec. for the spools and 30 sec. for the cards) and the even trials being scored as the time to do a given amount (72 spools and 30 cards). The performance of a 'control' group of 50 men was obtained for each task, both odd and even trials being scored in terms of work done in 60 sec. for the spool packing and 30 sec. for the card sorting. The order of trials for both groups was: 6 trials on the spool packing, 6 on card sorting, 6 more on the spool packing, and 6 more on the card sorting. With the exception of 8 *Ss* who worked singly, all the *Ss* were run two at a time, two spool packers and two card sorting trays being used. The social factor thus introduced seemed to aid in sustaining the interest of the *Ss* and probably served as a motivating factor. The *Ss* of the experimental group were told that the purpose of the experiment was to compare the rate of work under the two conditions, while the 'control' *Ss* were told that their services were needed in order to establish comparative norms for the two tasks. The time required of each *S* was approximately 35 min.

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[1] D. G. Paterson and M. A. Tinker, Time-limit vs. work-limit methods, this JOURNAL, 42, 1930, 101-104.

It should be noted that the odd and even trials of the 'control' or work *vs.* work-group were given under exactly the same conditions, whereas for the work *vs.* time trials of the experimental group a variable factor was present; namely, the unknown effect on the performance of the slower individual of a pair of the timed trials, which resulted from a knowledge that his partner had finished ahead of him. This factor, if operative, would probably tend to reduce the predictability of work from time-scores. Otherwise, the chief difference between the two groups concerns the mental set involved in the time-limit as compared with the work-limit trial.

TABLE I

PREDICTION OF WORK- FROM TIME-SCORES

(N = 50 for each group)

	Group	Type	r	S.D.$_w$	Standard error of estimate and its σ	r adjusted for range
Spool packing	I	W *vs.* T	.888	5.08	2.34 ± .23	.925
	II	W *vs.* W	.959	7.15	2.03 ± .20	.945
Card sorting	I	W *vs.* T	.951	3.30	1.02 ± .10	.954
	II	W *vs.* W	.937	3.49	1.22 ± .12	.934

TABLE II

COMPARISON OF RATE OF WORK UNDER THE TWO CONDITIONS

	Group	Type	Means and their σ		$D_M \pm \sigma_D$
Spool packing	I	W *vs.* T	68.7 ± 0.7	68.9 ± 0.7	.2 ± .3
	II	W *vs.* W	69.3 ± 1.0	70.1 ± 1.0	.8 ± .3
Card sorting	I	W *vs.* T	29.9 ± .5	29.9 ± .5	0.0 ± .2
	II	W *vs.* W	29.6 ± .5	30.3 ± .5	.7 ± .2

The data permit the determination of the relative accuracy of prediction of work scores from time-scores and work-scores from work-scores. The results of this analysis are given in Table I, from which it will be seen that there is no significant difference in the standard errors of estimate for work-scores predicted from time-scores (Group I) and work-scores predicted from work-scores (Group II). Or considering the correlation coefficients as adjusted for range (for 100 *Ss*, S.D. for spools is 6.21, for cards, 3.40, each based on odd trials, *i.e.* work-scores), it will be noticed that there is little difference between the two coefficients for the two groups. Stated in another way, the relationship between work- and time-scores in each case (spool packing and card sorting) is as high as the self-correlation will permit. This is in agreement with the results of Paterson and Tinker for a more intellectual function and based on group rather than individual testing.

In order to check the rate of work under the two conditions, the time-scores were converted to rate per 60 sec. for the spool packing and per 30 sec. for the card sorting. Table II gives the means (in comparable rate terms) for the odd and even trials for each group. Group II shows a significantly greater average rate on the even trials for both tasks, the difference being due probably to the practice advantage in the even trials. The absence of this difference in the case in which the even trials were timed (Group I) may be due to a slower rate under this condition. It should be noted, however, that this slower rate cannot be said to possess statistical significance, and that further experimentation is needed in order either to prove or disprove the existence of a difference in the rate of work under these two conditions.

The results of this study tend to support the view that work-limit scores are interchangeable with time-limit scores, and it is concluded that, except in the case where skewed distributions result from one type of score, it should make little difference in the practical situation which type of score is used. This does not, of course, apply to the highly paradoxical situation met with in connection with the study of the effect of practice on group variability.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF NEBRASKA

VIII. THE DEPENDENCE OF HUE, TINT AND CHROMA UPON AREA

By BEATRICE H. MARSHALL and J. P. GUILFORD

In recent months the senior author has had several occasions for using the *Munsell Book of Color* (Abridged ed.) in the evaluation of colored papers. Those who are familiar with the book know about the arrangement of colors included within its pages. Each one of twenty pages contains small rectangular samples of color all of the same hue but varying in tint (value) and in chroma. The vertical dimension of the page represents differences in tint, with the darkest colors at the bottom and the lightest at the top. There are usually eight distinct steps in tint, with psychologically equal differences between them. The horizontal dimension of the page represents differences in chroma. The column at the left has the least saturation and the chroma increases by equal steps as one goes to the right side of the page.

The *Book of Color* is provided with a gray mask with a rectangular opening equal in size and shape to the colored rectangular samples on its pages. This is for the obvious purpose of equating the area of colored material which is to be evaluated with the area of the standards in the book. Some of the senior author's *O*s were of the opinion that a colored sample, 4 in. sq. for example, would rate the same with or without the mask. Others were of a more decided opinion that without the mask there would be a distinct change in the rating; that the color square would rate much higher for chroma when viewed without the mask.

From a practical standpoint if it could be shown that the size of the color to be rated makes no significant difference in the evaluation of it on the Munsell scheme, then the use of the mask, in so far as it is used to control size, could be dispensed with and time could be saved. If, on the other hand, size is an important factor, then the mask must always be employed. From a theoretical standpoint, also, it would be interesting to know whether there is any change in hue, tint, or chroma as the size of the stimulus is changed. Do larger areas increase in lightness and darkness? Do larger areas appear to be more saturated than smaller ones? Does the hue of a color itself tend to shift in any systematic manner as the size of the stimulus is changed?

One finds very little in the literature that bears directly upon this point, although the dependence of chroma upon field size seems to have general recognition. To mention only a few, Abney,[1] in speaking of the extinction of different colors with low intensities of stimulation says, "The intensity of the light to just cause a loss of colour may be increased tenfold when the aperture is diminished to

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* Accepted for publication July 12, 1933.

[1] Wm. Abney, *Researches in Colour Vision and the Trichromatic Theory*, 1913, 156.

one-eighth the diameter." Purdy[2] agrees with Abney "that smaller field sizes gave higher values for both the specific (chromatic) and general (light) threshold." Judd,[3] in working upon sensitivity to differences in color temperature, found that the ability to discriminate differences depended upon field size. It is generally known that in peripheral vision the larger the color patch, the farther in the periphery it may be seen as color. The sizes of the traditional color zones increase with larger stimuli.

Certain facts connected with color-blindness also lead one to conclude that the greater the field size the greater the saturation. Color-blind individuals are commonly known to respond to red or to green or to both as colors when the stimuli are made large enough or saturated enough. Color-blindness is probably merely extreme color weakness. Nagel, for example, a typical deuteranope, could readily distinguish red if the field size was sufficiently great.[4] Tests of color-blindness take account of this fact by making the stimuli in the form of small spots of rather low saturation, as in the Nagel, Stilling, and Ishihara tests.

All these observations have to do with very low intensities of stimuli, as in the work on chromatic thresholds and the colorless interval, or else with the abnormal eye that has a very high threshold for some of the hues. The question that we are raising has to do with higher degrees of saturation and with normal eyes. Undoubtedly, under the special conditions just mentioned, the size of the stimulus is an important factor for chroma. It is conceivable that with medium intensities of illumination, normal eyes, and with sizes of stimuli such as were used in this study, the limits of the effect of size have been reached. Casual observations previously made during the use of the *Munsell Book of Color* seemed to show that those limits had not been reached. The present study deals just with that phase of the problem. A much more thorough approach would make use of an extensive range of stimulus intensities and of field sizes. The quantitative relationships between chroma and field size would then be established from the lower limit, the chromatic threshold, to the upper limit, the size beyond which no further increase in saturation is possible for a given degree of illumination and for the normal eye.

Stimulus materials. Fifteen different colors were chosen from the Milton Bradley series of 90. First, 5 representative hues were selected. The most saturated sample of each hue, and one lighter and one darker sample of it, were chosen. The entire range of values was from about 3 to 9 on the Munsell scale. The range in chromas was from about 6 to 14. The names of the colors will be found in the first column of Table I. In addition to these 15 colors, samples of black and white were used. These 17 samples were prepared in the form of squares in 4 sizes. The areas were 0.25, 1, 4, and 16 sq. in., so planned as to represent a geometric series. The squares were pasted on a gray background 8.5 x 8.5 in., the background having a rating of N 5 on the Munsell scale.

Procedure. The observations were made in a northeast room with ample indirect sunlight illumination, between the hours of 10 A.M. and 3 P.M. *O* sat at a headrest and the color sample was placed directly in front of him against a larger gray background 22 x 28 in. in size, and rating N 5 on the Munsell scale. The distance was constant at about 30 in. *O* first chose the page from the Munsell Book that was nearest to the sample in hue, and gave to the sample a rating for hue. He interpolated between pages when no single page exactly resembled the sample. The pages of the book had been numbered, beginning with R which was given a value of 1. After evaluating the sample for hue, *O* then gave his estimate of its tint and its chroma, interpolating whenever necessary. In all these comparisons the pages of the book were set up beside the sample on the large gray background.

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[2] D. McL. Purdy, Saturation and chromatic thresholds of spectral colours, *Brit. J. Psychol.*, 21, 1931, 296.

[3] D. B. Judd, Precision of color temperature measurements, *Bur. Standards J. of Res.*, 5, 1930, RP No. 252, 1169.

[4] W. Nagel, Farbenumstimmung beim Dichromaten, *Zsch. f. Sinnesphysiol.*, 44, 1910, 5-17.

The order of presentation of the samples was a prearranged one in which sevral rules were followed. No hue was followed by the same hue in a different size, tint, or shade. No hue followed its complement or near complement. No square was followed by a square of the same size. The order was completely reversed for the different trials. *O* rated the 68 samples during a single sitting, but at least 2 days intervened between sittings. Nine *O*s, with normal color vision, including 2 men and 7 women, made 10 complete sets of ratings.

Results. Since the ratings given the various samples by different *O*s agree so very closely, we have pooled the 90 judgments for each one of the 68 samples, and we present the means in Table I.

Table I demonstrates a striking uniformity of the ratings for any one color sample regardless of its size. Since this is typical of the results of all the *O*s taken individually, this uniformity in Table I is not therefore the consequence of pooling results and the smoothing out of individual differences.

There are one or two significant facts in Table I. Incidentally, the table shows that colors which bear the same name for hue in the Milton Bradley series may not have exactly the same hue. For example, OR has a rating of about 1.75 for one sample, and a rating near 1.10 for the other two. The 3 samples of OY vary from about 4.10 to 4.35. The samples of G vary from about 7.50 to 8.00. The samples of GB vary from about 14.10 to 15.00. Only the BV has a rather uniform rating for hue in the 3 samples used here. The other 2 samples of BV in the Milton Bradley series might not, of course, do so well. The authors had found previously that the Milton Bradley colors cannot be depended upon for uniformity of hue, nor for an even spacing along the color circle.

The only relation of size to the tint of the stimuli that can be discerned in Table I is in the case of black and white. There is a very slight but continuous tendency for the black to gain in darkness with increase in size, and for the white to gain in lightness. This led us to wonder whether the lighter of the colored samples also tended on the average to rise in tint, and whether the darker of the colors tended to fall in the scale. Taking the 8 colors that rated above 5 in value and the 7 that rated below 5, and averaging the ratings for each size, we found decidedly no such difference. The lighter colors gave means of 7.27, 7.30, 7.27 and 7.22 for the 4 sizes respectively. The darker colors gave means of 4.01, 4.03, 4.03, and 4.22 for the 4 sizes. Only black and white show any tendency at all to change in tint as the area is varied.

It had been expected that chroma would be most affected by variations in area. An examination of the data for single colors in Table I will show that in some

TABLE I

Combined Ratings for Hue, Tint, and Chroma for 68 Samples of Color in Four Sizes as Given by 9 Os

	Ratings for hue				Ratings for tint				Ratings for chroma			
Size	0.25	1.00	4.00	16.00	0.25	1.00	4.00	16.00	0.25	1.00	4.00	16.0
ORt2	1.70	1.73	1.85	1.80	7.69	7.72	7.61	7.71	7.20	7.09	7.09	7.13
OR	1.14	1.13	1.15	1.16	4.59	4.50	4.58	4.68	14.09	14.24	14.03	14.09
ORs2	1.08	1.06	1.07	1.08	3.02	3.16	3.23	3.32	9.70	10.14	9.86	10.28
OYt2	4.13	4.14	4.11	4.11	8.61	8.74	8.69	8.67	6.23	6.30	6.34	6.35
OY	4.31	4.38	4.36	4.33	8.04	8.04	7.89	7.97	10.03	10.20	10.33	10.46
OYs2	4.05	4.08	4.06	4.19	5.04	5.16	5.15	5.11	8.31	8.28	8.47	8.49
Gtl	7.99	7.99	8.00	7.98	7.67	7.71	7.69	7.42	7.51	7.66	7.47	7.64
G	7.82	7.78	7.82	7.70	7.02	7.02	6.98	6.98	9.09	9.09	9.33	9.43
Gs2	7.52	7.51	7.53	7.52	4.70	4.72	4.75	4.76	7.58	7.53	7.53	7.69
GBtl	14.09	14.04	14.11	14.17	6.89	6.87	6.93	6.87	6.32	6.28	6.49	6.37
GB	15.05	14.95	14.92	14.89	4.87	4.86	4.93	4.86	10.49	10.73	10.66	10.69
GBs2	14.93	14.93	14.88	14.97	2.92	2.95	2.88	3.98	7.56	7.37	7.64	9.86
BVt2	16.05	16.02	16.05	16.02	7.19	7.11	7.24	6.99	5.91	6.52	6.31	6.92
BV	16.03	15.98	15.98	15.95	4.84	4.76	4.69	4.75	10.32	10.52	10.76	10.63
BVs2	15.94	15.94	15.98	15.94	3.13	3.26	3.18	3.17	9.11	9.19	9.14	9.58
Black					1.64	1.63	1.59	1.56				
White					9.54	9.79	9.76	9.83				

cases a distinct trend does exist. This trend is an increase in chroma with increase in area. But in most cases the trend is uncertain. We have found the means of the ratings for chroma for the 5 lightest, the 5 darkest, and the 5 medium colors representing the 5 hues. For the lightest colors they are: 6.63, 6.77, 6.74, and 6.88 for the 4 sizes from smallest to largest. The total gain in chroma from smallest to largest is only 0.25 unit, certainly not anything very significant. For the medium colors the means are: 10.80, 10.96, 11.02, and 11.06. The gain is nearly the same; namely, 0.26 unit. For the darkest 5 colors the means are: 8.45, 8.50, 8.53, and 9.18. Here there is a more decided gain of 0.73 unit. A very large part of this gain can be attributed to one color, the GBs2. This color, for some unknown reason, took a sudden jump of about 2 units in chroma and at the same time a jump of about 1 unit in tint. The samples of any one color were originally cut from the same piece of colored paper material. This particular sample, the 4 in. sq. of GBs2, was different enough that the *O*s sometimes remarked about it. We are unable to account for this sudden 'sport' among the colors. Even with this sample left out, however, the trends of which we speak were decidedly present. The mean of the chromas with the unusual sample omitted was 9.01, which would yield a gain of 0.56 unit instead of the 0.73 with that sample left in. When all 15 samples are averaged for chroma, the means are: 8.63, 8.74, 8.76, and 9.04. With the sample GBs2 omitted, the last mean would be 8.98, and the total gain would be 0.35.

This is surely not enough to warrant the use of the Munsell mask with its small rectangular window, for the purpose of controlling the size of color samples within the range of 0.25 to 16 square inches.[5] It means that under our conditions the effect of increased size has about reached its limit. The darker colors, those of low degree of illumination did seem to gain more in chroma. Is this due to lower initial chroma or to lower illumination? It might be that any color of low original saturation would gain more by increased size. All the darker colors were poorly saturated as compared with the colors of medium tint. But on the other hand, they were more saturated, on the average, than the colors of lightest tint. All the colors of medium tint were rather highly saturated to begin with. It would be interesting to know what would have happened had they been very poorly saturated. For a solution of the more general problem a rather extensive variation in the stimuli must be used.[6] For the usual condition under which the *Munsell Book of Color* is used, however, we believe that we have demonstrated that for small samples of colored material from ½ to 4 in. in diameter, size is not an important factor.

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[5] The mask would still serve its function of isolating the colors to be matched and of giving them a uniform background where a uniform background is not otherwise provided.

[6] In a communication from Mr. A. E. O. Munsell I am told that very large areas do seem more highly saturated. He says, "On very large areas there is clear evidence of an increase in chroma of between one and two and one-half steps." To cite a particular case, a pigment which in small samples rated G 6/4, in an area of 80 sq. ft. rated G 6/6. Gray had to be added in order to reduce the rating to G 6/4.

IX. PERSONALITY TRAITS AND FLUCTUATIONS OF THE OUTLINE CUBE

By NORMAN O. FREDERIKSEN and J. P. GUILFORD

McDougall[1] has suggested that the rate of fluctuation of an ambiguous figure, such as the outline cube, might be used as an objective test of introversion-extraversion (I-E). The theoretical basis for this suggestion is that the traits of I-E are dependent upon a hypothetical "secretion X" whose function is to counteract the inhibitory effect of the cortex over the lower centers.[2] Individuals who are most extravert have this secretion in greater abundance; introverts are more deficient in this secretion. The secretion operates by increasing the synaptic resistance in the cortex, which affects the rate of change of an ambiguous figure; for introverts the cube will shift rapidly and for extraverts slowly.

This hypothesis has been subjected to experimental tests in the laboratory at the University of Nebraska[3] and by Professor Washburn[4] at Vassar. Very little has been found to give support to the theory, and yet the theory has not been conclusively disproved. Correlations between scores on the Marston and Neymann-Kohlstedt tests and the rates of fluctuation of the cube are statistically insignificant. It is a well-known fact, however, that the typical paper and pencil tests of I-E do not agree very well among themselves. It was one purpose of this study to use a better criterion of I-E and to examine a larger number of *Ss* than had hitherto been employed.

Certain earlier findings have even been encouraging enough to promise positive results more favorable to the theory. Guilford and Braly[5] found that the effect of strychnine is to increase the fluctuation rate, which is in agreement with McDougall's theory, since strychnine is supposed to reduce resistance at the synapses. Guilford and Hunt[6] found a correlation of 0.44 between fluctuation rate and the Laird test, a coefficient which was about 4.5 times its probable error. They also found some indication of a negative relationship between extent of knee-jerk and fluctuation rate, which would be expected if the latter measures the degree of cortical dominance over the lower centers. Hunt and Guilford[7] found in a later study that the average fluctuation rate for dementia praecox patients was about

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[1] Wm. McDougall, *Outline of Abnormal Psychology,* 1926, 444.

[2] McDougall, The chemical theory of temperament applied to introversion and extroversion, *J. Abn. & Soc. Psychol.,* 24, 1929, 293-309.

[3] J. P. Guilford and K. W. Braly, An experimental test of McDougall's theory of extroversion-introversion, *J. Abn. & Soc. Psychol.,* 25, 1930, 382-389; also J. P. Guilford and J. M. Hunt, Some further experimental tests of McDougall's theory of introversion-extroversion, *J. Abn. & Soc. Psychol.,* 26, 1931, 324-332.

[4] M. F. Washburn, et al., Experiments on the relation of reaction-time, cube fluctuation, and mirror drawing to temperamental differences, this JOURNAL, 41, 1929, 112-117.

[5] Guilford and Braly, *op. cit.,* 388.

[6] Guilford and Hunt, *op. cit.,* 329.

[7] J. M. Hunt and J. P. Guilford, Fluctuations of an ambiguous figure in dementia praecox and manic depressive patients, *J. Abn. & Soc. Psychol.,* 27, 1933, 443-452.

4 times that of manic depressives. If these pathological types are admitted to be the extremes of I-E, this fact further supports McDougall's theory.

By way of securing an improved criterion of I-E, Guilford and Guilford[8] have constructed and statistically analyzed what they consider to be a typical test of I-E of the questionary type. Thirty-five items were selected from a tabulation of the terms describing extraverts and introverts in the writings of recognized authorities on the subject, and from tests and rating scales in actual use. These items were selected on the basis of greatest frequency and of minimal overlapping, and were incorporated into a test. The 35 items are presented in Table I in the form of brief descriptive phrases. The exact form of wording of each item as it appears in the questionary may be found in the study just referred to. The test was given to nearly a thousand *Ss* and the data were analyzed according to Thurstone's[9] method of multiple factor analysis. By this method a surprisingly large number of factors were revealed in the test, 4 of which were thought significant enough to be described. Three of these 4 were identified as (1) *a tendency to shrink from the environment;* (2) *emotional sensitiveness;* and (3) *egocentrism.* They were found to be very closely related to one another, giving the appearance of one more inclusive variable. The fourth factor was called *impulsiveness,* which seems to correspond more closely to McDougall's conception of I-E. It is apparently independent of the other 3 factors, and it would be almost completely obliterated in the total score on the usual I-E questionary. A further purpose of the present study was to test the possibility that this factor of *impulsiveness,* or any other group factor within the test, correlates with fluctuation rate. In order to touch any possible personality trait which is represented in the typical I-E questionary, we correlated the fluctuation rate with every item separately, as well as with the total score.

The total score in the Guilford questionary was based upon differential weightings[10] for "Yes" and "No" responses to each item. These weightings were obtained by an application of Thurstone's[11] method of similar reactions to the data obtained from 930 *Ss.* In the present study, data were obtained from 95 *Ss,* all university students. As usual, self-ratings were secured, and in addition the ratings of two associates, close friends or relatives, on every *S.* The reliability of the questionary, used as a self-rating device, has been found to be 0.84, test-retest, with 120 *Ss.* The correlations between self-ratings and associate-ratings in this study were 0.482 ± .053 and 0.586 ± .048, the two different associate-ratings for each *S* being correlated separately. The correlation of the two sets of associate-ratings was 0.609 ± .046. The correlation of the self-ratings with the means of the two associate-ratings was 0.618 ± .045. It would appear that the associate-ratings are about as valid as the self-ratings, and so they were given equal weight with the self-ratings in pooling the 3.

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[8] J. P. Guilford and R. B. Guilford, An analysis of the factors in a typical test of introversion-extroversion, *J. Abn. & Soc. Psychol.,* 29, 1934 (in press).

[9] L. L. Thurstone, Multiple factor analysis, *Psychol. Rev.,* 1931, 38, 406-427.

[10] J. P. Guilford, An examination of a typical test of introversion-extroversion by means of the method of similar reactions, *J. Soc. Psychol.,* 4, 1933, 430-442.

[11] L. L. Thurstone, Theory of attitude measurement, *Psychol. Rev.,* 36, 1929, 222-241.

The 95 *Ss* observed the outline cube for a total time of 30 min., this time being divided into 3 sets of observations made on 3 different days. Guilford and Hunt[13] have found that a very reliable measure of fluctuation rate can thus be obtained. The number of changes per min. was taken as the fluctuation rate. Other conditions were almost exactly the same as in the study just referred to. *S* was instructed to take a passive attitude, since this has been found satisfactory.

TABLE I

THE TEST ITEMS AND THEIR CORRELATIONS WITH FLUCTUATION RATE AND WITH THE KEY ITEM FOR THE IMPULSIVENESS FACTOR

	Meaning of the item	r_{kf}*	r_{k30}†
(1)	Expresses self better in speech than in writing........	.105	.157
(2)	Limits acquaintances to select few..................	.115	—.110
(3)	Prefers to take the lead in group activities...........	.052	
(4)	Prefers to read about a thing rather than to experience it	.157	
(5)	Likes detailed work..............................	.042	
(6)	Particular about dress and personal property.........	.000	
(7)	Considerate of other people's feelings...............	—.051	
(8)	Inclined to act on the spur of the moment............	.249	.348
(9)	Has kept a personal diary of own accord.............	—.088	
(10)	Works much better when praised..................	.093	
(11)	Likes to change type of work frequently.............	.020	
(12)	Inclined to study the motives of others..............	—.072	
(13)	Daydreams frequently...........................	.018	.177
(14)	Prefers to work with others rather than alone........	—.016	
(15)	Worries over possible misfortunes..................	.076	.171
(16)	Is frequently somewhat absent-minded	—.161	
(17)	Likes to persuade others to his point of view.........	.091	.254
(18)	Keeps in the background on social occasions..........	.122	—.284
(19)	More interested in athletics than in intellectual things.	.048	
(20)	Dislikes to change opinions already formed...........	—.193	.216
(21)	Likes to speak in public..........................	—.001	
(22)	Prefers to work things out in his own way...........	—.019	—.217
(23)	Has frequent ups and downs in mood..............	—.120	.463
(24)	Inclined to be slow and deliberate in movement.......	.140	—.281
(25)	Feelings rather easily hurt........................	.090	.306
(26)	Enjoys getting acquainted with most people.........	.097	.265
(27)	Inclined to keep quiet when out in company........	.113	—.317
(28)	Adapts self readily to new conditions..............	.164	
(29)	Likes to confide in others........................	.001	.445
(30)	Expresses emotions readily......................	.053	
(31)	Thinks about self much of the time................	—.053	.159
(32)	Likes to be watched while working................	—.150	
(33)	Frequently rewrites social letters..................	.289	
(34)	Likes to sell things..............................	—.062	
(35)	Gets rattled easily in exciting situations............	.025	.200

* r_{kf} refers to the correlation between each item and the rate of fluctuation of the cube. The probable errors of these coefficients range from 0.063 to 0.069.

† r_{k30} refers to the correlations between item 30 and other items, only those coefficients more than 5 times their probable errors being given.

The mean of the rates was 21.6 with a standard deviation of 12.8, and the range was from 4.6 to 74.4 per minute. The inter-daily correlations of the rates were: days I and II, 0.894 ± .017; II and III, 0.897 ± .016; I and III, 0.713 ± .041.

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[13] Guilford and Hunt, *op. cit.*, 328.

When the three days' measures are pooled the reliability is much higher. Previous studies give a reliability coefficient (odds-evens) in the region of 0.96 to 0.98 for this test. A test with such high reliability and such a wide range of measurements must be a delicate indicator of some constant factor or set of factors in the individual. Studies now in progress aim to discover what some of the factors are.

Now for the crucial test, the correlation between fluctuation rate and I-E test score. Using the self-ratings alone, as has been done in the past, the correlation is only —0.047 ± .070. Using the pooled self- and associate-ratings, the correlation is 0.066 ± .070. This verifies the lack of relationship shown by previous studies, and it is more decisive because of the improved conditions of this study. We are even encouraged to generalize, and to maintain that there is probably no relationship whatever between the rate of fluctuation of an ambiguous figure and any questionary on I-E of the usual type.[13] The only loophole that we can now see is that some other fluctuating figure, perhaps even McDougall's windmill, might give positive results. Such results would mean that there is little or nothing in common between the fluctuating cube and the windmill, a very improbable event. The relationships between these two phenomena and between several varieties of fluctuation are now being investigated in our laboratory.

The next and final step was to compute coefficients of correlation between fluctuation rate and the individual test items.[14] Each test item was evaluated for each *S* by summing the 3 weights belonging to the judgments of the 3 raters. The correlations are given in Table I under the caption r_{kr}. According to the theory, these coefficients should all be negative, since the higher the weighting, the more extravert the individual on each item, as I-E has been traditionally conceived and as the statistical weighting of Guilford's study dictated. The highest coefficient, either plus or minus, is just 4.6 times its probable error, and only one other is anywhere near that size. One item—(33), Do you frequently rewrite social letters before mailing them?—correlated with fluctuation rate to the extent of 0.289 ± .063, and another item—(8), Are you inclined to act on the spur of the moment without thinking things over?—to the extent of 0.249 ± .065. ***But both of these correlations are positive while McDougall's theory calls for negative relationships.***

Let us now examine the correlation between rate and those items which contain the impulsiveness factor. The key item, *i.e.* the one most heavily loaded, with the impulsiveness factor, is number (30)—Do you express such emotions as delight, sorrow, anger, etc., readily? The correlation between this item and fluctuation rate is 0.053 ± .067, indicating no relationship. Those items which correlate relatively high with item (30), either positively or negatively, are probably loaded to some extent with the factor of impulsiveness. There are 24 such items. None of these correlations are high, to be sure, but their probable errors are in the region of ±0.02, so they have some meaning. In the column headed r_{k30} we have given the correlations between those 24 items and item number (30). If McDougall's theory were true, we should expect to find a significant negative correlation between item (30) and fluctuation rate. Whenever any item containing the impulsiveness factor correlates positively with item (30) it should correlate negatively with the fluctuation rate, and vice versa, and the amount of correlation should be in proportion to the saturation of the item with the impulsiveness factor, as indicated roughly by the correlation of the item with number (30). None of these requirements prove to be true. In 12 of the 24 cases there is a reversal of sign, and in 12 cases the signs are the same. The two items, (23) and (29), which correlate highest with item (30) correlate with fluctuation rate to the insignificant extent of —0.120 and 0.001.

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[13] This conclusion is further supported by the fact that one *S*, not included in the correlations, who had a fluctuation rate of 144.5 shifts per min., was just about average, or perhaps slightly on the extravert side on the questionary. Her abnormally high rate was verified 6 mo. later by another experimenter. Her outstanding temperamental trait as described by the experimenter was extreme vivacity, but not necessarily impulsiveness.

[14] The reliabilities of the individual test items have been found to range between 0.60 and 1.00, with 277 *Ss* (see reference in footnote 8).

One might in a similar way show that none of the other group factors bear any relationship to the rate of fluctuation of an outline cube, but it seems useless to do so, since the degree of correlation of the individual test items with the rate is so small in all cases. If we find no correlation with any of the traits which are expressed in the test questions, surely no larger factor made up of these traits will show any such relationship. The correlations of fluctuation rate with the key items of the other large common factors are only 0.122, 0.025, and —0.053, for items (18), (35), and (31) respectively. Surely none of the 4 main factors apparent in the typical I-E questionary are related to fluctuation rate.

CONCLUSIONS

(*1*) The results of previous experiments which indicate that the rate of fluctuation of an ambiguous figure cannot be used as an objective test of introversion-extraversion are verified by this study.

(*2*) Correlation of each individual item of the typical test of introversion-extraversion with fluctuation rate shows that with one possible exception no relationships exist between these measures.

(*3*) Examination of the correlations for the test items containing the factor of *impulsiveness* reveals that this factor does not bear any relationship to fluctuation rate.

(*4*) It is clear that no group factors residing in the typical test of introversion-extraversion bear any significant relationship to fluctuation rate, since no single items correlate with rate.

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DEMONSTRATION UNIT FOR PATTERNS OF APPARENT MOVEMENT

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It is a relatively simple matter to give a demonstration of apparent movement for a large or small group of students by alternately flashing two lights so placed that each throws its shadow of some opaque object at a different place on a wall or screen. A more interesting demonstration may be given, however, if the arrangement is such that the seen movement can be integrated into various patterns. The unit described here makes such versatile demonstrations readily possible.

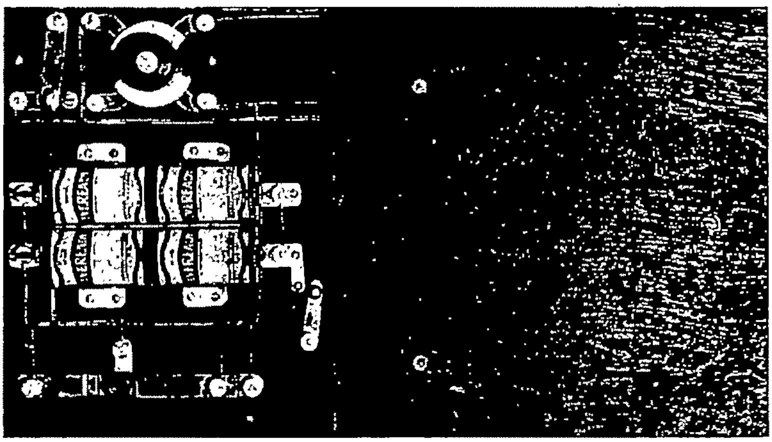

Fig. 1. A Self-Contained Portable Unit for Demonstrating Patterns of Apparent Movement

As an apparatus for demonstrating *phi* phenomena, this unit is fairly simple to construct and offers relatively little to get out of order. It can be used in large or small classes without darkening the room or making any other special arrangements. By reason of its compactness, the lecturer or experimenter can easily hold it so as to give all the observers in turn optimal conditions for observation. The basic ambiguity of some of the integrated patterns of movement instantly brings out individual differences in judgment or impression, and so creates interest in the phenomena.

The arrangement and operation of the device will be fairly obvious from the illustrations, one of which is a photograph of the actual unit, and the other is the wiring diagram. Two wooden panels, one (the base) 25 by 44 cm., and the other (the field) 25 cm. square, are hinged together at the right-hand end, so that the smaller panel, which in Fig. 1 appears folded down, may be easily turned up at right angles to the baseboard and locked by a bracket hook. The base panel carries the batteries, clips and switches; the field panel provides mounting for 5 lamps arranged in a 15 cm. square, with one lamp at the center. Small flash-light bulbs are used, and flash-light cells serve as the source of power.

As will be seen on referring to the diagram, Fig. 2, the circuit is a simple one. The on-off switch A is in series with the battery and must be closed when the unit is in operation. B, is a selector switch, which makes it possible to flash the lights in pairs as follows: 1–4 and 2–3 horizontal pairs; and 1–3 and 2–4 diagonal pairs.

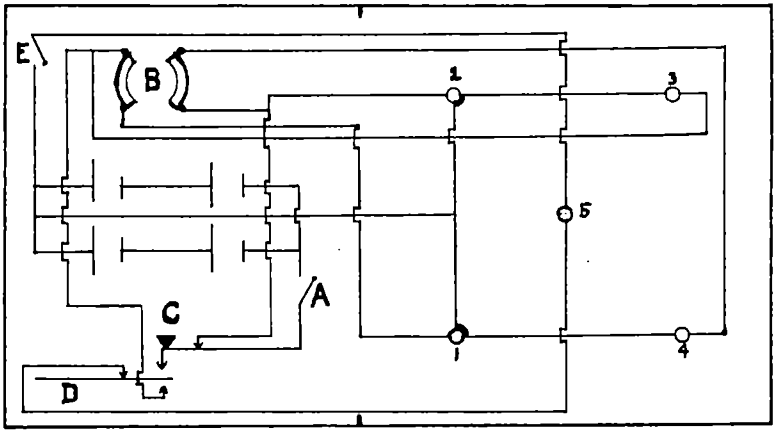

FIG. 2. DIAGRAM OF THE HAND KEY AND OF CIRCUITS FOR THE ALTERNATE FLASHING OF PAIRS OF LAMPS

The flashing of the lamps is controlled by the make-break key, C. When no pressure is on this switch one pair of lights remains continuously illuminated. A slight pressure serves to extinguish this pair of lights and more pressure on the key spring will close the lower pair of terminals and thus light the other pair of bulbs. An interval of about 1/15 sec. is optimal between the flashing of the pairs of lights but after observers have "seen" the movement, the rate can be varied through quite a range without causing the movement to disappear. Thus flashing the lamps by hand is satisfactory for demonstration purposes. When it is desired to use the center light, switch E is closed. Then the middle light is turned on and off in the mid-portion of the movement of the control button C.

The demonstration may well begin by the use of the diagonal pairs and without including light no. 5. If the unit is rested on a horizontal table, alternately flashing 1–3 and 2–4 will not only give the appearance of movement to the spectators who are sitting eight feet or farther away, but this movement, in most all cases, will assume one of two patterns: (a) reciprocal movement up and down and as a total resembling a seesaw; or (b) horizontal movement, the pattern giving the appearance of a double pendulum swinging back and forth about the unilluminated light no. 5 as a fulcrum.[1] Probably 95% of the spectators will see one or the other of these things happen, even though the light bulbs themselves are in clear view. Some *O*s report that the motion follows curved paths. Others are equally impressed with the straightness of the lines between the terminal positions.[2] As a rule, the *O*s cannot readily make a shift from a vertical to an horizontal movement configuration, *i.e.* from the seesaw to the pendulum, or in the opposite order, but if they continue to view the field for some time, the pattern will shift back and forth as in illusions of reversible perspective. Our preliminary data indicate that the seesaw is usually observed first when the four lights are equidistant and tends to dominate as a pattern. We will not undertake to discuss this finding at present.

The experimenter may hold the apparatus in his hands and, as he flashes it, rotate it on an axis perpendicular to the plane of the lamps by turning the entire base panel up on edge. Under these circumstances, if the *O*s maintain their respective vertical or horizontal movement patterns, these patterns should be exchanged by the shift in the position of the apparatus.

Covering one of the corner lights will, as a rule, destroy the seesaw or the pendulum interpretation and cause the substitution of another in which both vertical and horizontal movements appear. As an example, if light no. 1 is removed or covered, extinguishing the 2–4 pair and illuminating 3 will give the appearance that lights 2 and 4 suddenly concentrate in 3 and then 3 divides into its original components again. Similarly when the apparatus is rotated 45°, so that diagonal pairs assume vertical and horizontal positions respectively, flashing these pairs ordinarily gives no impression of a rotating pattern, but rather the appearance of movement toward the center of the field and then at right angles towards the periphery.

Having observed the appearance of a rotary pattern, it is very easy for the usual observer to get the impression of horizontal line movement from the left hand pair 1–4 to 2–3 at the right and back and forth. If the apparatus is rotated this "bar movement" will occur vertically instead of laterally.

The use of light no. 5 flashed between two pairs complicates the movement. The impression of the bar or straight line is destroyed, and each pair in turn seems to concentrate in the middle member and then to radiate from it.

A similar apparatus has been used for the purpose of gathering data in reference to the frequency with which different patterns are seen by naïve and practiced *O*s and the length of time that one pattern of interpretation persists before another takes its place. Although manual operation of the key is fairly adequate for class demonstration, we have found it desirable, for experimental work, to incorporate a mechanically operated switch for accurately controlling the interval and rate of flashing. A metronome with mercury contacts or a motor driven commutator may be readily substituted for the key, but either serves to reduce the portability and versatility of the unit.

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[1] Occasionally the pattern of a border moving clockwise or counterwise is seen.

[2] If ground glass or a sheet of tissue paper covers the lamps the movement will, of course, be a little more compelling. This, however, is not in the least necessary. Even though the bulbs are in full view, the illusion of movement is extremely strong. In fact, one undergraduate student, after seeing the demonstration made the following entry in his notebook: "The instructor connected small flashlight bulbs to a metronome, which moved back and forth."

MIRROR TONOSCOPES

By MILTON METFESSEL and HARRISON MUSGRAVE,
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Recent developments in stroboscopy[1] and in radio technique[2] have shown the way toward new types of tonoscopes. We shall describe two models. The feature distinguishing them from other pitch- and speed-indicating devices is an electrically-driven vibrating mirror. The vibrating mirror yields the stroboscopic effect in lieu of a flashing light. A series of delineations passing the vibrating mirror, at the rate of one delineation for each full vibration, will come into full view. By using a mirror instead of a neon bulb, the tonoscope can be used under any lighting conditions. A dark or semi-dark room is not required.

Vibrating-mirror unit. The sound to be examined is directed into a microphone. From there it is amplified by an especially designed voltage amplifier. The output from the amplifier is fed into two rochelle-salt-crystal motors,[3] wired in parallel. The crystals of Rochelle salt bend from their normal plane when a voltage is applied to their surfaces, in the same way that quartz does. Their torque has considerable force. The bending of the crystals is used to move lever-arms on the front face of the crystal motors, and the lever-arms are in turn attached to the upper edge of the mirror (1 x 10 in.) to be vibrated. The mirror is supported along the center of its long axis, so that it moves with a rocking circular motion. The attachment of the lever-arm of the crystal motor to the mirror's upper edge is not rigid. A small piece of brass tape supplies the needed flexibility.

The crystal motors have the characteristics of a capacity of about 0.013 μf, rather than that of an impedance such as conventional loud-speakers have. For this reason the output of most commercial amplifiers is not suited to the crystal motor. A power-amplifier causes the speaker to "howl" from mechanical and electrical feed-back. A voltage amplifier of from a 50- to 200-volt swing on the output (and no direct-current component) is more efficient. The accompanying diagram (Fig. 1) shows a simple amplifier with its constants which has operated the mirror unit satisfactorily. As in most laboratory work, a battery set is better than one operated by A.C. current. The first tube, together with its input and output transformers, should be completely shielded from the rest of the set, in a separate metal box, because the "57" tube is extremely sensitive. All of its leads should be as short as possible.

The rochelle-salt crystals have great power, but they are brittle. Care should be exercised not to move or jar the driving lever unduly, for the corner of the crystal to which this arm is fastened may break off inside the housing. If a crystal is broken, the cover of the housing may be removed and the drawing finger cemented to the opposite corner of the crystal.

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[1] M. Metfessel, Stroboscopy by means of impressed eye movements or mirror vibration, *Science,* 78, 1933, 416-417.

[2] C. B. Sawyer, The use of rochelle salt crystals for electrical reproducing and microphones, *Proc. Inst. Radio Eng.,* 19, 1931, 2020-2029.

[3] Crystal motors may be secured from either the Brush Development Co., 3715 Euclid Ave., Cleveland, Ohio, or the C. H. Stoelting Co., 424 N. Homan Ave., Chicago.

Model A. A laboratory set-up of the large model is shown in Fig. 2. In addition to the vibrating-mirror unit there is a drum (11 x 10 in.) turned by an electrical phonograph-motor of variable speed. There are 65 rows of sine waves inscribed on the face of the drum. When revolved at 78 r.p.m., the range of the number of sine waves in each row passing a given point in a second is more than one octave (from 126.7 to 265.1). Each successive row increases with a value equivalent to one-tenth of a step of the tempered musical scale. For the details of this scale the reader is referred to Seashore's description of his neon-light tonoscope.[4] The 65 rows on the drum of the mirror-tonoscope are grouped into 13 units of 5 rows each. The middle row in each group is the value of the note on the scale (C, C♯, D, etc.). The scale progresses straight across the drum, instead of doubling back on itself. We have found that the plan of grouping into 5 rows has certain perceptual advantages. A total pattern of stroboscopic movement in five rows makes it easy to find the single row with the smallest amount of movement.

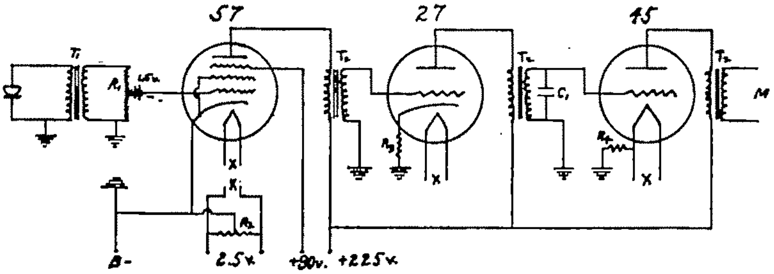

FIG. 1. AMPLIFIER FOR MIRROR TONOSCOPE

T_1 = Microphone input transformer (Kenyon)
T_2 = 3 to 1 audio-transformer (Amertran)
C_1 = 0.01 microfarads
R_1 = 250,000 ohm potentiometer
R_2 = 50 ohm center tap resistor
R_3 = 3000 ohm biasing resistor
R_4 = 1250 ohm biasing resistor
M = output to crystal motors

In singing a note, then, the singer attempts to make the sine waves in the middle row appear to stand still. In case of failure, the number of tenths of a step he is sharp or flat can be noted by determining which row to the right or left of the middle appears to be stationary. For the octave above the range of sine waves, C_2 to C_3, the notes appear as a chain. This permits an easy identification of the right octave.

To make these sine waves, we developed a neon-tube oscillator. (Neon tube, variable condenser, resistance, and B batteries.) The frequency of this oscillator may be controlled by varying the condenser capacity. The output was amplified by one tube, and fed into the moving coil of a dynamic speaker on which a mirror was mounted. The field of the speaker was excited by a B battery. A wooden drum was mounted on top of the Seashore tonoscope, turning with it. Strips of 35 mm. film were wrapped around the wooden drum. Then, using the light of the neon bulb of the oscillator to illuminate the dots of the tonoscope, the oscillator was timed stroboscopically so that its frequency matched the row of dots to be photographed. A light beam was arranged with a shutter so that the light would fall on the dynamic speaker's mirror for exactly one revolution of the tonoscope and drum. Pressing a button did this automatically. When the oscillator was timed to the row to be photographed, the button was pressed. This exposed the moving-film strip to the oscillating light line reflected from the vibrating mirror on the dynamic speaker. Five wave trains were photographed on each film strip.

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[4] C. E. Seashore, The measurement of pitch intonation in singing and playing, *Univ. Iowa Stud.: Ser. on Aims and Progress of Research,* No. 172, 1929, 29.

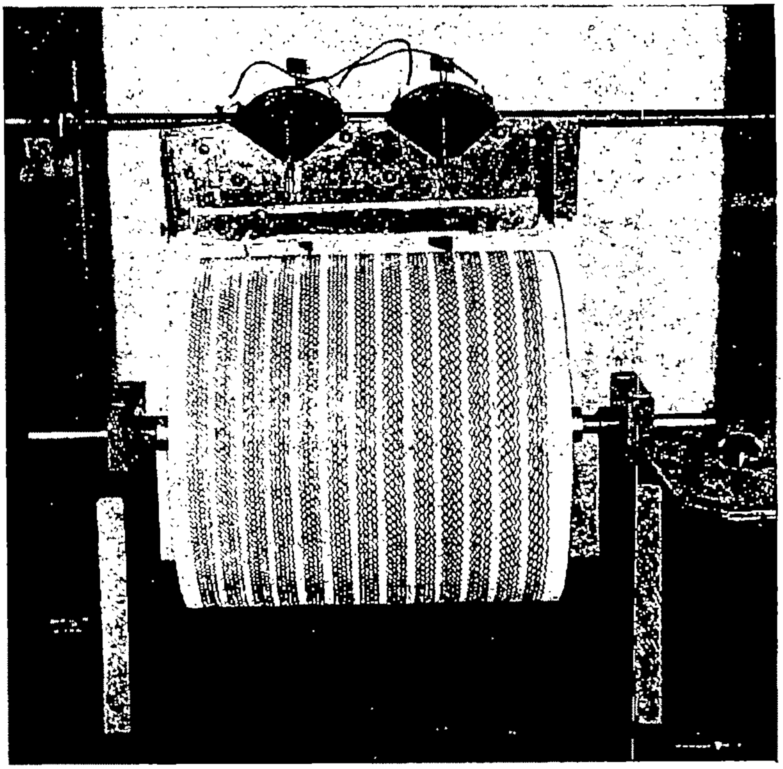

FIG. 2. MIRROR TONOSCOPE

One large negative was made by photographing all 13 strips together, so that prints can be made from the final negative with little cost.

Model B. The vibrating-mirror unit of this simpler model consists of the same amplifier as Model A, but has only one crystal motor which vibrates a mirror 1 x 4 in. Instead of a drum with 65 rows of sine curves, there is a disk 12 in. in diam. which is placed on a phonograph turn-table. The disk has 12 annular rings with the number of radial lines in each ring equivalent to the half-steps of the tempered musical scale (international pitch), when revolved at 80 r.p.m. A picture of the disk may be found in a previous article.[5]

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[5] M. Metfessel, *Science*, 78, 1933, 417.

INSTRUMENT FOR MEASURING THE SERIAL REACTION-TIMES OF LARGE MUSCLE GROUPS

By Malcolm Campbell, New York University

The apparatus described here permits the measurement of serial reaction-times from large muscle groups such as in the arm, the leg, the neck, or trunk. It is so constructed as to minimize preëxperimental differences between musculatures in accuracy of movement.

The reaction-block (Fig. 1) is diamond-shaped having an aperture in the center and a small light at each of the four corners. These lights supply the stimuli. *S* reacts to them by thrusting a short baton through the aperture in the reaction-block and striking the key adjacent to the illuminated bulb. Closing this contact causes the light to be turned off at the same time turning on another. *S*'s task is one of pursuit in which he sets his own speed. Two factors operate to minimize the factor of accuracy as differential between musculatures. First, the four contacts are within an inclosed space which is small enough so that *S* need never move his baton more than 3 in. to reach a target. The targets or contacts are themselves 1½ in. wide so that they are difficult to miss. Secondly, each target is in a corner of the diamond-

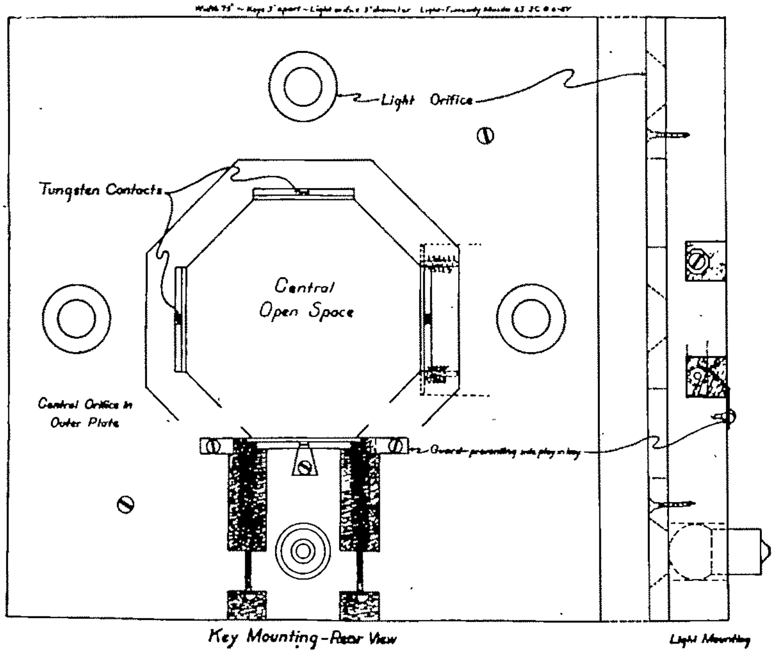

Fig. 1. Stimulus-Unit for Hand and Foot Reactions

shaped aperture or inclosed space. An inaccurate stroke would glance off the side of the aperture onto the key.

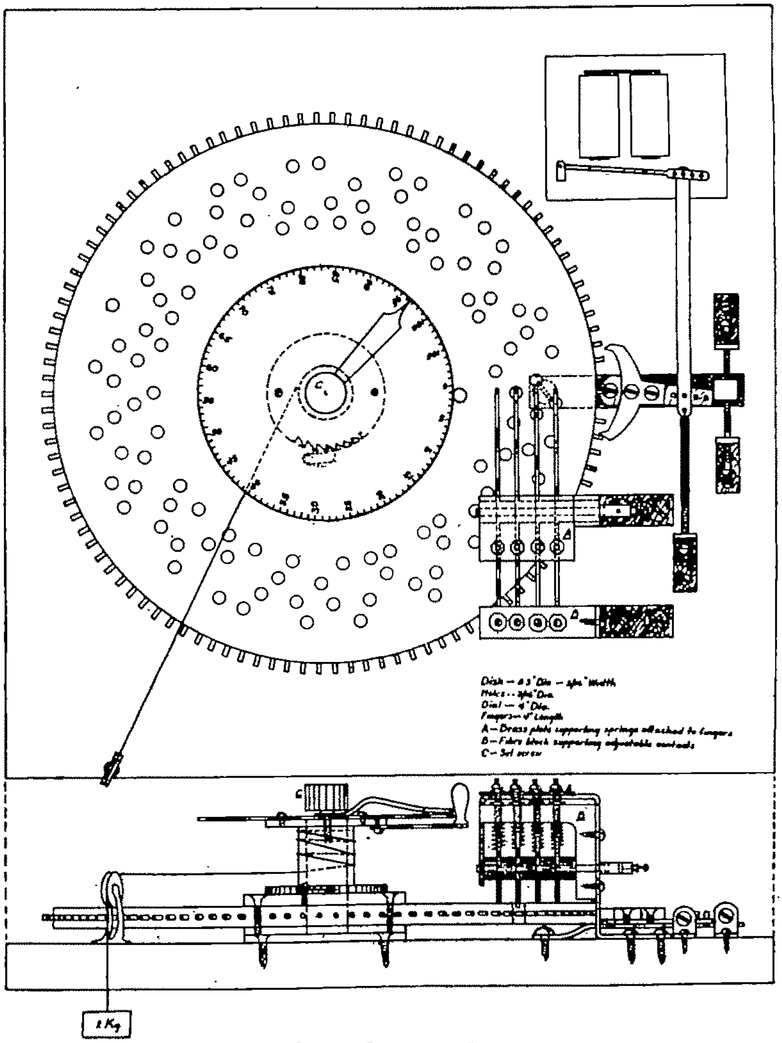
Dial — 4" Dia.
Fingers — 4" Length
A — Brass plate supporting springs attached to fingers
B — Fibre block supporting adjustable contacts
C — Set screw
A
B
C
1 Kg

FIG. 2. SERIAL STIMULUS-SELECTOR

An essential supplement to the reaction-block is the stimulus-selector (Fig. 2) which makes the presentation of stimuli automatic and in random order. The principal feature of this apparatus is a fiber disk which is mounted on an axle and is energized by a weight attached to a string wound around the axle. Into the edge of this disk are driven, at equal distances, 110 pegs. A clock-work escapement mechanism engages these pegs and is itself operated by the arm of a small door-bell magnet. *S*'s baton, in striking a target in the reaction-block, closes the circuit through the magnet which operates the escapement thus allowing the disk to revolve as far as the next peg. The movement of the disk in turn causes the magnet circuit to be opened again.

The selection of stimuli is the function of 4 metal fingers which are mounted on an axle over the disk. A small coiled spring attached to one side of each finger causes the tip of the other side to rest on the disk. As the disk revolves the 4 fingers trace concentric circles on its surface. On the surface of the disk are 110 radii, one for each peg on the circumference. On each radius a hole is bored through the disk. This hole is bored at the point where the radius intersects one of the concentric circles traced by the 4 fingers. For successive radii the holes are through different circles so that the order of succession is random between the 4 circles. At any point of rest of the disk the tip of one metal finger protrudes slightly below the disk's surface into a hole. The other end of the finger is correspondingly raised and a contact is made there which closes the circuit for one of the lights in the reaction-board. When any finger tips are not protruding into a hole of the disk, of which there is but one per radius, the contacts on their opposite ends are broken and the corresponding lights are out. Thus, one light appears to *S* at a time and the succession is random through 110 reactions before repetition begins. This is ordinarily too long a series to be learned in a few trials. Changing the connections of two or more fingers would institute a new pattern in case learning is suspected.

A mechanical counter may be introduced by numbering the radii of the disk and observing how many pass a fixed point during a trial. In operation with the arm the baton is simply held in the hand. No wires are necessary in connection with the baton. When the leg is employed *S* may be seated on a stool high enough so that the leg may swing freely from the knee and a reaction-block mounted in front of his foot. A baton is fixed to a device similar to a skate with the runner or rollers removed and is strapped firmly over the shoe.

A LECTURE-ROOM DEMONSTRATOR OF THE VISUAL TAU EFFECT

By EDWARD W. GELDREICH, University of Cincinnati

Aside from its actuality as a psychological experience, the Tau Effect is a lecture-room curiosity by virtue of its efficacy in demonstrating the actual dependence of space (perception) upon time.[1] Thus, if three discrete points of light are successively presented, and the distance between the points of light is equal, but the time interval between the first and second points of light is greater (lesser) than the time interval between the second and third, then the perceived space between the first and second will be greater (lesser) than the perceived space between the second and third points of light. To the psychologist, this phenomenon offers a multitude of problems, and forces the factor of time before his notice as a factor not to be

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[1] H. Helson and S. M. King, The tau effect: An example of psychological relativity, *J. Exper. Psychol.*, 14, 1931, 202-217.

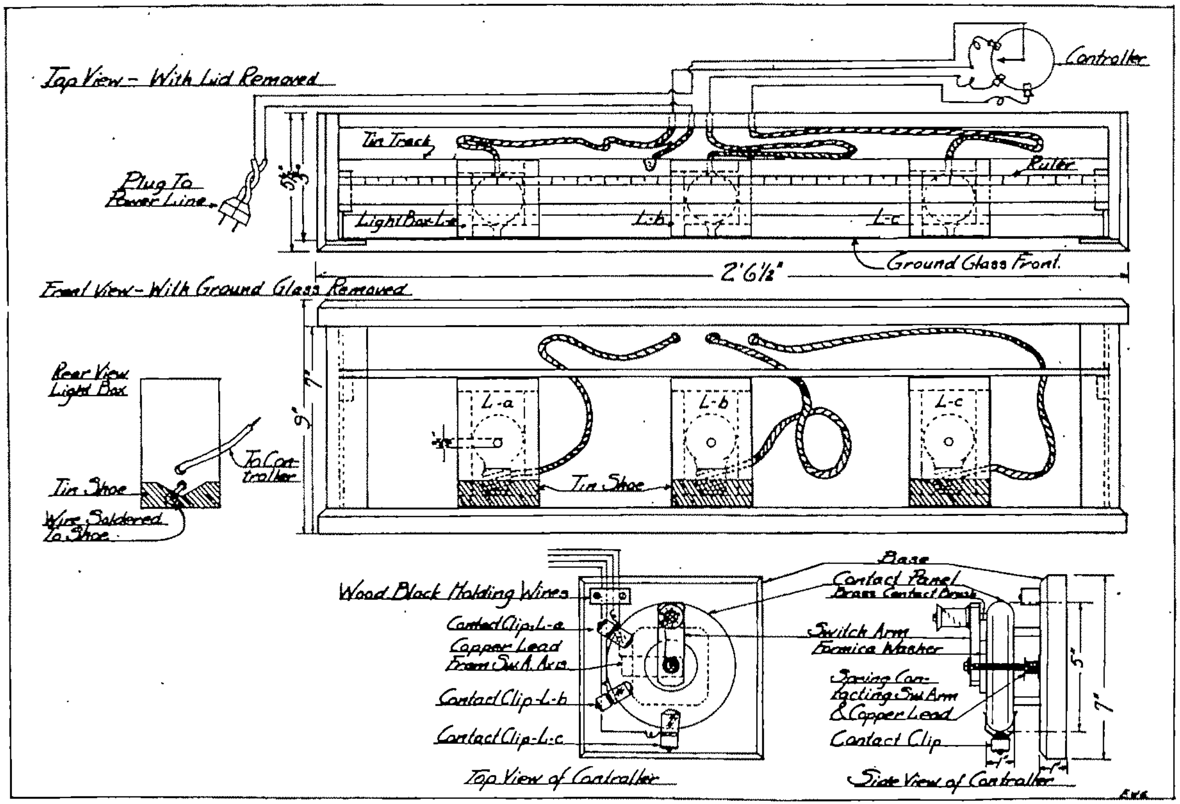

FIG. 1. DEMONSTRATOR OF THE VISUAL TAU EFFECT

slighted in any theoretical consideration of perception; but to the layman, the physicist, and the relativistic philosopher, the Tau Effect remains a curiosity and something to talk about. The apparatus here presented is a lecture-room demonstrator of the visual Tau Effect.

The demonstrator consists of a rectangular cabinet with a ground glass front and a lid, three light boxes each containing a 15-watt, 110-volt frosted lamp, and a manual controller made up of a revolving switch arm, three removable and adjustable contact clips, and a circular contact panel. (See Fig. 1.)

Construction features of the demonstrator are: (1) a tin track upon which the three light boxes ride, so as to make contact with the track by means of tin shoes, thus establishing a common return for the circuit; (2) a piece of glazed, 6-in. polygraph paper, pasted on the back of the ground glass front of the cabinet to obliterate the 'shadow-like effect' of the light boxes through the glass when just the ground glass is used; (3) a freely moving switch arm (its circuit is completed by means of a copper lead, a spring making contact with the copper lead and the base of the switch-arm axis, a metal washer at the other end of the switch-arm axis, a soldered wire from this washer to the brass contact at the distal end of the switch arm which in turn makes contact with the clips); (4) home-made brass clips bent just enough to pinch the contact panel firmly; and (5) a ruler running the full length of the cabinet against which the distance between the points of light can be read.

The demonstrational features of the apparatus are: the possibility of demonstrating the effect in a lighted lecture room, the ease with which the time ratio between the successive occurrence of the points of light can be changed, simplicity, freedom from the possibility of suggesting deception, and ruggedness of construction. The apparatus may be used also to demonstrate the phi-phenomenon.

NOTES AND DISCUSSIONS

FACULTY PSYCHOLOGY AND PERSONALITY TRAITS

Noteworthy is it that long after a psychological concept has proved untenable and has been ostensibly abandoned, it often survives in disguised form. For example, in spite of the fact that psychologists generally have rejected the term 'faculty' and the concept for which it stands, some investigators continue to speak and to write in a manner which suggests that they are still thinking in terms of a faculty psychology. In *The Commonwealth Teacher-Training Study* Charters and Waples present a list of 83 'traits' judged by competent critics of teachers to be extremely important.[1] "Competent" individuals were first interviewed to discover what they believed to be the traits most essential for success in teaching.[2] These individuals explained the meaning of each trait by giving examples (so-called trait-actions) wherein each trait was exemplified. Traits and trait-actions were matched in the manner following:

> "When the interviews were completed and the materials compiled in preparation for the next step, the result appeared as a list of trait names used by the persons interviewed and a list of several hundred trait actions, such as 'knows how to meet people,' 'endeavors to correct his faults,' 'keeps his records accurately,' 'does not shift responsibility,' 'helps willingly when asked to do so,' and so on. It then became the task of the translators, by consensus, to determine the traits expressed by the trait actions. The method of carrying out this process is described as translation."[3]

TABLE I

TRAIT-ACTIONS ILLUSTRATIVE OF "ADAPTABILITY"

1. Shows willingness to put up with a poor school system, and an unfriendly community.
2. Knows how to meet people.
3. Likes to be wherever he is.
4. Fits himself into the system.
5. Mingles with one class as well as with another.
6. Shows willingness to change method when he finds that the children are not being reached.
7. Becomes a part of the community in which he is teaching.
8. Shows the desire to adjust himself to new situations.
9. Does not make a fuss over irregularities of classroom procedure, when the room is cold, for example.
10. Meets the people of the community on their own level.
11. Does not dance or play cards if the community objects.
12. Shows willingness to readjust his theories and practices to existing needs and conditions.
13. Does not insist on having his class scheduled at a particular hour.
14. Moves about the place as if he belonged there.
15. Makes the best of every bad situation.
16. Observes local customs and conventionalities.

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[1] W. W. Charters and D. Waples, *The Commonwealth Teacher-Training Study*, 1929, 223 ff.

[2] *Op. cit.*, 15.

[3] *Ibid.*, 55.

When the translation was completed each of 83 traits was defined by a short list of trait-actions. Thus the investigators proceeded: (1) reports of experts or competents were employed to ascertain the characteristics of a good teacher; (2) the somewhat uncontrolled stimulation resulted in complex mutterings and confused descriptions such as "likes to be wherever he is" and "endeavors to correct his faults." These descriptive statements were labelled 'trait-actions;' and (3) the translators thereupon decided which of the descriptive phrases illustrated each trait. By this process of specious reasoning, "Shows willingness to put up with a poor school system" and "Likes to be wherever he is" were alleged to reflect 'adaptability.' Fourteen additional trait-actions, illustrative of 'adaptability' were set forth (see Table I). All appear so vague that it is doubtful that any other group of "competents" would associate the trait 'adaptability' with the descriptive phrases in this confusing miscellany.[4]

Because many of the 83 traits were nearly synonymous,[5] and because such a large number of traits is presumably unwieldy (for instructional purposes), the 83 were telescoped into 25 families of closely related traits. Each of these 25 families was assigned a general generic trait heading, followed by similar traits. The generic headings and the closely allied traits are presented in Table II. In Table II (Item D) the terms "carefulness," "accuracy," "definiteness," and "thoroughness" are to

TABLE II

FAMILIES OF TRAITS JUDGED TO BE EXTREMELY IMPORTANT BY COMPETENT CRITICS OF TEACHERS

A. Adaptability
B. Attractiveness (personal appearance)
C. Breadth of interest (interest in community, interest in profession, interest in pupils)
D. Carefulness (accuracy, definiteness, thoroughness)
E. Considerateness (appreciativeness, courtesy, kindliness, sympathy, tact, unselfishness)
F. Coöperation (helpfulness, loyalty)
G. Dependability (consistency)
H. Enthusiasm (alertness, animation, inspiration, spontaneity)
I. Fluency
J. Forcefulness (courage, decisiveness, firmness, independence, purposefulness)
K. Good judgment (discretion, foresight, insight, intelligence)
L. Health
M. Honesty
N. Industry (patience, perseverance)
O. Leadership (initiative, self-confidence)
P. Magnetism (approachability, cheerfulness, optimism, pleasantness, sense of humor, sociability, pleasing voice, wittiness)
Q. Neatness (cleanliness)
R. Openmindedness
S. Originality (imaginativeness, resourcefulness)
T. Progressiveness (ambition)
U. Promptness (dispatch, punctuality)
V. Refinement (conventionality, good taste, modesty, morality, simplicity)
W. Scholarship (intellectual curiosity)
X. Self-control (calmness, dignity, poise, reserve, sobriety)
Y. Thrift

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[4] Evidence in support of this assertion follows in this paper.
[5] How, then, are we to know that there were 83 real traits?

be regarded not as synonyms but rather as a family [sic!] of closely related traits! The usefulness of the foregoing list is ingenuously described.

> "Administrators, teachers, supervisors in teacher-training institutions, and the like will be able to use the evaluated list in a variety of ways. The instructor of each course may check off from the list those traits for which he will be responsible. Critic teachers in practice schools can use it as a basis for the development of traits in which the student is found weak. The list can be made the basis for direct instruction, either in classes or with individuals, and students can use it independently for self-evaluation."[6]

That this work of Charters and Waples has been considered seriously is reflected by the following quotation from Charters which Lomax has presented in *Commercial Teaching Problems.*

> "Each student should be made aware of the ***amount*** of each trait he possesses, and in the case of traits in which an individual is weak, ***exercises should be provided*** by which he may ***strengthen his control of the trait.***"[7]

No doubt educators would be extremely grateful if Charters and Waples (or Lomax) would define precisely how the teacher may determine "the amount" of each trait that the student possesses or should possess. Others would be interested in knowing something about the "exercises" by which the student may "strengthen his control of the trait." In addition, it is pertinent to inquire why 83 traits, if they are something more than nebulous verbiage, ***can not*** be used for instructional purposes. If we really know what traits and what amounts of each characterize a good teacher, we should capitalize this vital information.

Are the traits listed in Table II unitary? The foregoing suggestion of Lomax raises certain queries: (1) Is each of the 83 traits that are listed in Table II to be considered unitary? (2) If so, what is the difference between a 'personality trait' and 'a faculty'? Is it possible that the term 'trait' posits only that which was formerly designated 'a faculty'? Certainly, in their discussions of "traits" many modern writers (including Charters and Waples) do not indicate precisely what they mean by a "trait." It is true that in one of his books Charters defines the trait as a type reaction.[8] But the low intercorrelations which have been secured between various type reactions listed under the categorical heading "honesty" make us inquire whether the type reactions have an appreciable degree of objective reliability and validity. Certainly, the suggestion that exercises may "strengthen the control" of a trait is difficult to understand except in reference to 'faculties.'

The validity of the Charters-Waples proposal is without doubt open to serious question. Most modern psychologists agree with Thorndike, who in 1903 vigorously denied that man possesses "faculties."[9] In denying the validity of the faculty concept Thorndike described this concept as follows:

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[6] *Op. cit.*, 18 f.

[7] P. S. Lomax, *Commercial Teaching Problems,* 1929, 96. Italics ours.

[8] W. W. Charters, *The Teaching of Ideals,* 1928, p. 33.

[9] E. L. Thorndike, *Educational Psychology.* II. *The Psychology of Learning,* 1913, 363 f. Thorndike was, of course, not the first to refute the "faculty concept." Pratt has recently written as follows: "In the hands of Ch. Wolff and the later phrenologists the doctrine of faculties fell into disrepute, and the pride and delight which Herbart took in dealing it what he considered a death-blow is re-

> "The mind is regarded as a machine of which the different faculties are parts. Experiences being thrown in at one end, perception perceives them, discrimination tells them apart, memory retains them, and so on. By training, the machine is made to work more quickly, efficiently, and economically with all sorts of experiences. Or, in a still cruder type of thinking, *the mind is a storage battery which can be loaded* with will power or intellect or judgment, giving the individual a surplus of mind to expend. General names for a host of individual processes—such as judgment,[10] precision, concentration—are falsely taken to refer to *pieces of mental machinery* which we can once for all get into working order."[11]

We have examined the trait-actions and the traits that are presented in *The Commonwealth Teachers-Training Study.* We believe that Charters and Waples are once more exemplifying a common human tendency; namely, equivocation or nominalism. The following describes the process of equivocation staunchly followed and defended by some character testers. Set up a name or label; measure or describe something else, assign the name or label to the thing measured or described. Charters and Waples of course did little or no measuring of the traits that are listed in their teacher-training study.[12] They simply attached descriptive labels to generic terms. This practice is a greater violation of common sense than that of the personality testers who measure, for example, the tendency of a child to indulge in overstatement, and then assert that the overstatement test is a fairly satisfactory device for gauging general character.[13] Equivocation is clearly evident in the interchangeable use of the terms; viz., general character and overstatement.

Attempts to deal with specifics. Small wonder that the student of contemporary psychology finds himself a bit confused when he reads the long list of 'personality traits' that have been spawned during the last two decades! Cogent criticisms of the so-called 'traits' are numerous. Link has described recently the extreme view of those who insist that generality characterizes personality traits. He writes as follows:

> Most people believe that "if a man is lazy he *is* lazy. If he is industrious he *is* industrious. If he is cheerful he *is* cheerful. If he is disloyal he *is* disloyal. If he is ambitious he *is* ambitious.[14] If he is good he *is* good. And if he is bad he *is* bad. . . . Nothing could be farther from the truth. The moral qualities are not absolute. They are not blanket qualities which cover an individual's entire range of life no matter under what circumstances he may live. On the contrary, moral traits are *relative,* and their nature depends upon a very wide variety of external economic, social and bodily conditions."[15]

The foregoing assertion is substantiated in part by the work of May and Hartshorne. These investigators found that such a trait as 'deceit' is far from a unitary

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flected to-day in almost every textbook of psychology in those sections treating of theories once held by psychologists but now discarded." (C. C. Pratt, Faculty psychology, *Psychol. Rev.,* 36, 1929, 142.)

[10] Traits in this and in later quotations that appear also in the Charters-Waples list are identified by means of asterisks, see Table II, item K-1.

[11] *Op. cit.,* 363 f. Italics ours.

[12] In so far as the writers can discern by examining the published accounts.

[13] For criticism of this practice see the present writers' article, Further remarks upon character testing, *Psychol. Rev.,* 36, 1929, 91-95.

[14] See Table II, items N-1; P-3; F-3; and T-2.

[15] H. C. Link, *Employment Psychology,* 1919, 203 f.

entity. Indeed their work brings the very existence of this trait into question. Robinson has summarized their findings as follows:

> "When Professors Mark May and Hugh Hartshorne combined thirty-two tests of deceit they found that the composite scores of the children to whom the tests were given represented nothing of any practical importance. The most likely conclusion seems to be that deceitfulness is not a single general trait analogous to intellect. There are instead many types of deceit varying with the objective circumstances. Some of these individual traits are clustered into minor general traits, but one grand general deceitfulness seems to be largely fictitious."[16]

Another criticism of character measurement takes a somewhat different approach. Hollingworth has insisted that certain so-called traits are not so much traits of one individual as traits of a pair of people, or functions of the particular circumstances in which the individual is placed.

> "Many of these qualities or traits may be not so much traits of one individual as traits of a pair of people. A's coöperativeness[17] undoubtedly depends on the personality of B with whom he is coöperating. Should the resulting degree of coöperativeness be attributed to A or B? Apparently it should be applied to A and B as a team, rather than to either alone. Is a man's cheerfulness[17] a trait of the man or a function of the circumstances in which he is placed? We expect a stupid man to be quite generally stupid, and a musical person to be musical under all ordinary circumstances. Obviously coöperativeness and cheerfulness do not manifest themselves in this way, and such traits must be more closely related to circumstances and to social combinations of individuals than are the specific aptitudes and general competence."[18]

Hollingworth emphasizes a rather obvious fact; namely, that cheerfulness, coöperativeness, kindliness, and so forth, represent *effects produced;* he stresses also that these terms do not refer to psychological functions or basic elements of personality.[19]

One might continue almost indefinitely to cite quotations similar to the foregoing, but the field is familiar to the psychologist. Over and over, a battery of tests designed to measure traits such as persistence, or aggressiveness, or honesty, yields results so unreliable and undependable (when compared with other criteria) that one is led to question the actual existence of the general trait. In numerous instances the instruments are very loosely conceived and are clearly equivocal. A survey of the nature of a wide variety of frequently used tests suggests that there is a fundamental limitation common to most of them. Trait testers appear to assume that whatever they may *name* has objective reality; many need not so much to improve the measures but to improve or change their thought regarding 'traits.'[20]

Is the quantitative approach to the study of 'personality traits' a valid one? In 1914 Wells attempted a comparative study and a synthesis of the 'trait' schemes that had been proposed previously by Cattell, by Hoch and Amsden, by Heymanns and Wiersma, and by Davenport.[21] Wells listed 14 traits and about 95 sub-traits. He sought to justify his attempt by the fact that:

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[16] E. S. Robinson, *Man as Psychology Sees Him,* 1932, 249.
[17] See Table II, items F-1 and F-3.
[18] H. L. Hollingworth, *Vocational Psychology and Character Analysis,* 1929, 169 ff.
[19] *Op. cit.,* 145.
[20] Robinson, *op. cit.,* 253.
[21] F. L. Wells, The systematic observation of the personality in its relation to the hygiene of the mind, *Psychol. Rev.,* 21, 1914, 295-333.

> "In the scientific study of the personality, the information must be cast in such a form as to give a judgment of the quantitative relationship of the subject to other individuals."[22]

Wells assumed apparently that science implies the quantitative approach; that we *must* use a quantitative method. Thorndike once stated that whatever exists exists in some amount and that "to measure it is simply to know its varying amounts."[23] McCall and others redacted this view, and Ruch indicated that he considered its acceptance basic in a program of educational measurement. Such statements may lead to invalid assumptions, since they fail to represent fairly the total situation. We simply do not possess adequate technique for making a quantitative study of personality. Indeed, many thoughtful workers have insisted that the quantitative study of an individual must always be accompanied or supplemented by qualitative evaluation (careful case-study) if useful results are to be secured.

Limitations of the quantitative procedure. Certain limitations of the quantitative approach are well illustrated in Wells' subjective analysis of traits.

> "Traits are important not only for themselves but according to their combinations.[24] The same self-assertiveness, for example, that leads one individual to realize his aspirations, leads one less well endowed into situations he cannot cope with, thus strengthening the mental balance of the one, and undermining that of the other."[25]

One might mention also the trait (or is it merely a descriptive label?) "enthusiasm" which appears as item H in Table II. Enthusiasm is likely to be desirable when accompanied by (or combined with) high intelligence. Accompanying low intelligence or an unfavorable environment (or both), enthusiasm may resemble ingenuousness.[26] Since a characteristic such as enthusiasm never exists in isolation from total behavior patterns, it may or it may not be a desirable characteristic. The desirability or undesirability of enthusiasm depends obviously not only upon enthusiasm *per se* but also upon a host of concomitants. If enthusiasm *is* never an isolated or a detached phenomenon, one may logically ask: What is the best way of dealing with and directing desirable and undesirable manifestations of this aspect of behavior?

It is sometimes said that the educated, well-adjusted individual is characterized by a superior scheme of values. In other words, education and adequate adjustment imply useful and appropriate enthusiasm. An important difference between the educated person and the ignoramus is that the educated person's enthusiasm is evoked by worthy goals and aspirations. For attainment of these ends, enthusiasm is of course essential. Therefore merely to increase or to decrease enthusiasm might or might not be desirable. When change is expedient, reapportionment or realignment of enthusiasm is essential. Thus, the trait can not be considered in the quantitative way suggested by Charters and Waples. Surely, *it is enthusiasm as a part of a complex situation that is important!*

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[22] Wells, *op. cit.*, 296.

[23] E. L. Thorndike, Measurement in education, *The Twenty-first Yearbook of the National Society for the Study of Education*, 1922, 9.

[24] William James and others had pointed to this fact previously. See James, *Talks to Teachers*, 1899, 114.

[25] Wells, *op. cit.*, 297.

[26] Trait terms such as 'ingenuousness' and 'enthusiasm' are linguistically or colloquially satisfactory and useful. However, in such usage, no scientific basis is assumed by us.

It will be recalled that in a foregoing quotation Charters and Waples suggested that the instructor may check off from Table II those traits for which he will be responsible. But, as has been emphasized by Robinson, "The more deep-seated traits are interwoven."[27] Moreover, as has been remarked by James, Wells, and others, it is *the combination* of traits that is really important in the highly integrated behavior of man. Do Charters and Waples consider "enthusiasm" to be an isolated, unitary, and quantitatively measurable entity which operates more or less independently and which can be "strengthened" by appropriate exercises? Do they assume that enthusiasm is a piece of mental machinery which we can once for all get into working order? The writings of these men suggest affirmative answers to the foregoing questions.

Charters and Waples, by means of an elaborate and expensive technique, attempted to define certain traits objectively. One is of course led to inquire whether the 83 alleged traits have any corporate existence; whether they are not simply verbalizations that are useful socially, but very misleading when an attempt is made to employ them scientifically. To the present writers it is certainly doubtful that these abstractions can be treated objectively; and serious question may arise regarding whether Charters and Waples have disentangled the categorical patterns sufficiently to establish and disclose the elements.

TABLE III

TRAIT-ACTIONS ILLUSTRATIVE OF CERTAIN OF THE TRAITS LISTED IN TABLE II

1. Settles things once for all when he does take an issue with a pupil.
2. Effectively prepares pupils for the next grade.
3. Overlooks the deficiencies of a suburban community.
4. Makes people feel that he knows his subject.
5. Gives up preconceived notions.
6. Does not make threats which cannot be carried out.
7. Does not waste words.
8. Wears clothes that are appropriate for school.
9. Compares other teachers' work with his to find his shortcomings.
10. Interprets and solves complex situations quickly.
11. Does not clear his voice frequently.
12. Studies at home in the evening.
13. Seeks more effective ways of doing work.
14. Keeps in touch with personal friends.
15. Conserves personal strength.
16. Knows where to find things.
17. Attends strictly to his own business.
18. Carries on a dinner conversation of value.
19. Discerns lack of interest and the reason for it.
20. Does what he believes to be best.
21. Reads good books.
22. Keeps skin in healthy condition.
23. Does not just sit.
24. Endeavors to correct his faults.
25. Pays bills promptly.

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[27] Robinson, *op. cit.,* 214.

TABLE IV

RESULTS OF AN ATTEMPT TO IDENTIFY THE TRAIT-ACTIONS THAT WERE ALLEGED TO BE ILLUSTRATIVE OF CERTAIN TRAITS

	Trait actions																									Total number of attempts	% correct	Rank of the Charters Waples choice
Traits	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25			
A	1		15		4			1		5								2	1					4		33	12½	3½
B								24						3				1				7				32	22	2
C		5	9	1					2			1	1						1		8					28	4	6
D	0	19				1	7						1			9										37	0	?
E	2		9		2						2			7					1	1						27	—	—
F		3	3						5					10					3							24	41	1
G		2				16										2	4			3					4	31	64	2½
H				5						1			3					1	0				17			27	63	1
I				2			4				9							12								27	44	1
J	21			5		1		4		1							1			3			1			37	14	2
K		1				5	3	1	1	16					2		1		8	4				1		43	2	9
L											3				21							14				38	55	1
M					1	1			3										1	15				2	8	31	—	—
N		1								1		21	3				1				1		4	1		23	3	6
O	3			14			1		1								1			2			3			25	8	4
P												3		7	1			11					2			24	13	3
Q								7								10						18				35	29	2
R			1		28				2															6		37	3	4
S									2	6			12			1	1	3	5							30	40	1
T					1				12			3	13										4	11		44	25	2
U	3					1				1						1						2		23		31	3	5
V								9	0		9						4	2		1	3				3	31	0	?
W		1	4						1			8				1					21					36	58	1
X				1		9	3				4	1			5		6	1	1							32	19	2
Y							7						1		3	1	1								8	21	33	2
																										Av.	22	3

The validity of definition in terms of trait-actions. As a means of studying the validity of the Charters-Waples technique for defining traits, the following investigation was made recently by the present writers. The 25 families of traits assembled by Charters and Waples were mimeographed as shown in Table II. Twenty-five trait-actions (illustrations of these traits) were obtained from *The Commonwealth Teacher-Training Study;* these also were mimeographed (Table III). In preparing Table III the writers deliberately avoided including certain trait-actions that obviously were tautological. For example, as an illustration of the trait "optimism" Charters and Waples present the trait-action, "Looks optimistic." Useless and flagrant repetitions of this kind were of course omitted.

Sixteen faculty members of a state university and 16 graduate students from a school of education were then instructed as follows:

> Table II contains 25 so-called families of "traits." Table III contains an equal number of corresponding "trait-actions" or illustrations of these traits. On the left margin before the letter indicating each family of traits, write the number (or numbers) of the corresponding trait-action in Table III.
>
> One helpful hint will be given; namely, two of the traits in Table II are illustrated twice each, two of the traits are not illustrated at all, and the remaining 21 traits in Table II are illustrated once each in Table III.

In making this investigation the writers assumed that if the trait-actions are valid descriptions of the *traits* which they are supposed to define, faculty members and graduate students should be able to determine with a fair degree of success the particular trait illustrated by each trait-action.

The results of the study revealed that no individual was able to match as many as one third of the trait-actions with the traits. The largest number of correct responses for any one judge was 8 (32%) and for the entire group of judges, the median number of correct responses was only 5 (20%).[23]

In Table IV the letters arranged vertically at the left represent the traits given corresponding letters in Table II, and the numbers arranged horizontally across the top of the table represent the trait-actions with corresponding numbers in Table III. Table IV is to be read as follows:

1 person thought that trait-action No. 1 best illustrated trait *A*.
15 " " " " " " 3 " " " "
4 " " " " " " 5 " " " "
1 " " " " " " 8 " " " "
5 " " " " " " 10 " " " "
2 " " " " " " 18 " " " "
1 " " " " " " 19 " " " "
4 " " " " " " 24 " " " "

According to Charters and Waples, trait *A* ("adaptability") is illustrated by trait-action No. 5 ("gives up preconceived notions"). Only 4 persons out of 32 (12½%) chose this trait-action correctly, but 15 agreed that trait-action No. 3 ("overlooks the deficiencies of a suburban community") best illustrates "adaptability."

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[23] Only 32 *Ss* were employed in this first investigation; additional data were later assembled, and the results set forth here were corroborated.

In Table IV the Charters-Waples choice is in bold face. At the right of Table IV the percentages of correct responses are given. For example, 12½% of the attempts made to find the trait-action that is supposed to illustrate[29] trait *A* were correct, 22% of the attempts to find the illustration of trait *B* were correct. In two instances (traits *D* and *V*), *none* of the efforts resulted in matching the trait-actions with the trait! For the entire list of 25 traits, the average percentage of correct response was only 22! Greater chaos could scarcely be imagined. Are we to conclude on the basis of these data that the "translators" who aided Charters and Waples were misguided or "incompetent"? Perhaps not. We believe that, because of overlapping of the trait-actions, the translators attempted to weigh an imponderable. Charters and Waples were aware of the fact that the trait-actions might illustrate more than one trait. But surely, if the trait-actions add to or enhance the clarity of the familiar trait name, one might expect something approaching greater unanimity in the matching.

What should a trait-action add to the trait name to bring about greater clarity and precision? One thing appears certain: The trait-action should convey an *additional,* if not a new idea, to the reader. Certainly, the trait-action should not be tautological. In some respects, a trait-action might well resemble a definition. Through expression *in action* or activity, it should portray an example of the trait at work. In a sense, therefore, a new idea should be expressed in the trait-action. Apparently, the Charters-Waples trait-actions do not satisfy these criteria.

The quantitative expression of certain personality traits. Let us examine further the problem of measuring traits. Assume for the moment that the term "intelligence" expresses a personality trait, and grant further that this personality trait is subject to rigid quantitative measurement. Degrees of intelligence may then be represented along the horizontal base-line of a normal distribution surface and the percentages of individuals displaying various amounts of intelligence may be represented vertically. Individuals who display an ordinary amount of intelligence fall near the middle of the distribution and individuals markedly intelligent or markedly unintelligent fall at one or the other extreme. He who falls near the extreme right of the curve (one highly intelligent) is considered an enviable deviate. For this and for certain other so-called traits it is valid to say "the more the better." But the foregoing statement of course does not hold for all traits.

Consider for a moment the hypothetical trait, 'emotional responsiveness.' Of this trait it is not valid to assert, "the more the better." The extreme deviate in either direction is to be pitied. The individual who falls near the average is perhaps fortunate. In other words, for certain traits, such as 'physical health,' 'mental balance,' 'intelligence,' 'reasonableness,' it is probably valid to say "the more the better;" but for certain other traits, 'emotional responsiveness,' 'self-confidence,' 'courage,' 'unselfishness,' 'cautiousness,' 'sociability,'[30] the statement does not apply. Measurement of the first type is obviously not directly comparable with measurement of the second.

It is, of course, apparent that individuals are distributed in the case of most traits according to the normal surface. And, in part, because we have found that beyond a certain point in a scale some traits are undesirable in certain environments, ve come to think of types, the introvert and the extravert, the honest and the est, the clever and the dull.

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[29] According to Charters and Waples.

[30] It is to be remembered of course that the very existence of these general traits as scientifically established 'quantities' has been questioned.

is practice would perhaps be satisfactory if two conditions held: (1) the would have to be quantitatively measurable; and (2) the dichotomy must be ht of as a result of *quantitative dispersion,* wherein the extremes are set apart a base-line which expresses the full range of the distribution.

several traits which have been somewhat clearly defined and rather reliably ured, homogeneous age groups vary according to the normal distribution surface. so-called "opposite types" are found at the extremes of the distribution sur- But many of our trait words have been developed to convey ideas which ignore ull range of the distribution and continue to operate under the domination e type concept. For example, of one man we remark that, "He has person- ." Of another we say, "He has *no* personality." Such statements rest almost ly on an all-or-none concept.

he foregoing concept of dichotomy probably exists when most of the trait ls listed in Table II are used. It is relatively easy to find words to represent extremes of these traits, but much harder to find terms to represent the *full ibution* of the trait. It is true that we can measure general intelligence with e degree of accuracy. We can (but perhaps seldom do) think of two types, dull and the bright, meaning the extreme deviates. This is true also of an abil- uch as arithmetical comprehension. But with character traits, the story is likely e quite different. A quantitative measure seems to be less valuable here. For ts such as courtesy and honesty (when they are not resting upon the all-or- e concept) involve an almost interminable number of reactions which make, present, specificity or precise description seemingly impossible. In spite of the , however, that the trait words lack scientific accuracy, they have been appro- ited by certain character testers; the patterns of behavior which they represent e been assumed to be well-restricted and somewhat precisely understood. But words themselves have been (and continue to be) simply society's means of veying gross, unanalyzed impressions, not of expressing rigid, accurate, and cific ideas.

Attitude words. There is still another reason for our dilemma in trait measure- nt. For pairs of opposites such as 'courteous' and 'discourteous' the normal curve ght be employed for representing the facts. But for scientific work a two-dimen- nal curve does not suffice. Many words represent much more than external, ob- tive facts. They signify certain aspects of an objective fact as well as the *attitude* the *reaction* that is aroused. Apparently there is no available method for partial- g out the relative importance of the external fact (or some aspect thereof) and e attitude of the speaker when these words are employed. This is patently true the 'trait' words; descriptive labels which are used to convey numerous attitudes, actions, and feelings.

Many factors combine to produce a total effect which is often described by a it term. Among these factors the momentary attitude of the person hearing the word a factor of no small importance.

That a trait rating represents an attitude or effect produced in addition to ob- rvance of objective behavior is not a novel thesis.[21] It is made apparent by the results that are obtained whenever trait rating is attempted. For example, one worker found that long acquaintance makes for *greater leniency* in rating.[22] This finding was doubtless due in part to the fact that with long acquaintance the individual becomes inured or adapted to eccentricities, and therefore the shortcomings of one's acquaintances are frequently overlooked or disregarded. Shen reports a positive relationship between friendship and a tendency toward overestimation of desirable traits.[23] Terman, Courtis, and others have reported that ratings on school work are influenced by the halo effect that is engendered by the pupil's personal traits. In

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[21] H. O. Rugg, Is the rating of human character practicable? *J. Educ. Psychol.,* 3, 1922, 30-42, esp. 37 f.

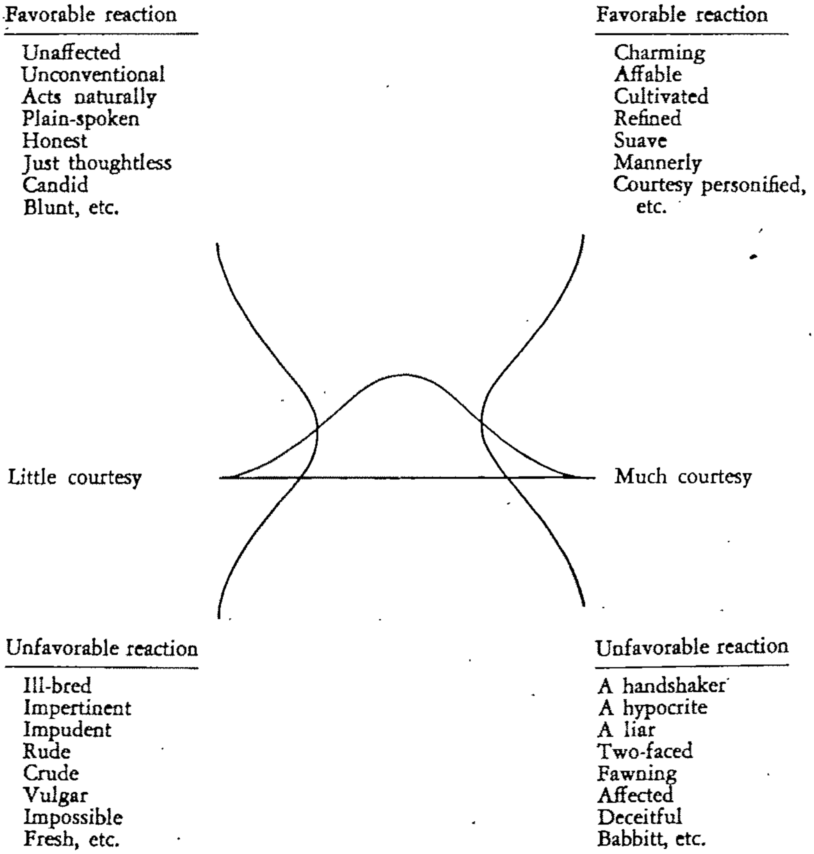

FIG. 1. VARIOUS ATTITUDES THAT MAY BE AWAKENED BY COURTEOUS BEHAVIOR

these illustrations we find again a major difficulty which confronts us when we use trait words. Adequate or even partial control of the many variables which may affect the trait is seldom provided. Surely it is obvious that if the scientific worker would retain his critical sense of fact, he must be very cautious in using *generic* terms in experimentation.

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[22] F. B. Knight, The effect of the 'acquaintance factor' upon personal judgments, *J. Educ. Psychol.*, 14, 1923, 129-142.

[23] E. Shen, The influence of friendship upon personal ratings, *J. Appl. Psychol.*, 9, 1925, 66-68.

A host of words are employed daily whose function is little more than that of expressing personal reactions or of description. Examples of such descriptive terms and their flexible use are frequent. An individual displays so-called 'thrift.' His friends say that he is 'economical' or 'thrifty.' His enemies remark that he is 'stingy' or 'penurious.' Another individual displays what some observers might call 'dignity.' Unfriendly observers accuse him of being 'proud,' 'vain,' or 'haughty.' Attitudes are of course the result of innumerable reactions; in a sufficiently large number of people, an identical situation or behavior may produce attitudes varying from extreme disgust to open idolatry. Although the excess freightage of an attitude word is not easily diagrammed, an attempt has been made in Fig. 1 to portray the implications of certain attitude words.

Fig. 1 reveals the various attitudes that may be awakened by any behavior that might be placed theoretically at some point along a distribution surface of 'courteous' behavior. By displaying extreme or unusual courtesy, an individual may arouse wholly different reactions in different witnesses. The foregoing statement holds also for the individual who is markedly lacking in what is ordinarily called 'courtesy.'

In Fig. 1 are at least 4 conceivable sets of terms which might be employed in describing one type of behavior. If the attitude aroused is friendly, one of the expressions in either of the two sets of phrases that are listed in the upper half of Fig. 1 will probably be employed to describe the person or his behavior. If, on the other hand, the observer's attitude is unfavorable or hostile, a term from the lower half of Fig. 1 (or a similar term) will probably be selected. Thus, the ratings that are attached to objective behavior are determined *by the observer* as well as by the behavior of the person eliciting the response. Possibly the incalculable nature of the *responses* that may be aroused by a given behavior is a chief reason why lists of so-called 'personality traits' are so unsatisfactory as measures of objective behavior. It is for this reason that it has been necessary to employ three curves instead of one in the construction of Fig. 1. With its three normal distribution surfaces, Fig. 1 illustrates merely the fact that two dimensions are inadequate for diagramming the total freightage of 'attitude words.' Most of the trait terms that are listed in Table II are obviously little more than attitude words.

Further remarks. The foregoing presentation gives facts which are clearly recognized by the thoughtful person. These facts, however, illustrate the difficulties encountered by those who attempt to measure traits and attitudes precisely. Certain ingenuous workers seem to have assumed the existence of 'faculties'—of static, unified, and more or less isolated characteristics which operate independently. Apparently Charters and Waples believe that verbal abstractions named traits have quantitative existence, and that they can be studied and defined with objective precision. Re-christening a 'faculty' by calling it a 'personality trait' is a matter of relatively minor importance. But the underlying concept appears essentially invalid in both instances.[34]

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[34] We have still to consider whether objectivity may logically be used in personality measurement. The answer is, Yes: within reason. But the objective ap-

Dashiell has recently commented on "the fallacy of distinct professions," *e.g.* the fallacious assumption that the learned professions may be distinguished readily by psychological traits necessary in persons engaged successfully in their pursuit.[35] Dawson has gone even further. Commenting upon the Charters-Waples trait terms that are listed in Table II, Dawson remarked:

> "Only one [term] 'scholarship' . . . seems to bear directly on what we have been accustomed to call teaching or even directed learning. Nearly all, if not all, of the remaining twenty-four items are desirable as traits of teachers as they are desirable as traits of all other decent and civilized people."[36]

The writers find themselves wholly in agreement with Dawson's statement for the reasons set forth in this paper. Moreover, the writers are a bit confused by Charters's enthusiastic assertion that, "the discovery of the trait is one of the most significant factors in human progress and happiness."[37] Just what is the essence of this discovery and who was the discoverer? What is the difference between a 'trait' and a 'faculty'? Is the 'trait' really a discovery, or is it merely a relapse in scientific thinking to the days of 'faculty' psychology?

SUMMARY

(1) The assumption that the so-called 'personality traits' are unitary is one that is of extremely doubtful validity. Acceptance of this assumption is, in the judgment of the writers, tantamount to an acceptance of a 'faculty' psychology.

(2) If 'personality traits' are not unitary entities, attempts to rate, to measure, and to discuss them in quantitative terms are hampered by the almost infinite numbers of reactions that are grouped under a generic trait heading. We have yet to see a character test or rating device which provides for or controls an appreciable number of the complex variables.

(3) In the light of available technique, it is doubtful that the quantitative approach to the study of personality is wholly valid. Certain traits are functions of individual reactions, of thinking, of pairs of individuals, of a situation, and of a particular concatenation of accompanying factors.

(4) Even if the so-called 'personality traits' were measurable, quantitative measures are not available for most of the traits that are listed by present-day workers. Until such measures are available, quantitative descriptions convey little precise meaning.

(5) For several reasons, the writers are convinced that the Charters-Waples attempt to define traits is not valid: (1) because the over-lapping of the trait-actions is excessive; (2) because many of the trait-actions convey no new or additional idea; and (3) because qualified students are unable to identify the traits which accompany trait-actions.

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proach must deal with adequate samples of behavior covered by descriptive terms. And objective measurement should be accompanied by or supplemented *by case-study and by subjective analyses.* We should admit frankly that, when we use most trait words, we are simply employing adjectival expression, which includes the *observation* of behavior and our *reaction* to the behavior.

[35] J. F. Dashiell, Personality traits and the different professions, *J. Appl. Psychol.,* 14, 1930, 297-301.

[36] Edgar Dawson, Review of The Commonwealth Teacher-Training Study, *The Historical Outlook,* 20, 1929, 357 ff., 358.

[37] Charters, *op. cit.,* 22.

improvement of personality are far more definite than is warranted by the facts themselves.

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Northwestern University — Paul A. Witty

DANDY'S RADICAL EXTIRPATIONS OF BRAIN TISSUE IN MAN

The psychological literature affords very little cross-reference to the results of neuro-surgery in spite of their great theoretical importance and interest for psychology. Such data are widely scattered among surgical, neurological and clinical reports and are concerned with specific therapeutic problems. However, the facts justify an attempt to bring certain results of the neuro-surgeon into brief, though inadequate, review for psychology.

Dandy has completely removed the right cerebral hemisphere from eight patients. He has performed total extirpations of one or more lobes much oftener. The results summarized in this note are from *right-handed cases only.* We cannot undertake to examine the critical differences between them and left-handed patients.

There are tabulated below certain generalizations on the effects of removing the right hemisphere. These generalizations derive from those cases affording the longest observation.[1] The object of this note is to present only those effects which stand out as univocal and consistent in their interest for psychology.

The operation was the complete extirpation of the right frontal, temporal, parietal, and occipital lobes peripheral to the corpus striatum. The weight of the tissue removed varies, with the pathological conditions involved, from 250 to 584 grm.

Coherent conversation began within twenty-four hours after operation, and in one case on the afternoon of the same day. Later examinations showed no observable mental changes. The patients were perfectly oriented in respect of time, place, and person; their memory was unimpaired for immediate and remote events; conversation was always coherent; ability to read, write, compute, and learn new material was unaltered. Current events were followed with normal interest. There were no personality changes apparent; the patients were emotionally stable, without fears, delusions, hallucinations, expansive ideas or obsessions, and with a good sense of humor; they joked frequently. They showed a natural interest in their condition and future. They coöperated intelligently at all times throughout post-operative care and subsequent testing of function. One patient was a clergyman and a year later was conducting vesper services from his wheel-chair in the sanitarium where he lived.

Cranial nerve findings after operation were as follows:

I. Smell seemed unaffected, though a check on the pre-operative sensibility was not always possible. Various odors were promptly named.

II. Complete homonymous hemianopia for form and color.

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[1] Cf. W. E. Dandy, Physiological studies following extirpation of the right cerebral hemisphere in man, *Bull. Johns Hopkins Hospital,* 53, 1933, 31-51; also Removal of right cerebral hemisphere for certain tumors with hemiplegia, *J. Amer. Med. Asso.,* 90, 1928, 823.

III, IV, VI. All extraocular movements were normal; no nystagmus.

V. The left trigeminal domain was remarkably preserved. Light touch was lost but slightly more intense stimulation with loose cotton was recognized and located quite accurately. Sharp and dull objects were easily differentiated, though less acutely than on the right side of the face. A pin prick gave pain. Heat and cold were felt, though less acutely than on the right side. The masseter muscles contracted on both sides, but were weaker on the left. The jaw was not deflected from the midline when the mouth was open. The corneal reflexes were acute and equal.

VII. Taste was preserved in part at least; perhaps it was somewhat dulled but pre-operative data were not available. There was definite facial asymmetry with some variation from case to case: smiling drew the left side of the mouth into slight movement. The left eyelid closed tightly. The forehead wrinkled weakly on the left.

VIII. Watch tests in all cases and an audiometer record in one showed that both ears were normal. There was no dizziness.

XI. Sternomastoid and trapezius muscles functioned, but more weakly on the left side.

XII. The tongue protruded normally in the midline.

Arm, leg, and torso findings after operation were as follows:

There was a left hemiplegia; no movement was possible in the left arm. A complete flaccidity of all muscles on the left side followed the operation and was present for several hours. In none of the cases did more than a mild rigidity develop within two months. The left hand tended to assume a claw shape, though the fingers were not rigid and could be manipulated easily. One person, who lived three and a half years after operation, exhibited contracture of his arm but not his leg. In general the foot muscles moved slightly but the toes could not be moved voluntarily nor the foot lifted from the bed. Voluntary movements of the knee and thigh were not possible. There was ankle and patellar clonus. The biceps and knee reflexes were greater on the left side, though varying somewhat from case to case. Plantar stimulation caused flexion of the leg but no movement of the toes. Abdominal reflexes were equal on both sides in some cases, though decreased on the left in other cases when they had been altogether absent on the left before operation. There was complete sphincter control. Cutaneous sensibility was abolished on the left side, except in the facial areas noted above. Deep sensibility of certain kinds was present, *e.g.* quite accurately localized pain from squeezing the muscles and pain from the manipulation of joints. The patients could identify which toe was being manipulated but not the direction of the movement.

Such cases have lived three and a half years after operation in apparently normal mental condition, though Dandy is careful to point out that it is better to say that no abnormalities could be *disclosed,* since hemiplegia obviously prevented their adjustment in competition with others.

Dandy has made many less radical extirpations, removing only one or two lobes. This work yields data[2] which can be outlined as follows:

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[2] W. E. Dandy, Effects of total removal of left temporal lobe in a right-handed person: localization of areas of the brain concerned with speech, *Arch. Neur. and Psychiat.,* 27, 1932, 221-224; Division of Brain Surgery in *Lewis's Practice of Surgery,* 1932.

Frontal lobes. The right or the left or both lobes may be removed with no loss of function, provided the line of excision is anterior to the prerolandic areas, the 'motor cortex,' and, in the case of the left lobe, anterior also to Broca's area.

Temporal lobes. Either the right or the left lobe may be completely removed, with the only permanent loss of function being incomplete homonymous hemianopia (right or left as the case may be). Transitory anomia does follow removal of the left lobe in right-handed persons, but disappears as the margin of the parietal lobe recovers from the trauma.

Parietal lobes. The right lobe can be removed with no loss of function provided the line of excision is posterior to the postrolandic area, the 'sensory cortex.' The left lobe, peripheral to the corpus striatum and posterior to the mesial half of the rolandic area, seems to have become localized for surgery as the most important area of the brain for mental functions. Loss of tissue from this region results in predictable and permanent loss of a variety of functions depending on the area removed.

Occipital lobes. Excision of the right lobe gives complete homonymous hemianopia and there is no other sign or symptom referable to this lobe. Removal of the left lobe produces complete homonymous hemianopia with no other loss, provided the line of excision is posterior to the supramarginal and angular gyri; if these convolutions are involved, visual aphasia results.

The *cerebellum* also can withstand the involvement of surprising amounts of tissue with no objective functional disturbance. The vermis can be incised lengthwise, the outer cap can be removed, even the cortex can be removed to a depth of 2 cm. with only temporary disturbance. However, the dentate nuclei can not be injured without devastating results.

Thus, increase in the number and amount of extirpations of brain tissue, plus the ability to carry out much operative procedure under local anaesthesia with the patient fully conscious, plus suitable unipolar electrodes for exploration, has yielded a quantity of 'experimental' data. It is of particular psychological interest that the evidence does not uphold such notions as mass action or equipotentiality with functional transfer from one region to another. On the contrary, it supports and defines specific localization.

Most of the misunderstanding on this question has arisen from faulty interpretation of data on tumors: the functions disturbed were so easily thought to be 'located' at the site of the lesion. The argument for transfer to another area arose when removal of the tumor and *immediately* contiguous brain tissue was followed by reappearance of the function. With increase of the data on radical extirpations it is seen that tumors in the surprisingly numerous silent areas exert great influence by pressure and oedema upon relatively distant and localized 'centers.' For example, the left frontal lobe has long been thought to be especially important for intellectual functions, and, while it is true that tumors so located give rise more frequently to mental effects than tumors in some other sites, this result is due to pressure and oedema of the left parietal lobe. A slightly different kind of evidence is afforded when the removal of the tissue from Broca's area to a depth of 2 cm. is followed by temporary loss of motor speech and its subsequent return; however, if the area is excised to a depth of 4 to 5 cm. motor speech never returns. Similarly, all other 'speech' functions seem to be localized in the left parietal region. Auditory and visual verbal apprehensions are separately localized; the latter, moreover, is independent of the visual area in the calcarine fissure of the occipital lobes. The accepted teaching of bi-temporal auditory representation, inferred from tumors in the temporal lobes, now gives way to the evidence from total extirpation of either temporal lobe with no impairment of hearing. The motor and sensory localization in the paracentral region has also become more certain.

An interesting throw-back to the once popular question of the 'seat of consciousness' is offered not only by the above data (removal of the right hemisphere or the left frontal, temporal, or occipital lobe) but especially by the fact that interruption of the left anterior cerebral artery, posterior by about 2.5 cm. from the anterior tip of the corpus callosum, is fatal to the reappearance of consciousness. The patient may live weeks, but consciousness never returns nor any of the voluntary movements in any part of the body, and under these conditions reflex movements can only be elicited by very strong stimulation. Hence the deep paracentral and parietal regions of the left hemisphere, near the corpus callosum, though not posterior to the splenium, would seem to have dominant importance for consciousness. This conclusion is also born out by evidence originating below this region, since pressure on the midbrain, pons and sometimes the medulla induces drowsiness or unconsciousness.

At the present stage of knowledge we can summarize the *critical* cerebral zones for the right-handed person as being the paracentral region including Broca's area, the left parietal lobe (most important for mental activity) back to and including the supramarginal and angular gyri, and the calcarine fissure in the occipital lobes.

Harvard University KENNETH DIVEN

OPINING

Obviously related to belief stands a group of topics which includes supposition, conjecture, doubt, query, conviction, and opinion. When these words appear in this form they suggest human inclinations toward questions of fact or truth. But if we say "believing," "supposing," "conjecturing," and so on, we suggest an active functioning of the organism and therewith an interesting and important group of psychological problems. Upon this entire group of operations psychologists have done very little work of a descriptive, not to say of an experimental, kind.[1] In his approach to the topics of opinion and opining from the field of public speech, the writer has found but little aid in psychological books and articles. He sets down, therefore, a preliminary sketch of a functional treatment of the subject in its more active form of opining.

Psychologists have agreed upon no clear and standard definition for 'opinion' and the more active 'opining.' If we accept for the moment the common definitions of the dictionaries, we may regard opinion, or *an opinion,* as a belief or persuasion held with or without assurance by an individual, but not generally agreed upon by the group or community. So far as the social sanction is concerned, therefore, only the individual is committed to the validity or truth of the opinion. However tenacious the individual may be in asserting and standing for his view, and however strongly he may be convinced of its validity, the sanction tends to be only an individual sanction. Where opinion is described as "stronger than impression, less strong than positive knowledge" (Webster's New International Dictionary) the definer evidently has in view objective or social acceptance rather than the conviction of the opiner. We commonly reserve the unpleasant epithet "opinionated" for unwarranted conviction and sturdy assertion. The verb "to opine," however, implies a certain modesty and reserve in expression, as if the opiner knew the supposititious and insecure nature of his views. In our further comments upon opining we shall assume this connotation.

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[1] A few exceptional titles may be noted. W. L. Gard, A preliminary study of the psychology of reasoning, this JOURNAL, 18, 1907, 490-504; T. Okabe, An experimental study of belief, *ibid.,* 21, 1910, 563-596; M. E. Bulbrook, An experimental inquiry into the existence and nature of 'insight,' *ibid.,* 44, 1932, 409-453.

We give a few hypothetical instances of this active process.

(1) Brown, a prominent citizen, is urged to attend a mass meeting on the local problem of taxation and to advise the community. The problem is new to him but he proceeds systematically with the task, formulates the problem, analyses it, and collects, arranges, and weighs evidence and argument. As he proceeds, several possible solutions of the problem occur to him. He examines each with care, weighing its advantages and disadvantages, and attempting to foresee the consequences of its adoption. Eventually he arrives at a conclusion regarding what should be done. He has formed "an opinion." He opines a certain solution.

This is an instance of thinking in the stricter sense of that term, "a search initiated by a problem or task and carried on by the aid of symbolic objects."[3] Under occasional instruction Brown formulates a problem, manipulates symbols (chiefly verbal) and finally arrives at a tentative solution.

Several aspects of the process deserve special notice. It should be observed, in the first place, that while it is primarily the function of thinking that we here see in operation, other psychological functions are also involved. In reading the Sunday paper Brown's eye is caught by an article on taxation, and he remarks, "Might throw light on our problem" (inspection and comment); he digests the contents of the article (comprehension); he reaches for his pencil to note down a set of statistics (action); he recalls his experience in dealing with a similar problem in the past (remembering) and he imagines the practical consequences of this solution and that. Indeed, it is doubtful whether there is any one of the basal psychological functions that might not be involved at some stage in the process.

One practical consequence of the manipulation of symbols during the second stage of the process is that one or more solutions become identified with, or incorporated in, existing action determinations. Brown may generally act in ways that will decrease his annual expenditure. As he examines the argument, it becomes clear that such and such a course of conduct would reduce his taxes. The result is that the course of conduct becomes incorporated in the general inclination to do whatever will decrease annual expenditure. It would be tedious and futile to attempt an enumeration of all the inclinations of the organism which might be involved in his thinking. But it is safe to say that the process might involve the individual's habits, concrete past experiences, dominant interests, and general relation to the social environment. It is probable also that current functional employment and inner organic states might be involved; fatigue, for instance, might lead Brown to accept a plausible solution without careful consideration of its consequences.

(2) In glancing through his favorite magazine, Riker comes upon an advertisement of a new safety-razor. He reads the persuasive statements which dwell upon the

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[3] M. Bentley, *The New Field of Psychology*, 1933, 89.

ment in the back of Ribot's *German Psychology of Today* published by the same house. Curiously enough James mentions McCosh in his index, referring to p. 501 in the first volume of the *Principles,* but this is a typographical error which should read p. 591. The citation is, however, to another book by McCosh, *Psychology, the Cognitive Powers,* 1886, and alludes to his discussion of the 'law of similarity' in connection with the topic of Association. In this otherwise remarkable book on *The Emotions* occurs a very striking passage. McCosh first discussed at length and with numerous citations the expression of the emotions in a variety of ways throughout the body. There is every indication that the author explicitly understood the literature and even advanced a step or two beyond it. For example in a previous section he wrote:

> "I have often remarked," says Burke, "that on mimicking the looks and gestures of angry or placid, or frightened or daring men, I have involuntarily found my mind turned to that passion whose appearance I endeavored to imitate." Here is an important fact, but it is not correctly stated; that which comes first is put last. The only effective way of mimicking a passion is to call up by the fancy an object or scene fitted to awaken the feeling.
>
> I rather think that sympathetic action is to be accounted for very much in this way: we put ourselves in the position of others, by calling up by the idea the same feelings, which go out in the same manifestations. Tears shed are apt to call forth tears in the beholder, or quite as readily in the listener to the tale told which makes us realize the position. It is the same with laughter, which is apt to be echoed back till the noise rings throughout a large assembly. When a company as a whole is moved it is difficult for any person to keep his composure.[2]

He made mention of what he called 'sympathetic imitation' or 'sympathetic action.' Now comes the section which is as clear a statement of the Jamesonian position as I have seen outside of James:

> We are here in the heart of a subject which cannot be cleared up at present —the reaction of mind and body. If it be true that emotion produces a certain bodily state, it is also true that some bodily states tend to produce the corresponding feelings. Dr. Braid, in his very curious experiments as to hypnotism, found that a person put in the attitude of devotion became devout. I am not disposed to speak dogmatically about this mysterious phenomenon, but I believe that association of ideas has to do with it. The act of kneeling will naturally suggest the feelings we cherished when we knelt. If we take the attitude of striking, the idea of fighting will be suggested. If the expression of affection, or of pity, is assumed, it will call up the feeling associated with it. In the very act of bringing a cloud on the brow the idea of care will be brought up.[3]

Obviously while the reversed process of bodily activity produces an emotional effect of a psychical nature it is not an exclusive process but an alternate possibility and, while the theory is consequently not so definitely worked out in a one-sided manner as in the case of James, it is, nevertheless, a very definite statement of that contingency.

It is hardly unlikely that this book was unknown at the time, since McCosh must have been a prominent scholar of the day. Dr. James McCosh (1811-1894) was a Scottish-American metaphysician and Presbyterian clergyman who became president of the College of New Jersey in 1868 and resigned the presidency in 1888.

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[2] J. McCosh, *The Emotions,* 1880, 102-103.

[3] *Ibid.,* 105.

By that time the institution had become known as Princeton College. Warren groups James McCosh with Thomas Reid and Dugald Stewart as members of the school of Scottish intuitionism. They regarded mind "as a collection of faculties, each fully capable of performing its function from the beginning. . . . But while their psychology is thus furnished with *a priori* elements for knowledge, the actual rise of knowledge is regarded as strictly empirical."[4] Cattell in his dedicatory address on the occasion of the inauguration of the Psychological Laboratory at Wittenberg College, specifically mentioned him in connection with the history of American Psychology. He said, "For earlier work tribute should in passing be paid to James McCosh, Presbyterian clergyman from Scotland and president of a Presbyterian college, who at Princeton promoted the study of organic evolution and physiological psychology. George Trumball Ladd, also a clergyman, was called to Yale in 1881 and developed there courses in physiological psychology, leading to the publication in 1887 of his *Outlines of Physiological Psychology*."[5]

Boring refers to him as "one of the last of the old school of philosopher-psychologists who therefore finds no other mention in this book."[6] Pillsbury heads a section in his history of psychology with McCosh's name—indeed the section just before that on James!—and says, "One of the men who deserve more than passing mention is James McCosh, who came to Princeton from Belfast in 1868 when disappointed in his efforts to receive the appointment at Aberdeen won by Bain. He was for long time professor of philosophy and president of Princeton University. An adherent in the strictest form of the Scotch School, he wrote volumes on *The Cognitive Powers* and on *The Motive Powers* that had a psychological bearing, but gave no great evidence of originality."[7] The reader will note the omission of *The Emotions* from this list. Murphy does not mention him in his *Historical Introduction to Modern Psychology*, but Baldwin, a student of his, lists him among the philosophers with two columns of books and articles to his credit in the *Dictionary of Philosophy and Psychology*.

A man in this position, writing a rather lengthy and significant book on the emotions in the year 1880, could hardly have been overlooked by a fellow psychologist at Harvard. That he was known to James goes without saying in as much as his name occurs in the *Principles of Psychology*. It is surprising, too, that Titchener's historical review fails to mention this contribution to the James-Lange doctrine.

University of Iowa CHRISTIAN A. RUCKMICK

BOOK REVIEWS IN PSYCHOLOGICAL PERIODICALS

Book reviews in general literature obviously serve as a means of revealing content, indicating merit, and orienting the new publication in its special field as well as in its broader setting in the world of literature. This seemingly humble chore often becomes the recognized contribution to literature and literary criticism.

In psychological journals many book reviews are worthy of distinction not only because of literary quality but more so because of a special contribution to psychological literature. This paper is limited, however, to a study of several quantitative aspects concerning book reviews that appeared in journals listed in the directory of American Psychological periodicals during 1933.

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[4] H. C. Warren, *A History of Association Psychology*, 1921, 12.

[5] J. McK. Cattell, Early psychological laboratories, *Feelings and Emotions: The Wittenberg Symposium* (ed. by M. L. Reymert), 1928, 432.

[6] E. G. Boring, *A History of Experimental Psychology*, 1929, 517.

[7] W. B. Pillsbury, *The History of Psychology*, 1929, 235-236.

The results of this inquiry are shown in the tables that follow. There were 305 reviews on 254 different books, covering approximately 419.5 pages. Of the books printed in the English Language, 53% were not reviewed until two years or more after publication. The same trend but with more delay is revealed for books written in a foreign language.

TABLE I

NUMBER OF PAGES AND NUMBER OF REVIEWS IN 1933

Periodical	Approximate total number of pages	%	Number of reviews	%
Jour. of Abnormal and Social Psychol.	64.75	15.4	40	13.1
American Journal of Psychology....	107.00	25.5	133	43.6
Journal of Applied Psychology.....	20.25	4.8	14	4.6
Psychological Bulletin.............	109.50	26.1	67	22.0
*Psychological Clinic..............	11.50	2.7	7	2.3
Psychoanalytic Review............	40.50	9.7	28	9.2
Journal of Social Psychology........	9.75	2.3	4	1.3
Journal of General Psychology......	33.00	7.9	8	2.6
Pedagogical Seminary.............	23.25	5.6	4	1.3
Total.......................	419.50	100.0	305	100.0

* Last quarterly issue not available.

TABLE II

DATE OF REVIEW COMPARED WITH YEAR OF PUBLICATION OF BOOK

Date of review	English language (219 books)	Foreign language (35 books)
During year of publication	8.2%	0.0%
1 yr. after publication	36.1%	17.1%
2 yr. after publication	36.1%	22.9%
3 yr. after publication	15.1%	37.1%
4 yr. after publication	1.8%	5.7%
6 yr. after publication	0.0%	5.7%
7 yr. after publication	0.0%	2.9%
10 yr. after publication	0.0%	2.9%
12 yr. after publication	0.4%	5.7%
Not indicated	2.3%	0.0%
Total	100.0	100.0

Most books are reviewed only once. It is interesting to note that "The Physical Dimension of Consciousness," E. G. Boring, was reviewed four times and "Habits: Their Making and Unmaking," K. Dunlap, was reviewed six times. There are not many assiduous reviewers. About a maximum of one book review per month during the year is confined to only two reviewers.

TABLE III

NUMBER OF TIMES SAME BOOK IS REVIEWED

No. Times Reviewed	No. Books	%
1	218	85.8
2	25	9.8
3	9	3.6
4	1	.4
6	1	.4
Total	254	100.0

According to Table III, constant readers of only one journal may very likely miss many book reviews of particular interest. The Psychological Abstracts more or less serves to acquaint the reader with many of these books that have not appeared in reviews. But a more important fact revealed by this table is that there is a need for a critical evaluation of books by more than one reviewer.

TABLE IV

NUMBER OF BOOKS REVIEWED BY EACH REVIEWER

Number reviews	Number reviewers	%
1	95	60.9
2	30	19.2
3	16	10.3
4	4	2.6
5	5	3.2
6 to 10	4	2.6
10 plus	2	1.2
Total	156	100.0

TABLE V

STATUS OF REVIEWERS WITH RESPECT TO THE AMERICAN PSYCHOLOGICAL ASSOCIATION

	Number of reviews	%
Associates	102	33.5
Members	147	48.2
Non-members	48	15.7
Not identified	8	2.6
Total	305	100.0

This study shows in general that the available facilities for book reviews are not being used to the fullest extent. A number of obvious defects appear in the present system of book reviewing. These are briefly as follows:

(*1*) A small number of pages are given to book reviews in all but two journals; 51.6% of the space utilized for book reviews is confined to THE AMERICAN JOURNAL OF PSYCHOLOGY and the *Psychological Bulletin.*

(*2*) Most of the journals review few books. THE AMERICAN JOURNAL OF PSY-

CHOLOGY reviewed the largest number of books in 1933, amounting to 43.6% of the total.

(3) There is considerable delay in publication of reviews. Only 8.2% of the books published in the English language are reviewed during the year of publication and 46.6% of the reviews do not appear until two years after publication.

(4) Of the books reviewed 85.8% were reviewed but once.

(5) There are not many assiduous reviewers; 60.9% of them were credited with only one book.

(6) There is a question as to the professional competency of the reviewers. Only 42.2% of the books were reviewed by full members of the American Psychological Association, and over 15% by non-members.

The Psychological Corporation
New York, N.Y.
RICHARD S. SCHULTZ
HELEN PALLISTER

THE NORWEGIAN PSYCHOLOGICAL ASSOCIATION

The Norwegian Psychological Association was organized on April 4, 1934. The aims and objects of the Association are similar to those of the American Psychological Association.

The following officers were elected: Dr. Harold Schjelderup, President; Dr. Richard Eriksen, Vice President; Aase Gruda Skard, Secretary. The Association may be addressed care of Psychological Institute, University of Oslo, Norway.

Mooseheart Laboratory of Child Research
MARTIN L. REYMERT

THE CAMBRIDGE MEETING OF THE SOCIETY OF EXPERIMENTAL PSYCHOLOGISTS

The 30th annual meeting of experimental psychologists was held at Harvard University on April 3–4, 1934 under the chairmanship of Professor E. G. Boring. The meetings were attended by 18 members from 11 laboratories. After a short business session, reports of work in progress were received from the following universities and colleges: Brown, Clark, Columbia, Cornell, Harvard, Illinois, Pennsylvania, Princeton, Smith, Vassar, and Yale. Professors K. Lewin and M. Wertheimer were guests of the Society.

The next annual meeting will be held at Yale University on Thursday and Friday April 4–5, 1935, under the chairmanship of Professor W. R. Miles.

S. W. F.

MEMORY OF RACCOONS

In the spring of 1931 we conducted several experiments on raccoons in order to test their learning ability. We motivated them by depriving them of all food for a few days previous to the experiments. We then induced them to attempt to get out of a puzzle box with a lift latch by putting food outside of the box in sight of the animals. The first raccoon got out of the box the first time in 17 min., the second time in 4 min., the third time in 50 sec., the fourth time in 15 sec., and the fifth, sixth, and seventh times in 2 sec. each. We then considered the learning experiment completed. Two other raccoons showed similar learning curves.

A year later (1932) we repeated the experiments using the same raccoons as subjects, the same puzzle box, and the same hunger motive. In all three cases the raccoons got out of the puzzle box immediately, within 2 or 3 sec. We then repeated these experiments one year later (1933) with the same results, the raccoons getting out of the puzzle box immediately, within 2 or 3 sec. These results thus obtained under carefully controlled and carefully timed and carefully repeated conditions seem to point to the remarkable memory of raccoons for trial and error learning such as was represented in these experiments.

Mount Union College A. B. KITZMILLER

THE TWENTY-NINTH ANNUAL MEETING OF THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY

The twenty-ninth annual meeting of the Southern Society for Philosophy and Psychology was held in the Tutwiler Hotel, Birmingham, Alabama, March 30 and 31, 1934, under arrangements made by the Birmingham-Southern College.

All sessions but that on Saturday morning were divided into (A) Psychology, and (B) Philosophy. In the Friday morning Psychology session, with Joseph Peterson in the chair, the following papers were presented: A Method for the Investigation of Palmesthetic Sensitivity, by Beverly von Haller Gilmer (University of Virginia); The Diminished Seventh, Excitement, and Musical Relativity, by C. P. Heinlein (Florida State College for Women); An Example of the Apparent "Persistency" in the Auditory Mechanism, by R. C. Wingfield (University of Virginia); A Case of Anomalous Localization in a Skin Graft, by Lyle H. Lanier (Vanderbilt University); Visual Factors Influencing the Duration of Post-Rotational Nystagmus, by O. H. Mowrer (Princeton University); Flicker Relations within the Fovea, by Frank A. Geldard (University of Virginia); and Timing Devices in the Psychology Laboratory, by Knight Dunlap (Johns Hopkins University). The philosophers' program, under the chairmanship of Anna Forbes Liddell, was The Great Analogy in Idealism, by L. E. Loemker (Emory University); Logic and Functionalism, by P. A. Carmichael (Converse College); The Constancy of Human Values, by W. Preston Warren (Furman University); and The German Church and Liberalism, by Fritz Marti (Hollins College).

In the afternoon, H. M. Johnson being in the chair, Wayne Dennis (University of Virginia) made a preliminary report on An Experimental Study of Human Instincts. This was followed by a report on The Maturation and Learning of Serial Time and Muscular Coördination Involving Visual Stimuli, by Allen O. Gamble (Birmingham-Southern College); and one on Equivalent Stimulation During Learning, by Dael L. Wolfle (University of Mississippi). Due to the absence of certain members the program of papers was short, allowing more time for discussion. Four papers by the philosophers, under the chairmanship of Albert G. A. Balz, were presented and discussed: Measurement in Plato's Republic, by L. M. Hammond (University of Virginia); The Metaphysical Postulates of Mediaeval Aesthetics, by Katherine Gilbert (Duke University); A Basis for Modern Ethics, by Anna Forbes Liddell (Florida State College for Women); and Ethical Relativity, by Marjorie S. Harris (Randolph-Macon Woman's College).

The presidential address given after the banquet, Friday evening, by the President, Roy M. Dorcus, dealt with psychological research in colleges.

The papers presented in the joint session Saturday morning, presided over by

President Roy M. Dorcus, were The Undergraduate Curriculum in Psychology, by P. F. Finner (Florida State College for Women), chairman of the Committee on the Teaching of Psychology; Thought as Awareness and Thought as Behavior, by Marten ten Hoor (Tulane University); Modern Nerve-Physiology and Descartes' Doctrine of the Production of Bodily Movement, by H. M. Johnson (American University); St. Thomas and the Establishment of Modern Science, by Albert G. A. Balz (University of Virginia); The Problem of Psychic Reality, by H. C. Sanborn (Vanderbilt University); and The Secret of Aristotle, by George G. Leckie (University of Virginia).

In the final session of the psychologists' section on Saturday afternoon, under the chairmanship of J. E. Winter, H. W. Martin (Emory University) led out with a paper on The Development of Judgments of Traits through Practice and Correction. The other papers were The Selection of Students for Mental Nursing, by G. Wilson Shaffer (University of Baltimore); The Effects of Hypnotically Suggested Blindness, by Frank A. Pattie, Jr. (Rice Institute); The Relation between Pitch Discrimination and Accent in Modern Languages, by Emily S. Dexter (Agnes Scott College); Measuring Attitudes towards the Specific Social Rights of the Negro, by Euri Belle Bolton (Georgia State College for Women); The Inter-Relations of Certain Physiological Measurements and Aspects of Personality, by Katherine T. Omwake (Agnes Scott College); and The Use of the Drake Musical Memory Test for Awarding Scholarships, by Raleigh M. Drake (Wesleyan College). The philosophers devoted their final session to a round-table discussion of The Teaching of Philosophy in Colleges, under the chairmanship of Herbert C. Sanborn who is chairman of the Committee on the Teaching of Philosophy.

At the business meeting, following the Saturday morning session, the Committee on the Teaching of Philosophy was continued and was authorized "to prepare a digest and summary of the data collected, together with such recommendations as it may deem desirable, and, with the approval of the Council, that such a report be transmitted to the institutions concerned." After a discussion of the report submitted in mimeographed form by Professor P. F. Finner, Chairman of the Committee on the Teaching of Psychology, it was voted that the report of the chairman "be placed on file subject to the approval of the Committee as a whole, and that further release of the report to outside institutions or individuals be withheld until such release is approved by the Council." Certain members of the Committee had not seen the revised report since it was mimeographed. The officers elected were John E. Winter, University of West Virginia, President, and Albert G. A. Balz and Frank A. Pattie, Jr., members of the Council. The Society voted to accept the joint invitation by Vanderbilt University and George Peabody College for Teachers to meet next year in Nashville, Tennessee. The time of the meeting was set for April 19 and 20, 1935.

George Peabody College JOSEPH PETERSON

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

The Blind in School and Society: A Psychological Study. By THOMAS D. CUTSFORTH. New York, D. Appleton & Co., 1933. Pp. xix, 263.

"The purpose of this book is to acquaint the seeing with the blind and the blind with themselves." To accomplish this end the author presents a series of studies bearing upon various phases of the social psychology of blindness. He begins with two chapters designed to show how lack of vision affects early mental development and gives a striking case of retardation which he explains in terms of a restricted home environment resulting from mistaken ideas of a blind child's needs and possibilities. He finds two major difficulties in the social education of the blind, the danger of verbalism as a substitute for concrete experience and the tendency to egocentricity as a compensation for thwarting. These themes recur frequently throughout the book. This tendency to verbalism is constantly fostered in the literary type of education given the blind. "Never yet has the writer seen an object lesson taught in which there has not been an evident attempt on the teacher's part to read into the experience some sensory perceptions that do not exist for the pupil and at the same time utterly to neglect and discredit whatever reality may exist in the experience of the pupil" (p. 60).

Chapter IV, on the phantasy life of the blind, is a distinct contribution to analytic literature. The author's point of view is the phantasy life of the blind does not arise from their primary physical disability, but from the social relations that such a disability involves. Curtailment of self-expression results from the blind's living in the world of the seeing and is registered emotionally. Thus, the act of asking a stranger the name of an approaching street car is an admission of inferiority for which there must be compensation. Moreover, with loss of vision, communication depends largely on hearing and an uncanny perception of hidden meanings develops. Voices convey not only the meanings of the spoken word, but also those wordless forms of language which are made evident to the seeing by facial expression, gestures of hand, head, and body. Since social convention demands a greater control of visible expression than of voice modulation, the blind are keenly aware of their inability to grasp many subtle indications of meanings of the seeing. But the blind develop a keenness for the perception in the voice of the seeing of "a significance as clear and unmistakable as the eye perceives in the gesture of tapping the forehead." Emotional disturbances and compensations naturally arise from detections of duplicity. The seeing persons who practice duplicity "have learned to lie to the blind with words only. Their duplicity is only partially complete. They have not mastered their intonations and modulations as artfully as they have chosen their words. The phantasy life of the blind reveals that many violent phantasies arise largely from this unfortunate situation" (p. 104). Thus the environmental situation of the blind child compels him to become self-centered and this frequently results, according to the author, in speech and personality defects. The chapter continues with an extended discussion of specific causes for speech defect both physical and mental.

Chapter VI is devoted to various problems in the emotional life of the blind and gives the seeing an insight into the social attitudes of the blind which could come only from a thoughtful and keenly observant blind author. Chapter VII on the sex behavior of the blind covers the ordinary topics, but shows their unusual importance in view of the fact that a large percentage of the blind are segregated in residential schools. "Since it is impossible in most cases for the blind school child to talk, walk and work with the opposite sex, his social development takes place in the realm of phantasy, where there are no social codes or taboos" (p. 151). Chapter VIII on the aesthetic life of the blind is a closely reasoned study of the changes in meaningful wholes when the important substitution of touch for vision is overlaid by the personality changes incident to blindness.

From the beginning of the book pedagogical factors receive attention and frequent suggestions are given for a wiser treatment of blind children in the home, in the school, and in society. The pedagogical point of view becomes paramount in Chapter IX, where institutions for the blind are severely criticized. "The creative spirit in the education of the blind in America died with Samuel Gridley Howe in 1872. The educators since that time have satisfied themselves by imitating the material achievements of that pioneer, but none has dared to go beyond what he was able to achieve." They have imitated his material results solely by building bigger and better institutions, with bigger and better libraries; by devising better and more efficient punctiform systems of reading and mechanical devices for printing; and by making improvements in shops, shop methods, and school equipment; but "they have encumbered the entire system of education with stifling, sacrosanct concepts which are the traditional mores of institutional life throughout the country" (p. 199). In such an environment individuality and personality have little chance for development; it merely equips pupils with the concepts and social behavior appropriate to those who are to be social and economic dependents. The seeing world requires those weak and self-effacing attitudes which it at the same time despises in the blind. "First of all, they should live according to the way of God, which happens to be the way of each particular institution. They should practice the Christian virtues, especially stressing obedience, respect for superiors, turning the other cheek, more blessed to give than to receive, honesty is the best policy, early to bed, and it pays to advertise. They must always appear cheerful and pleasant to others and on all occasions should appear happy" (p. 207). The final chapter is devoted to striking case studies of two blind boys in a college community.

Cutforth's book is an important contribution to our literature upon the blind. Although appearing under the guise of science it is written with the zeal of the reformer and the extravagance of the iconoclast. Pedantic with *Gestalt* and psychoanalytic terminology—with a glossary for the uninformed lay-reader—and dogmatic with the implicit assumption that a blind man must know the truth about the blind (though it is not always clear how a person not born blind should presume to speak for this small and highly specialized group) the author has produced a book that will be widely read and hotly discussed. His blind readers take violent exception to his statement "under the conditions imposed upon them by society, not one of these sixty thousand human beings has developed, or can possibly develop, a strictly normal personality" (p. 2), and resent many generalizations which do not accord with their own experience. Seeing persons working for the blind wonder whether the book will do more harm than good to their cause; they question whether the general public is prepared for such frank statements on many questions and fear that the violence of his strictures may alienate the very people in institutions who might profit by a consideration of the issues he raises. There is no doubt, however, that the author has written many true statements, some based upon experimentation and some rising from keen observation and thoughtful consideration.

Mt. Holyoke College SAMUEL P. HAYES

Behind the Door of Delusion. By INMATE WARD 8. New York, Macmillan Co., 1932. Pp. xiii, 325.

This book, describing an insane hospital, is written in the fluent, witty style of a professional newspaper man. Though lacking the dramatic intensity of Clifford Beer's great book, it is characterized by reportorial conscientiousness, avoidance of sensationalism, and an effort to be fair to everyone, including those in authority. It paints a haunting picture of the dreary monotony of the well-regulated institution. Unlike most insane writers, the author is fairly objective. Without attempting to deny the mental malady (dipsomania) which led him to consent to commitment, and without concealing the other traits which make him a sorry character, he nevertheless does not intrude himself very much upon the reader. He is more concerned in portraying the environment, his fellow inmates, and some of the attendants and doctors he has known both in the institution and outside of it. As might be expected, nearly every chapter contains examples of the author's fallacious reasoning, warped values, and questionable ethical outlook; but after all, we do not go to the insane for our logic and our ethics, nor does the author expect us to. He merely contends, perhaps rightly, that as one of the insane, living the life they live, "the recipient of their confidences," seeing them "when they are not on guard," he knows the fellow-inmates of his ward better than the physicians and attendants who "must always look down on the mentally afflicted from a height." He implies that the very position of exercising disciplinary authority necessarily precludes a profound and thorough acquaintance with wary ones under this authority. This patient has spent his entire time so far in the receiving ward, and every man admitted to the institution has therefore come under his trained reporter's eye.

Delusions of grandeur, delusions of persecution, manic-depressive states, religious manias, epilepsy, post-encephalitic conditions, dementias, and other familiar types are portrayed in brief vignettes in non-technical language; but it is the author's comments on "the sane insane" that are most interesting. He divides them into two classes, the criminals who feign insanity to escape imprisonment and the helpless dependents who are put away by heartless relatives. In some places, it requires only the signature of two or more doctors to procure a commitment; and once committed, a man rarely secures his release unless someone outside the institution will act in his behalf. He can not send out mail or even secure writing materials, if the authorities forbid it. He can not see visitors without official consent. He can not even make a telephone call. The author tells an amusing tale of a young farmer whose wife telephoned the county officials that her husband had suddenly gone insane. He was brought before a county lunacy board composed of three general practitioners. On the wife's testimony, the board committed him to the asylum and he was locked up before nightfall. The next day, the wife was committed on the complaint of several neighbors. "A thorough examination brought out the fact that those physicians had committed a sane man on the complaint of an insane wife, and they had both man and woman before them at the husband's hearing." The hospital authorities soon released this man; but the author is convinced that there are many aged people, paralytics, and sick people who are not really insane but are confined for life against their wills because relatives do not want them and they are incapable of self-support. In another chapter, entitled "The Sterilization Spectre," the author reminds his readers that according to the records of Salem witchcraft "among the witnesses in several cases were a 'leech and a minister.' " His arguments against sterilization are of course inconclusive when weighed against the well-known arguments in favor of it; but he shows how some of the insane feel.

Though the author pokes fun at "Dr. Pomposity Nincompoop" and his ilk, he has no grudge against doctors in general, and his attitude toward the hospital attendants is one of frank admiration for the courage, tact, and resourcefulness of most of them—yes, and even for their frequently demonstrated athletic prowess. Contrary to general opinion among laymen, he believes that really brutal attendants are comparatively few. He reminds us that even in the violent wards no attendant is allowed to carry any sort of weapon. All attendants, he claims, must be adroit in ju-jitsu or something like it, and must not hesitate to use their hands when necessary to stop a fight, restore order, or secure obedience to some important command; but the author states that he has never seen a patient injured beyond a few minor cuts and bruises. He approves of the straight jacket, contending that it is not an instrument of torture, as frequently supposed, but a humane device for protecting an excited patient from injuring himself. He approves of the barred windows for himself as well as others. He points out, however, that many factors in institutional routine are well calculated to drive a sane man insane, and that some persons who are fairly sane when first committed eventually crack under the strain. He pleads especially for those who are neither quite normal nor really insane. He says, "They should not be locked in with lunatics and subjected to the same rigorous discipline and unending, mind-destroying repression." Under this discipline, many mild cases eventually become violent.

The author certainly offers no encouragement to sane criminals who feign insanity to escape prison. Hospital attendants, physicians, and patients are aware of the imposture. The criminals are despised and ostracized, and are as closely confined and rigorously disciplined as though they had gone to prison where they belong; but the imposture is entirely too successful with their friends and families, who after they have once been pronounced insane, never again believe them rational and are quick to have them recommitted. The end is likely to be life imprisonment in the hospital, often with real insanity eventually developed.

There is no index.

West Springfield, Mass. MABEL F. MARTIN

The Art of Educational Research: Its Problems and Procedures. By HAROLD H. ABELSON. Yonkers-on-Hudson, N.Y., World Book Co., 1933. Pp. xi, 332.

This book is the most recent of about a dozen that have appeared on methods of educational research, and it will, in the opinion of the reviewer, prove the most satisfactory.

Three of the chapters of the book give a concise summary of statistical principles, including an excellent discussion of the problems that result from the general absence, in educational measurements, of true zeros and equality of measuring units. The first two chapters of the book discuss the place of research in education and the question of how to find and how to evaluate research problems. How to report research is discussed only in the brief final chapter. The major portion of the book deals with the techniques of planning and conducting educational research that will be sound and significant. One contribution of this discussion is its re-organization of research techniques under the five heads of integrative, descriptive, causal, evaluative, and constructive methods. A few changes in the more detailed classification of research methods would seem to have been advisable, such as the inclusion of the case study method under the "inferred control causal method" rather than under descriptive method, and such as giving to testing methods the more distinctive place which they would seem to merit; but as a whole the material is very clearly and thoughtfully outlined.

Throughout, Abelson's book is well organized, clear, and concise. No quotations are made from other authors, although the author draws heavily upon them, particularly upon McCall. Relatively little concrete illustrative material is included in the book, but what is included is used very effectively. This book has made progress towards minimizing merely common sense advice on methods of work, and toward emphasizing, instead, the more technically derived methodological principles. The principal suggestion of the reviewer, however, is that the author should sometime revise this book with both of these tendencies more rigorously developed. In the latter direction, for instance, such minor points as the distinction between chance and systematic errors should be clearly brought out; the whole matter of factor analysis should be given a much more adequate treatment; and in the discussion of studies of problem children, the relatively particular methods of such workers as Samuel Hartwell should be described. There are really quite a few of such general methodological principles which, though admittedly not as technical in their derivation as statistical formulae, nevertheless have been made possible only through the accumulated experience of years of actual research. Such material should furnish the main content for such a book as this.

Psychologists may find this book of considerable value. No corresponding work on psychological research is available, and perhaps there is no occasion for one, since with a few changes of illustrative material this book applies directly to the problems of psychological research.

University of Arkansas ROBERT LEEPER

Le choc des patriotismes: Les sentiments collectifs et la morale entre nations. By EDMOND PRIVAT. Paris, Félix Alcan, 1931. Pp. vii, 179.

The author of this book—not a psychologist—has made contributions to recent political history, and has had considerable first hand experience at observing conflicts among national sentiments, attitudes, and points of view in the League of Nations at Geneva, with which he has been connected. The book is written in a sententious style, abounding in epigrammatic statements. It cannot be called a scientific work.

Pointing out that all centuries have had their conflicts, this Swiss observer holds that the conflict of the present century is between separatism and federation. Since the economic and political aspects of this conflict have been abundantly investigated, Privat proposes to treat certain different aspects which also cannot be neglected—traditions, customs, fixed ideas, and collective emotions. It has been supposed that it was sufficient to "change the heart of man to bring about definite peace," but history proves otherwise. One must still enlighten the people and adapt their institutions; otherwise their heroic virtues only serve collective passions. The habits acquired carry away the heart, and sentiments attach themselves to institutions (p. vii). Privat points out, as others have done, that the morality of nations is low as compared with that of individuals, this being due chiefly to the fact that the ordinary citizen will not accept criticism of his own country, and that emotional appeals to groups is so effective against rational understanding. National patriotism comes to be the supreme religion because one may even be required to sacrifice his life for it; that for which one pays the most becomes most appreciated.

Privat gives a good account of the aims of the Nationalist movement in India as one of the great experiments now going on in national groups. The Gandhi movement is attempting to raise India to an entirely new national morality; there is developing a patriotism without anything of the warlike spirit. A non-resistance by people willing to make any sort of sacrifice, even to their own lives, is appearing. Gandhi is preoccupied with the soul of his country and is trying to make India heroic but non-military. The leaders of this movement are trying not to humiliate the adversary but to force acceptance of equality. "The Russian revolution attracts attention by its technical experiment; the revolution in India, by its moral experiment. Indeed there is novelty of technique in both. What is new in U.S.S.R. is the end; in India it is the means" (p. 85).

Privat contends that the trouble with the League of Nations is that it lacks the sentimental background and the mysticism which inspire the loyalty found in real national groups and in religions. He rightly recognizes the great rôle of the language factor in such irrational organizations, and sees now in Esperanto the intensive cultivation of the prerequisite basis for the development of trans-national community feeling. It appears somewhat doubtful to the reviewer whether the masses in different national groups can be sufficiently influenced by such an artificially created language to respond with the 'natural' sentiments which bind them to their church and their national groups.

The book, even though its actual contributions to social psychology as a science are small, offers useful data and suggestions to psychologists. It is interesting to note that the erroneous concepts of 'imitation' and 'suggestion,' so frequently found in the old social psychology books, are absent in this treatment. There is no index.

J. P.

Sinn and Unsinn. By WALTER BLUMENFELD. Munich, Ernst Reinhard, 1932. Pp. 110.

During the last two decades psychologists have spent much time discussing the "meaning of meaning," but with little reference to the correlative concept of the meaningless, or the domain of non-meaning. Yet essential formal characteristics of concepts or objects may remain unknown through a failure to delineate properly their opposites and contraries. Sense can only be fully evaluated in the light of a clear conception of the nature of nonsense. With this fact in mind Dr. Blumenfeld offers a compact yet comprehensive analysis of meaning and meaninglessness, sense and nonsense, from the varied standpoints of philology, logic, metaphysics, and psychology, the net result being a fairly convincing proof of the relativity of the terms and of their dependence on what might be called the functional context in which meaning or its correlative absence or denial appear. Thus the meaning of a term or series may be present or absent according to the circle of conceptional relationships within which the thinker moves at the moment. An item of consciousness may possess semantic and lack purposive or logical meaning, and vice versa. The author discusses five types of meaning and correlative deprivation, viz., semantic, purposive or teleological, configurational (*Eidischer Sinn*), logical, and motivational, with their varying relational criteria from the point of view of formal logic. The meaningless is characterized by the absence, inversion, or perverted use of relationships. It is interesting how according to the author's premises, the 'absolute' of certain idealistic philosophies, as well as solipsism, must inevitably culminate in the acceptance of the meaningless or irrational elements intuitively advanced by mystics from a psychological standpoint. The production of pure or relatively pure nonsense, the deliberate achievement of the meaningless, must be regarded as a mark of high intelligence since it involves the conscious perception of the meanings to be avoided, and this fact may be used as the basis for a new type of intelligence test. The nature of mental processes in the insane, and the course of mental decay, are illuminated by analysis of the types of meaningless activity and thought displayed in such cases. Most of the book is devoted to verities of meaning but a short section discussing the laws and processes governing the apprehending and production of meaning and nonsense will be of particular interest to the psychologist. A still briefer section on the significance of the meaningless in the daily life of the community and especially in relation to laughter, where a whole category of the ludicrous is based on the artistic use of nonsense, completes this remarkably lucid presentation of a difficult subject.

University of Cincinnati CHARLES M. DISERENS

Psychology Applied. By GEORGE W. CRANE. Chicago, Northwestern Univ. Press, 1932. Pp. xii, 586.

If a good textbook is one which is interesting and readable, which requires no mental labor, and which will appeal to second and third rate undergraduate minds, this is an excellent book. According to the publisher's statements, within six months it had been adopted in over 50 colleges and universities. Commercially it is a success.

Nor is this popular approval difficult to explain. The author has used skillfully the tricks of the applied psychology he is presenting. Snappy headings, concrete and vivid illustrations, amusing anecdotes, allusions to sex, omission of everything scholarly (critical evaluations of evidence, problems of method, alternative interpretations)—and the feat is accomplished, Applied psychology is synonymous with common sense plus clever salesmanship. It is a quasi-scientific description of how to "put things over," how to reach the top in a "go-getter" world. The volume might well be dedicated to the spirit of Glenn Frank's approvingly quoted statement: "I suspect that the most important man in the world is the good salesman."

Applied psychology is in constant need of rescue. Unceasing effort is required to keep it above the level of off-hand opinion, charlatanism, preaching, and "success" literature. If it is a legitimate branch of academic psychology it must in some degree be critical, systematic, scientific in spirit, insistent on rigid methods and on thorough evaluation of evidence. Measured by these standards, Crane's book is sadly lacking. It gives only a superficial, incomplete, largely non-psychological, and often lop-sided picture of the topics treated.

To be sure the volume contains many fragments that are amusing, useful, and even true. Interesting thought-provoking discussions occur; for example, on personality differences between the sexes, on the emotional effects of music, on mental development in children, on emotional maladjustments. Likewise in the treatment of advertising and selling, learning, tests, public speaking, education—in fact throughout—one may find sound statements, perhaps whole sections. But the good and poor, the fact and opinion, are so intermingled, and all with sufficient lack of organization and perspective, that one must be very optimistic to believe that students will profit intellectually from their study of Crane's *Psychology Applied.*

University of Chicago ARTHUR W. KORNHAUSER

Intelligence: Its Manifestations and Measurement. By PAUL L. BOYNTON. New York, D. Appleton & Co., 1933. Pp. xi, 466.

This textbook should make a place for itself because of its excellence for training students in the cautions to be observed when interpreting test results. It does this together with offering careful directions for giving tests and an adequate background of the history of intelligence tests and their applications to school and social problems. By taking at times a frankly critical attitude and selecting for discussion studies which bear upon practical problems the author cultivates good sense in the use of test measurements without losing an appreciation of their value. The book provides a training guide rather than a complete compendium. It thus makes a distinct contribution to the series of excellent books on mental tests.

In his discussion of the nature of intelligence, Boynton adopts a social point of view. Accepting social inadequacy as a criterion for feeble-mindedness, he uses "social sufficiency" as the criterion for judging all grades of intelligence. The concept is amplified, however, to mean the inherited capacity of an individual manifested through ability not only to adapt himself to his social environment, but also to reconstruct that environment to meet the fundamental needs of himself and his group. This social criterion tends to change the emphasis from an intelligence-test record to its interpretation in the light of the individual's whole personal history. The author treats in a practical way the causes and control of mental abnormalities, the distinction between deficiency and mental diseases, legal aspects of diagnosis, and provision for gifted children.

A feature which will stimulate much interest among students is the author's frequent use of cases observed and studies made during his own rich experience in dealing with school children, college students, delinquents, and inmates of institutions, while formerly in the psychology department at the University of Kentucky and now in that of educational psychology at Peabody. The text also includes elementary statistical information necessary for the introductory student to understand test data, including simple correlation and the use of the author's correlation chart.

The last hundred pages are a summary of the applications of intelligence measurements to educational problems, racial comparisons, to defectives and delinquents, and to occupational problems. The author does well to recognize that the present tests only indirectly and quite inadequately measure the capacity for social sufficiency which, he believes, should be the aim of capacity diagnosis.

University of Kentucky J. B. MINER

The Psychology of Laughter. A Study in Social Adaptation. By RALPH PIDDINGTON. London, Figurehead, 1933. Pp. 227.

The author is more to be congratulated for the completeness with which he reviews and criticizes the theories of other psychologists and philosophers (from Plato to Kimmins) than for the cogency of his arguments in support of his own theory.

When he contends that "we must seek a *biological* rather than sociological explanation for elementary laughter" (p. 73) because laughter has been observed in apes, we wonder why he should ignore recent reports of the rather highly developed social character of apes, until we learn from a footnote (p. 78) that *social* life "differs from a merely gregarious existence in being associated with culture (in the anthropological sense)." Certainly most biologists apply the word *social* in a less restricted sense.

The author finds his so-called biological explanation in the fact that laughter notifies the mother of the well-being of her infant, just as crying tells her that something is wrong with the baby. "Laughter expresses, maintains, and communicates a mood in which there is no need felt for the organism to make any further adjustment to its environment beyond the one at the moment existing." This is made to explain also the laughter of somewhat older children, which occurs chiefly in those play situations that exercise the 'instinctive' activities of pugnacity and escape—activities that sometimes change suddenly from play to a dangerous struggle. Laughter in such situations show the playmate that the struggle is still "all in fun" and that he may safely continue it. Cessation of laughter shows him that the play mood is gone and he had better stop.

It is with great difficulty that the author bridges the gap between childish laughter in such situations and the laughter of adults at "the ludicrous." He tries to show that the ludicrous, whether pun, practical joke, or comedy, always involves conflicting social evaluations. The discovery that in a given situation two or more equally applicable social evaluations conflict irreconcilably might seem to demand immediate reorganization of the whole scheme of social values; but to laugh is to deny the seriousness of the inconsistency. Thus laughter at the ludicrous serves to preserve social evaluations by expressing complete satisfaction with things as they are. The author does not tell us how his theory accounts for the bitter laugh of disillusionment.

West Springfield, Mass. M. F. MARTIN

A Series of Experiments on the Learning of French Vocabulary. By LOUISE C. SEIBERT. Johns Hopkins University Stud. in Educ., No. 18. Baltimore, Johns Hopkins Press, 1932. Pp. xi, 106.

The problems and methods of this study seem to have grown out of the practical needs of a teacher of French, unfamiliar at first with certain psychological techniques which she finally discovered and improved. There seems to have been good coöperation on the part of 81 college girls who served as subjects. The work covers only the learning of vocabulary. The author holds, rightly, that results obtained by experiments on nonsense material can hardly be taken over uncritically by teachers of foreign languages.

Four hundred words, suitable in comparative difficulty and origin were selected. From these words, series of 12 words each were made up and put with their respective English equivalents. The learning was always "active," *i.e.* the reproduction (in writing) of the foreign equivalents was always required when the English words were written on the board by *E*. Recalls occurred generally after periods of 50 min., 2, 10, and 40 days; and relearning was required after each recall test, always taking place in the same manner as in the original learning.

Detailed results are given in the tables in the form of comparative means and their standard deviations. We can here indicate only their general trends: studying aloud was found to be more efficient than studying silently; words studied in associated pairs were better learned than when studied in sentences (context); the whole method of studying associated pairs was better than the part method (but the series were short); distributed practice over 10 days was more effective than equal practice concentrated at one sitting; the recitation method of learning was superior to the reading method; the quick learners retained better than the slow learners; those *Ss* that were better in immediate (50 min.) recall were also better in delayed recall; etc. One might quibble over certain details and interpretations, but space forbids. The book is well documented, and a bibliography of 5 pages is added. Unfortunately dates are frequently omitted in references, and no general form is adhered to, even in the bibliography.

J. P.

Dementia Praecox: A Psychological Study. By HARRIET BABCOCK. New York, Science Press, 1933. Pp. 167.

Ultimately the test data from studies such as this should be of distinct value to both psychology and medicine. It is unfortunate, however, that so much that is indefinite, controversial, or completely disproved in psychology should be advanced as verity to those not intimately associated with the science.

One hundred twenty-four "coöperative" praecox subjects made higher relative scores (compared with 216 non-psychotic subjects) on vocabulary than they did on learning tests and on a composite of tests other than vocabulary. That these findings are similar to those reported by J. H. McFadden for 507 feebleminded (*Ment. Meas. Monog.*, 1931, (no. 7), 1-85) is either overlooked or ignored by the author.

The author states that native mental capacity is isolated by intelligence tests (p. 14). "Practical work shows that vocabulary is a better measure of original capacity than school history" (p. 22). Claim is made to having proved previously that the difference between mental ages on vocabulary and on tests other than vocabulary (listed above) is a measure of mental efficiency (p. 15). "The discrepancy between untimed responses which require old associations of a kind which correlate highly with intellectual level [avowedly, vocabulary] and responses which emphasize time and the fixation of new data" (tests other than vocabulary) is given as proof of what the author once claimed was "obvious" (cf. her study in *Arch. Psychol.*, 18, 1930, (no. 117), 58).

While appreciating the difficulties encountered in such a study, the reviewer sees no justification for the finality of opinion expressed. Emphatic assertion as to what tests measure is not acceptable proof.

Studies in this field would do well to give more attention to factors too frequently ignored. Tests used should have adequate accuracy at the extremes, a patent weakness of most of the measures now used. Any study of dementia praecox should safeguard diagnosis more adequately against the inclusion of other types of dementia as well as against the inclusion of the non-psychotic. In so far as possible, mental ability or abilities, age, and possibly other factors should be held constant. Finally, the basing of any evaluation of pre-psychotic ability on observation made after the onset of the psychosis is, to say the least, a hazardous undertaking.

University of Louisville MILTON B. JENSEN

Personality, Many in One: An Essay in Individual Psychology. By JAMES W. BRIDGES. Stratford Co., 1932. Pp. 215.

The book begins with the following definition: "Personality is that which characterizes, constitutes and distinguishes a human being. It is the totality of all physical and mental characteristic, traits and processes, and their interrelationships. . . . The psychology of personality is therefore the whole of human psychology." What a task to set one's self in two hundred pages!

The author bravely sets out by enumerating the various classifications of personality that have been made on the basis of physique, intellect, temperament, and character, doing it all in such an extremely impartial and abstract manner that the reader begins to wonder, at about page 119, what it is all about. Then, after a long chapter on the "total personality" in which various tendencies toward disintegration are discussed, he takes up the various psychological types as expounded by William James (tender-minded and tough-minded), by Furneaux Jordon (reflective, more-impassioned and active, less-impassioned), by Carl G. Jung (extravert and introvert), by Eduard Spranger (theoretic, economic, aesthetic, social, political, and religious), by the ordinary psychiatrist (manic-depressive, schizophrenic, hysterical, paranoid, psychasthenic, neurasthenic, and epileptic), by Theophrastus (objective observers and subjective depictors), and by various other individuals (classic and romantic types, sensitive and active types, conservative and radical types, egoists and altruists, masculine and feminine types, and, finally, snobs).

The final chapter points out the importance of education in the development of personality and discusses three types as determined by development; the undeveloped, the overcompensated, and the unfolded types.

Northwestern University JOHN J. B. MORGAN

Sweeping the Cobwebs. By LILLIEN MARTIN and CLARE DE GRACKY. New York, Macmillan Co., 1933. Pp. viii, 181.

This book is a sequel to *Salvaging Old Age,* 1930, by the same authors. The book was written primarily to answer the "many hundreds of letters asking for further elucidation of the work done" in the Old Age Center, as described in the former book. The number of letters "is increasing rather than decreasing" and the book is to relieve the burden of responding individually to these letters in an adequate manner. The senior author is thus involved in her interesting and useful work at the age of eighty years.

The authors relate the procedures followed in receptions to the Old Age Center, the getting of life histories—information chiefly regarding the individual's motivations and main lines of activity, tendencies to fall into fixed systems of acting and thinking, reaction patterns acquired and resistances to modification—the purpose being to reveal to the consulter "what he has that is worthy of preservation, what needs correction, and what must be actually banished to bring about a more satisfactory existence for himself" (p. 10). These data, added to those obtained from physical examination and a brief mental test, to determine "how his mind is functioning," supply the basis for the making of daily schedules for different sorts of training, for rest periods and for recreation. The schedules when filled out are carefully checked as to the carefulness of execution. Results obtained constantly serve as basis for further helpful analyses and assignments in the process of "sweeping the cobwebs." Details like budgeting and methods of personal finance; getting the proper tempo for efficiency; maintaining calmness of emotions, progressive attitudes in recreation, personal habits, and contacts with social life are not forgotten in the organization of the consulter's life toward individual freedom and happiness. Such a program would evidently be valuable to most people much earlier in life.

The author's work goes beyond the programs thus briefly indicated: it extends to effecting provisions by society for the better care of the aged. The authors are thus becoming pioneers in a unique sort of social work of high value, that of increasing the worth and the happiness of human life through old age. There is no index.

J. P.

La determinazione del presente psichicho. By RENATA CALABRESI. Firenze, R. Bemporad & Figlio, 1930. Pp. xii, 188.

The nature of the psychological present representing as it does the minimal unit of lived experience, interests psychologists and philosophers in the same way that physical ultimates interest the physicist or mathematician. The present monograph, inspired by Bonaventura's studies of the psychology of time, shows a thorough acquaintance with previous philosophical speculation and experimental studies in this field. In her summaries and conclusions the author displays great ability in drawing logical distinctions and in criticizing the methodology of her predecessors. Learning activity is selected as the best psychic material available because of the simplicity attainable and the possibility of repetition. One set of experiments was carried out by means of Wundt's tachistoscope and is closely related to the work of Wundt and his pupils. The object of these experiments was the determination of the content of the psychological present, the results corresponding closely with those of previous studies of attention, the extension of consciousness, and learning through simultaneous presentation. Another set of experiments involved the use of a new double tachistoscope designed by Bonaventura, and show the limits of the temporal extension in which the results of successive presentations assume for introspection the form of a unified experience. The psychological present is a concrete experience, with limits varying according to the subject and the nature of the content of his experience. In general it may be said, that successive presentations between the limits of 800 and 1000σ are synthesized in cognition and attention to form a single moment of experience. The experiments point to a temporal rhythm inherent in cognitive function, a rhythm in which the unity of the conscious act is identified with the temporal extension of the psychologically actual and the duration which is directly sensed. The distinction between reality and unreality is illuminated by this comprehensive study, which merits equal praise for experimental skill and philosophical suggestiveness.

University of Cincinnati — CHARLES M. DISERENS

Nervous Breakdown: Its Cause and Cure. By W. B. WOLFE. New York, Farrar & Rinehart, 1933. Pp. xv, 240.

This interesting book does not attempt to make any new contributions either to scientific psychology or to technical psychiatry. It aims primarily to help practicing physicians, the patient's family and friends, and above all, the patient himself, to understand the unconscious mental mechanisms concerned in a nervous breakdown. The point of view is that of Adlerian individual psychology, but the illustrations and detailed explanations are largely new, based on the author's own broad clinical experience, many patients having consented to permit the use of their own life stories, disguised to insure anonymity. The author endeavors to convince every neurotic that the intelligent thing for him to do is to consult a psychiatrist who will help him discover the meaning of his symptoms and substitute wholesome attainable goals for the neurotic goals that must be replaced. Illustrative case histories in this book include a college boy who attempted suicide when threatened with failure, a young woman whose ignorance of sex led to nervous breakdown after marriage, a passé vamp whose life-long pride had been her sex appeal and power to enslave men and who found loss of her youthful beauty unendurable, and numerous other examples of unfortunate individuals whose self-esteem depended upon some superficial fancied superiority beyond their own control.

The author contends that any individual whose self-esteem depends on wealth, personal beauty, intellectual preëminence, or any other real or fancied superiority beyond his own control is liable to lose "face," and in consequence, to suffer a nervous breakdown. The only secure basis of self-esteem is service to one's fellowmen. The last two chapters are devoted to constructive advice to patients on the art of widening their horizons and developing creative self-expression.

West Springfield, Mass. — M. F. MARTIN

Die seelische Eingliederung in die Gesellschaft. By W. STEINBERG. München, Ernst Reinhardt, 1933. Pp. 126.

Though rich in detail, the theme of the book is clear and simple. Man's social responses are expressions of the inmost core of his being. He *demands* sociality and passionately opens his mind and heart to social influences. To say that man's mind and heart are "molded by social influences" is true enough, but this is not the compulsion which the sculptor exercises upon an inert block of matter; rather, it is the expression of social forces which are demanded, assimilated, and used by the inner self. We speak of the constraint which custom and tabu exercise upon the savage, forgetting that it is because he strives to immerse himself in the group ethos that these customs and tabus are supreme. They are, to be sure, coercive, but the coercion is self-imposed. In modern Europe a doctrine which (like Locke's) has man conform to social usage for the sake of the good opinion of his fellows overlooks the fact that such a desire can exist only because a deeper need for fellowship is already there.

Individual men could never be made into cohesive communities by virtue of pressure acting upon each man *from without.*

The book is didactic and expository. The author aims to "show" how these things are. That his hypotheses permit of, or call for, testing seems not to occur to him. At any rate he betrays no familiarity with the rich source material from child psychology and psychiatry which could be used as a criterion for evaluation of his thesis. For example, though on almost every page one is reminded of the pertinence of Piaget's data on the moral judgment of the child and of psychoanalytic material on the "identification" of the child with the parent, he seems unaware that such studies exist. Greater concern for evidence might contribute more solid and convincing substance to the author's subtle, well-wrought, and highly suggestive doctrine.

Columbia University GARDNER MURPHY

Consciousness: Brain-Child. By PERCY A. CAMPBELL. East Cleveland, Ohio, 1933. Pp. 109.

This slender, theoretical volume, copyrighted and published by the author, reiterates the familiar mechanistic dogma that consciousness is a by-product of brain functioning. Many a discarded theory is dug up to be confuted again, many an old battle refought, many a man-of-straw set up to be knocked down. New metaphors dress up old theories with a semblance of novelty which is enhanced by five neologisms—proud coinages of the author to express his amazingly naïve psychophysical assumptions: (1) *ideopulse,* "the pulse through the brain constituting an idea;" (2) *ideocircuit,* "the circuit through the brain made use of by this activity-pulse;" (3) *ideoprocess,* "the process constituting one item or term only of an idea;" (4) *ideopattern,* "the pattern or brain-trace made use of, or underlying, this process;" and (5) *ideorapport,* "the condition of accordance, correspondence or sympathetic relation existing in any one of these units—with respect to something in the world with which, for the purposes of consciousness, it forms some sort of a liaison. The word *idea* is here used as a generic one signifying any type of consciousness whatsoever; and an idea is here freely considered to be a thing of the brain, wholly cerebral." Precise anatomical and physiological details of neurology are lacking, but some of the broad generalizations which often appear in elementary textbooks of psychology are even more broadly and more dogmatically stated. In so far as the author draws any distinction between obvious facts of introspection and questionable neurological hypotheses, it is to prove the facts by copious arguments and by abundant quotations from philosophers, poets, and psychologists of an earlier generation, and then to state neurological theories as though they were unquestioned axioms.

West Springfield, Mass. M. F. MARTIN

An Experimental Study of Certain Factors Affecting Transfer in Arithmetic. By JAMES ROBERT OVERMAN. Baltimore, Warwick & York, 1931. Pp. vii, 235.

Transfer of training is defined in this study in Ruger's terms "to include the effect of any given experience on any subsequent one" (p. 53). Employing this definition, Overman selected 112 paired "quartets" of second-grade children and studied transfer in connection with the learning of simple types of addition and subtraction. His findings indicate that transfer was an actuality in the case of 99% of the pupils, that the transfer was almost 100% in many cases, and that the amount of transfer varied with the intelligence of the learner, with the instructional technique employed, and with the closeness in kind of the new to the old material, but not with the sex of the learner.

The special merits of the study lie, not so much in the findings summarized above (though they have large importance for the direction of education, if not for the theory of transfer), as in the extraordinary care with which the study was organized and prosecuted, the quality of the investigator's insight into his problem, the detail of his report, the checking of experimental errors, the testing of hypotheses and assumptions, and the penetrating and intelligent interpretation of the data.

Duke University WILLIAM A. BROWNELL

How We Think: A Restatement of the Relation of Reflective Thinking to the Educative Process. By JOHN DEWEY. 2d ed., enlarged. New York, D. C. Heath & Co., 1933. Pp. vi, 301.

The present book retains the basic ideas set forth in the first edition. A considerable amount of illustrative material has, however, been added, and the position of several chapters has been altered. Part I, which is devoted to the problem of training thought, remains essentially unchanged. The most extensive revisions occur in Part II, which aims to give a theoretical account of the operations of the mental processes as they actually take place in the acquisition of knowledge, in contrast to the older formalistic conceptions. Part III, which deals with the practical problem of training thought, has received a somewhat fuller treatment than in the former edition, although no changes of fundamental importance occur in this section. The present volume contains many valuable pedagogical suggestions drawn from the author's wide experience. The rather free use of a terminology adapted to the level of the average grade teacher (*i.e.* non-professional psychologist) frequently results, however, in mere verbalization and psychological inaccuracy.

Vanderbilt University EUGENE G. BUGG

Phyloanalysis. By WILLIAM GALT. London, Kegan Paul, Trench, Trubner & Co., 1933. Pp. 151.

Phyloanalysis is here presented as something which cannot be expressed in words; it is thalamic rather than cerebro-cortical; it is of the realm of feeling and emotion rather than of concept. There is much about experimental work and laboratory techniques, but the most commonplace principles of scientific method and of laboratory control in experimentation are ignored. Familiar psychological terms appear in a bewilderingly loose usage (feeling-sensation, social image, affective social image, subjective affect-tone, subjective contrast-tone, sensation of anger, feeling of anger, sensation of love, sensation of hate, emotions of love and of hate, feeling-perception). All this vagueness seems to Trigant Burrow, founder of phyloanalysis and author of a preface to this book, exactly as it should be. But to the reviewer the whole thing seems to be no more than an adaptation of psychoanalysis for group work, with most of the scientific crudities of psychoanalysis retained. Much of what is claimed for phyloanalysis has long been the common property of psychology and general psychotherapeutics. Such deliberate efforts to be vague and esoteric are far more likely to confuse and obfuscate than to expand and clarify human knowledge.

University of Oregon EDMUND S. CONKLIN

The Big Cage. By CLYDE BEATTY with EDWARD ANTHONY. New York, Century Co., 1933. Pp. v, 409.

Mr. Beatty, the well-known animal trainer, relates in this book his personal experiences and the methods used by him in training the "big cats" (lions and tigers, principally) and other ferocious animals for their performances in the circus arena. The different steps taken in training the animals to mount their pedestals and perform their other stunts are described and reasons given for the methods used. Though one notes the popular tendency to anthropomorphize throughout, it is evident that the trial and error method, though not known as such, is the fundamental one employed. The account is delightfully illustrated throughout with occasional episodes in the arena involving mix-ups of lions and tigers (both sexes), which now and then result fatally to one or two animals. Beatty also describes his own escapes from various dangerous circumstances and corrects various erroneous views regarding his methods of handling the animals, denying emphatically the so-called hypnotic-eye effect on the animals.

While the book is not scientific it is an attempt by an expert trainer to state facts, though in a somewhat dramatic form. The book is absorbingly interesting, and it may not be out of place in the library of comparative psychologists. Numerous full page cuts are given. There is no index.

J. P.

The Mystery of Sex through Psychology and Religion. By LESLIE D. WEATHERHEAD assisted by MARIAN GREAVES. Forewords by A. Herbert Gray and J. R. Rees. Epilogue by W. F. Lofthouse. New York, Macmillan Co., 1932. Pp. xxv, 246.

This is an example of the kind of book produced by those who uncritically combine psychology and religion. It is not psychology in the sense that it indicates anything new resulting from research, but does combine with some degrees of success the author's rather moralistic attitude and the views held by Ellis and others. There are chapters dealing with sex education of the young, courtship, marital maladjustment, celibacy, social maladjustments in the sex sphere, and "healthy-mindedness." The author's attitude toward sex is relatively modern, for, although he frowns more than usually on promiscuity, he does have a few good words to say regarding marriage and divorce: liberalism which would not be expected from an English churchman. As a general thing the opinions offered are those having acceptance by the mental hygiene movements of both this country and England and by the authors of a rather extraneous group of arguments in the literature of the church. The author treats perversions and sexual maladjustments with much seriousness. There are brief appendices on the physiology of sex which are fairly adequate. All of the psychology in this book is adopted from the descriptive literature and therefore the psychologist would find reference to the original sources of more value. The forewords and epilogue add nothing.

Institute for Juvenile Research, Chicago, Ill. LOWELL S. SELLING

Neuropathology: The Anatomical Foundation of Nervous Diseases. By WALTER FREEMAN. Philadelphia, W. B. Saunders Co., 1933. Pp. 349.

The book is highly technical and written for the advanced student of neurology or pathology. A minute analysis is made of cell structure and composition, or nerve fiber and its sheath, of supporting cells, and of connecting tissue and lymphatic spaces. The changes brought about in nerve structure and architecture by diseases that include the nervous system in their ravages are set out in great detail. The degenerative alterations in brain and spinal cord are well illustrated by photographs. Great care has been taken to make the work complete, but it is difficult to read without a medical dictionary. The chapters on Intoxications, the Functional Psychoses, and Degenerations give ample evidence of a physiological basis for abnormal behavior. The entire content is the factual results of scientific research.

George Peabody College H. W. FENKER

The Sciences of Man in the Making: An Orientation Book. By EDWIN A. KIRKPATRICK. New York, Harcourt, Brace & Co., 1932. Pp. xv, 396.

The volume gives a description of scientific methods and their applications to the study of human nature and behavior. After an introductory discussion of scientific method, the author plunges into an exposition of the social sciences from anthropology to morals and ethics. Not only are the essential facts and methods outlined, but unsolved problems are clearly indicated and suggestions given for further work. In covering so wide a field of knowledge, the author makes no claims to being exhaustive. Some of his interpretations necessarily tend to be arbitrary and some descriptions of applications sketchy. Very helpful to the student are the illustrative "selected researches" and lists of readings following each chapter.

University of Buffalo HERMAN J. P. SCHUBERT

Über Psychoanalyse und Individualpsychologie. By JOSEF DONAT. Innsbruck, F. Rauch., 1932. Pp. vii, 303.

The author proposes to give a critical review of psychoanalysis and individual psychology from the psychological point of view. Actually, however, his criticism is determined by his philosophical and religious beliefs; namely, by orthodox catholicism. The theories of psychoanalysis and individual psychology are rejected if only for the reason that they are an outcome of "biological materialistic thinking" and because of their immorality.

In aiming at disproving these theories the author collects the arguments from different non-psychological sources: religious, political, and educational writings. The purely psychological arguments are not numerous and are of no theoretical significance. Though written very vividly and giving a clear and simple exposition of Freudian and Adlerian theories, the book is to be considered too much biased to be a good introduction to the study of these theories.

Worcester State Hospital, Worcester, Mass. T. DEMBO

The Story of Scientific Psychology. By ADELBERT FORD. New York, Sears Publishing Co., 1932. Pp. xii, 307.

This book was written to place before the layman the story of the development of psychology as a science. It is composed of a mixture of material, some of which is historical and some of which is material of the type that is ordinarily found in textbooks intended for introductory courses in psychology. Accuracy is frequently sacrificed in an attempt to achieve simplicity and yet in many places extremely technical words are used with no explanations. Approximately one third of the book is devoted to a discussion of the learning process.

George Peabody College of Teachers WENDELL W. CRUZE

Courses and Careers. By RALPH P. GALLAGHER. New York and London, Harper & Bros., 1930. Pp. xxi, 404.

This is a vocational guidance text for use in junior and senior high schools. The arrangement is in the form of suggestive topics and questions. The aim is to orient the students of vocational guidance classes as to the nature of generally valuable personality traits; the value of different courses of study; the nature of the many and varied vocations, together with the requirements for those who would succeed in each. There is a tendency toward over-emphasis of special abilities as distinct from interests or aptitudes. The acceptance of current industrial tendencies, rather than any attempt to inculcate sound psychological principles, seems to be at the basis of too much differentiation between men's and women's jobs.

University of North Carolina HARRY W. CRANE

Validating and Testing Home Mechanics Content. By LOUIS VEST NEWKIRK. Iowa City, University Press, 1931. Pp. 39.

From more than 100 tasks of home mechanics, 72 have been selected on the basis of high favor on the part of schools giving home mechanics courses and according to the social utility of such tasks as indicated by adults. A test of ability to do these tasks has been constructed and two forms are provided for diagnosing practical mechanical ability and for testing achievement. The test was found to correlate 0.26 ± .09 with the Stenquist test, 0.44 ± .07 with I.Q. obtained from the Otis group test, and 0.64 ± .06 with grades given by teachers of home mechanics.

Southwestern College W. R. ATKINSON

The Major Forms of Inhibition in Man. By ERNEST BURTON SKAGGS. Chicago, University of Chicago Press, 1931. Pp. vii, 104.

The monograph is eclectic and as the author says there is "certain discontinuity between chapters." Gaps in the system of facts in the field of inhibition, he thinks, make this "discontinuity" temporarily inevitable. The book is addressed to students: a source book for their acquisition of facts about inhibition which conveniently spares them a certain amount of search through the psychological and physiological literature.

Bethel, Maine H. G. BISHOP

Ethics and Moral Tolerance. By ARTHUR KENYON ROGERS. New York, Macmillan Co., 1934. Pp. v, 323.

It is an indisputable fact that ethics somewhere must connect with psychology. Whether moral action is regarded as conformity with an ultimate law or a decision concerning what one ought to do in specific instances, there are psychological problems involved. Professor Rogers follows a psychological approach to ethical questions in preference to the cosmic abstractionism of both the older absolute idealisms and the newer logical rationalisms. The basis of morality he locates in man's affective life and takes as his position an ethics of sentiment.

So far so good. But unfortunately, although Professor Rogers inclines toward a psychological position, he is at the same time interested in the generality of moral judgments and rigidly opposed to the pragmatic doctrine that morality concerns specific situations. For this and other reasons his affective factors—sentiments of approval and disapproval—are really universalized counters in discussion, rather than actual psychological phenomena. So, despite the original agreeable proposition connecting moral behavior with psychological happenings, the present essay falls short of a satisfactory statement of the psychological aspects of moral phenomena.

Indiana University J. R. KANTOR

Methoden der individuellen Auslese für gewerbliche Berufe. By THEOBALD FÜRST. Abderhalden, Handbuch der biologischen Arbeitsmethoden, Abt. IV, Teil 16, Heft 1. Berlin and Vienna, Urban & Schwarzenberg, 1931. Pp. 288.

The methods described in this volume are not so much of interest to the psychologist as to the physician who has to pass on matters of general health and constitution in connection with problems of vocational guidance and who is required to examine large groups of individuals in schools, clinics, special bureaus, or in industry. The author shows that relevant information on hereditary and morphological aspects can be obtained even in investigations of such kind. For more detailed investigations he presents special methods for examining skeleton, muscular system, respiration, heart and circulation, blood, nervous system, skin, eye, and ear. Most of the methods can be employed without the use of elaborate equipment. No doubt, some of the methods recommended by the author, although of interest from the point of view of research, are of slight value when it comes to making practical decisions in questions of vocational guidance, selection, and placement. The book may serve as an introduction to the widely scattered German literature on the 'applied' side of the science of constitution.

University of Chicago HEINRICH KLÜVER

L'écriture et le subconscient: Psychanalyse et graphologie. By PIERRE MENARD. Paris, Felix Alcan, 1931. Pp. 172, 45 autographs, 22 Fig., and 5 diagrams.

Graphology is regarded as a chapter in the psychology of movement, and as a method of the scientific study of the 'subconscious.' The author discusses the field and limitations of graphology as a scientific method, the objections and difficulties, and the techniques of studying and analyzing handwriting. Handwriting, says Dr. Menard, is a graphic record of uncensored gestures by means of which energy, extent, direction, form, and rhythm can be determined and studied in a precise and objective manner. Each of these aspects of handwriting is discussed and analyzed in detail, stress being laid on rhythm as a subject too little considered in psychological investigations. "The whole of the subconscious is recorded in the rhythm of handwriting" (p. 142).

The book appears to the present reviewer to be chiefly a contribution to the techniques of analyzing handwriting. Although much of the interpreted material is taken from diaries, from the context of which the author has taken clues, it is not clear that "sensuality," "anguish," "egoism," and other mental states described as "subconscious emotions" are themselves reliably defined or reliably correlated with their alleged characteristics in handwriting.

University of Iowa HAROLD H. ANDERSON

De la connaissance de soi. LEON BRUNSCHVICG. Paris, Felix Alcan, 1931. Pp. xi, 196.

This series of lectures attempts to construct a philosophy rather than a psychology of the self. The philosophy in which it engages is, however, mainly of that literary variety so exasperating to readers of a scientific bent. If one can bring himself to read it as he would read Hegel's *Phenomenology of Spirit,* one will find that it has a certain charm. The technique of the discussions is purely dialectical. Thus man begins as *homo religiosus,* is complemented by *homo faber,* develops into *homo magicus,* who, feeling the need of other men, becomes *homo loquens,* who generates by his own necessities *homo politicus.* These various selves are of course all "aspects" or "stages" of the 'Self;' all necessary stages, moreover, in the knowledge of the human pirit.

This book is an attempt to write an edifying series of lectures,. and in the eyes of people capable of being edified, it will seem to have succeeded.

Johns Hopkins University GEORGE BOAS

Classroom Organization and Management. By FREDERICK S. BREED. New York, World Book Co., 1933. Pp. xvi, 472.

This book is one of the "Measurement and Adjustment Series," edited by Lewis M. Terman. It is written for the practical school man, not for the professional psychologist for whom it contains nothing new. This statement is not intended as derogatory and the reviewer feels sure that teachers of educational psychology in our colleges and normal schools will find stimulating material on the subject of achievement and intelligence tests in relation to curriculum reorganization, vocational guidance, supervised study, classification of pupils, and similar topics.

The time is rapidly approaching when the school must judge itself by the success of its students vocationally and avocationally sometime after leaving school. Early determination of ability, of special abilities and of their lack, with corresponding adjustments in extent of academic training and emphasis upon trade training and motor skills must be made early and must be reflected in a differentiated curriculum. Dr. Breed gives valuable suggestions along this line from the standpoint of applied psychology.

University of Pittsburgh W. T. ROOT

Case Studies in the Psychopathology of Crime. By BEN KARPMAN. Washington, D.C., Mimeoform Press, 1933. Pp. x, 1042.

Psychiatrists as well as sociologists are beginning to make assiduous use of the case method in their studies. For the psychiatrist this is undoubtedly a step forward in the right direction, for there is hardly any useful alternative method for the study of abnormal behavior. As long as the conception prevailed that mental disturbances were merely the malfunctioning of an organ—say, the brain—the individual as such could be put into the background, behind biopathological conditions. But that time is happily passing.

In the present voluminous work Dr. Karpman publishes in elaborate detail the case studies of five patients in Howard Hall, the criminal department of St. Elizabeths Hospital. Among the materials, besides the official records, are autobiographies of the subjects, letters, opinions of other prisoners, etc. There is undoubtedly a mine of material here for the study of the criminal insane. The psychiatric interpretation of the present material Dr. Karpman promises to incorporate in a second volume.

Indiana University. J. R. KANTOR

Elementares Lehrbuch der Sozialpsychologie. By W. HELLPACH. Berlin, Julius Springer, 1933, Pp. v, 165.

The author of this brief outline defines social psychology as the study of the means whereby persons are psychically related. Accordingly, he assumes that there are five general subjects to be investigated: (1) the interaction of persons brought about either directly or indirectly (expression, communication, or action); (2) the bases that make for cohesion or detachedness of people (love, hate, etc.); (3) the character of collectivities on the basis of strength, duration, cohesion, etc.; (4) the influence of a collectivity on the innate romanticism or realism of individuals, whether they become leaders, followers, antisocial, etc.

As the above statement indicates, Hellpach uses the term social psychology as a catch-all for various kinds of phenomena. He does not differentiate between psychosociology (the psychic conditions of society); social psychology (the social or cultural forms of behavior); or interpersonal psychology (the study of reactions of persons to persons). More serious than this failure to make social psychology a distinctive discipline, is the fact that the author does not describe and analyze fundamental psychological mechanisms, despite much discussion of "laws." And finally, we may point out that the entire trend of the book is toward traditional conceptions of psychic states operating directly upon other psychic states or through the intermediation of bodily movements and other actions.

Indiana University J. R. KANTOR

College and Life. By M. E. BENNETT. New York, McGraw-Hill Book Co., 1933. Pp. xiv, 456.

This book is designed to meet an important need, and it meets it well. Its purpose is to furnish rational guidance to the college student, with respect to his inner problems, his social problems, his life-plan, and methods of study. The book may advantageously be placed in the hands of any serious-minded young person about to enter college; and also in the hands of any student who might not be serious-minded: it ought to make him so. It is written without condescension and without exhortation; and its motto might well be: "Come and let us reason together." Its standpoint and statements are based on modern psychological principles and findings. It shows how this new knowledge can be utilized for the benefit of the college student.

Before a second edition of the book is called for, which will probably be the case, the author might look up the quotation, "Manners make the man." It is generally ascribed to William of Wykeham, and not to the source the author mentions. Also, the feebleminded child mentioned on p. 192, who could name the capitals of the states, was something more than an idiot.

Knox College R. T. WILTBANK

An Outline of General Psychology. Rev. ed. By ROBERT H. GAULT and DELTON T. HOWARD. New York, Longmans, Greene & Co., 1933. Pp. vii, 452.

This new edition makes only very slight changes in the first one; a few chapters have been shifted and many have been put under slightly different captions—

YOUR JOB

is teaching—our job is manufacturing and publishing. These two lines of activity can not be successfully combined. One or the other must suffer.

In these days of handicaps and financial stringency there is always present the pressure for economy, and about the first thing that comes to mind is the cost of apparatus—a rather insignificant percentage of the total budget—and the possibility of improvising and making equipment with the aid of students. Don't turn your laboratory into an embryonic manufacturing establishment and make amateur mechanics of the students. It doesn't pay. At best the mechanical skill acquired by the student in improvising and making apparatus is of doubtful value and makes no appreciable contribution to his knowledge of psychology.

Your colleagues in the other sciences do not feel called upon to make their own equipment, so why should you?

Like the other apparatus makers we are ready to help you to the best of our ability in your problem of meeting the exigencies of the depression, but we must have your coöperation. Neglecting your equipment reduces the manufacturers' volume and makes it difficult, if not impossible, to keep down prices. Coöperation of instructor, researcher, and apparatus maker is still the best guarantee of progress in the science of psychology the same as it is and has been in the other branches of science.

The last decade and a half has been a period of unusual activity in the erection of buildings—the present should be the beginning of a marked renaissance of lecture room, laboratory, and clinic equipment to repair the ravages of the depression. Buildings are, of course, necessary, but it is men and equipment that make the college and university. Psychology is a laboratory science and as such can not be successfully taught without the requisite equipment.

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Developmental Psychology

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THE PERCEPTION OF COLOR AND CONTOUR: AN UNUSUAL ABNORMAL CASE

By WILLIAM D. TURNER, Bryn Mawr College and HARRY R. DESILVA, Massachusetts State College

The present paper describes a singular case of defective color perception.[1] A superficial examination of the subject (*C*) would seem to indicate total color blindness, but adequate methods of investigation disclose responses influenced by *quality* as against mere *intensity of stimulus*.[2] The *S* really lacks, not physiological 'color processes,' but the more 'central' ability to describe, interrelate, identify and make other explicit judgments regarding color phenomena as such. Except for 'black' and 'white' color names were meaningless to him; yet he discriminated chromatic and achromatic (neutral) colors. His differentiation among chromatic colors was rudimentary, and he did not specifically recognize the category of hue. His perceptual defects even embraced the achromatic colors. There were still other peculiarities which showed no obvious relations to his color anomalies: perception of contours and patterns was poor, and contours tended, if steadily fixated, to change their form, to blur very rapidly, and often to disappear entirely. This 'blurring' differed both in its rate and qualitative characteristics from ordinary visual fatigue or local adaptation.

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* Accepted for publication August 1, 1933.

[1] The experiments reported in this paper were made at Kansas State College, Hays, Kansas and at the University of Kansas, Lawrence, Kansas. To *C* (the subject), and to Professors H. A. Zinszer and R. R. Macgregor of Hays, Kansas the writers are grateful for time and assistance given to the investigations. They wish also to thank Dr. Lyle S. Powell of Lawrence, Kansas, for ophthalmoscopic investigations, and the Faculty Research Fund of the University of Kansas for financial assistance. The greatest indebtedness of the authors is to Dr. Donald McL. Purdy of the University of Kansas for his assistance in the experiments described here and for his suggestions and contributions to the preparation of this report.

[2] His performances with the Ishihara Test for color blindness taken at their face value might indicate total color blindness, but, as will be seen later, they are a result of his defective perception of contour.

Attention was first drawn to this 19-yr.-old *S* when he complained of unusual difficulties in verifying the laws of color mixture. His naming of colors was hesitating, generally incorrect, and inconsistent from time to time. He stated that the words *green, blue* and *gray* meant nothing to him and that his use of them was based somewhat haphazardly on what others had told him about colors of things. He could not name from memory the color of a two-cent stamp (red), nor say whether the stripes in the American flag were red or blue. What color vision he had was of no practical use to him, for his choice of garments depended on the tastes of salesmen and his companions. Yet he alone seemed to be aware of his color difficulties, and supposed that they were due to a lack of intelligence on his part.[8] He could perceive ordinary pictures (colored or uncolored) without seeming difficulty.

PHYSIOLOGICAL DATA

Ophthalmoscopic examination showed normal ocular media and fundi. *C* was slightly hypermetropic and astigmatic: the necessary correction was: sph. +0.75, cyl. —0.25, ax. 90, for each eye. The subnormal visual acuity (20/30 for each eye) is accounted for completely by this refractive defect, since with correcting glasses *C*'s visual acuity was better than 20/20. His binocular amplitude of accommodation was 7.5 diopters. He read ordinary print without difficulty. The correcting glasses reduced eye-strain appearing with extended reading periods.

Moderately bright lights were "unpleasant" and "glaring" to him, but not painful, and could readily be borne. His squinting of his eyes, wrinkling of his brow, and his tendency to turn his head and voluntarily to shift his eyes away and then back to a figure being regarded were due, not to a central scotoma (the high visual acuity and the charted visual fields preclude that!), but to his peculiar defects of contour vision which were more pronounced in central vision. His shifts of fixation were definitely voluntary and were not of the nystagmic variety generally accompanying total color blindness.

C's parents and only sister passed the Ishihara Test as normals; the former knew of no color difficulties in their own parents. *C* had no symptoms of hysteria, nor was there any history of serious disease or brain lesion.

ACHROMATIC SENSITIVITY: LUMINOSITY OF THE SPECTRUM

(1) Visual fields. C's markedly reduced visual fields are shown in dotted lines in Fig. 1. In obtaining them a 2-mm. white stimulus target against a dark gray campimeter was used.

(2) Absolute light thresholds. C's photopic and scotopic sensitivity to light were subnormal, and correspond with his reduced visual fields, his spectrum (shortened at both ends), and the data on dark adaptation and flicker mentioned below.

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[8] By the Army Alpha Intelligence Test his intelligence quotient was found to be above the average.

in the academic curriculum. This, it would seem, is due to the specially constructed scientific test, which is, of course, in favor of the aptitude emphasis of this portion of the study.

The author offers special statistical technique to take care of the inequality between the two experimental groups. These techniques "are designed to do for differences between means what partial correlation does for raw coefficients of correlation." Experimental control would be better. The study shows several tendencies to calculate abilities rather than measure them.

Lincoln Memorial University JESS H. EDDS

Handbook of Child Psychology. Rev. ed. Edited by Carl Murchison. Worcester, Clark University Press, 1933. Pp. 926.

This second edition of the Handbook of Child Psychology retains 15 chapters from the first edition, and includes 9 chapters on new topics. With few exceptions, the chapters retained have been enlarged by the inclusion of references to studies reported since the appearance of the first edition. The volume has increased from 687 to 926 pages and the bibliographies have increased from 1470 to 2815 references. The new topics include discussions of the origin of behavior in pre-natal growth and in development during the first two years; physiological appetites, difficulties of adjustment, and speech pathology; measurement of mental growth and sex differences. A chapter on the adolescent child is also included. These problems have become increasingly important for the student of child psychology and their addition increases the value of the handbook.

Johns Hopkins University BUFORD JOHNSON

Modern Man in Search of a Soul. By C. G. JUNG. Trans. by W. S. DELL and C. F. BAYNES. New York, Harcourt, Brace & Co., 1933. Pp. ix, 282.

This volume consists of 11 more or less related chapters, all save one having been given in lectures. According to Jung, much of the neuroticism which obtains today is attributable to the loss of religious faith. The methods of Freud and Adler are held to be inadequate in dealing with what the author holds to be a spiritual problem, since they fail to take the entire psyche into account.

The present book, although written for the most part in popular rather than scientific style, contains many suggestions of therapeutic value for the student in this field. Furthermore, modern man's need for a more unified self based upon a worthy ideal is stated in a sufficiently convincing manner. The author, however, indulges frequently in a type of *a priori* speculation which though perhaps carrying comforting implications for the religionist is of questionable scientific value. Whereas Freud made the unconscious something to abhor, Jung actually raises it to the status of the World-Ground.

Vanderbilt University EUGENE G. BUGG

"Sensing or Being Aware," for "Sensation," "The Nervous System," for "Reaction and the Nervous System;" "Perceiving," for "Perception," "Motor Reactions—Chiefly Native," for "Instinct," and "Motor Reactions—Chiefly Learned," for "Habit." Neither the methods and results of psychology nor the references are brought up to date. The point of view remains the same. Such terms as 'refractory phase,' the 'all-or-none law,' and such contributors as Lashley, Adrian, and A. Forbes, are not mentioned in the Index; and the questions and chapter-references, in the last part of the book, are practically unchanged. This includes spelling errors and all (except that 'Lickey' has taken on the necessary "l"). The chapter on "Applied Psychology" shows no recent progress, and that on "A Sketch of the History of Psychology" does not even refer to the excellent recent histories by Gardner Murphy and E. G. Boring. The authors may feel that such changes are not necessary for the purposes of the book.

The printers and binders have done an excellent job.

Persons one and Three. By SHEPHERD IVORY FRANZ. New York, Whittlesey House, 1933. Pp. 188. J. P.

This book by Franz was his final contribution to the field of psychology. It is undoubtedly a major contribution in spite of its apparent duplication of similar material extant in the writings of Janet and Prince. No other work sets forth the procedure and technique of reëstablishing a dissociated personality with the same clarity. The pertinent facts relating to the disintegration and reintegration has been woven into an excellent narrative. The reader will be surprised with the tremendous amount of labor expended and the tediousness of such labor. It is recommended to psychologists since in it Franz removes the obscurity which has usually clouded the description of earlier work. Since no preconceived theory on the part of the author had to be proved one finds the discussion free from the mystical concepts of the analysts as well as devoid of other terminology which would tend to hide the real facts. The lay reader will find it intriguing in spite of the incorporation of a considerable amount of conversational material. It will become one of the classic examples of multiple personality and be grouped with such notable cases as Miss Beauchamp, Mr. Hanna, and Patience Worth.

Johns Hopkins University ROY M. DORCUS

Specialization of Verbal Facility at the College Entrance Level. By WARREN G. FINDLEY. Teachers College Contribution to Education, No. 567. Bureau of Publications, Teachers College, Columbia Univ., 1933. Pp. v + 76.

A study of differentiable mental ability is made by applying the Spearman tetrad technique to the results obtained from a scientific and a literary vocabulary test given to 391 college freshmen applicants at Cooper Union Institute of Technology and to 139 students at Yale University. The factor analysis shows that the two tests overlap 86%; 8% of the variance is due to specific factors, and the other 6% is attributable to chance factors of unreliability in the tests; whereupon the author concludes that the two tests measure chiefly a common element and that verbal facility is unitary. The original test data are correlated with the students' records at the two schools and the results suggest a "reliably higher" relationship between scientific vocabulary and grades in the science curriculum than between the literary vocabulary and grades

(3) Dark adaptation. By means of a Nagel Adaptometer *C*'s ascending and descending absolute thresholds were determined alternately. Average light intensities (thresholds in terms of adaptometer readings), each of which was based on one 'appearance' and one 'disappearance' judgment for adaptation periods of 10, 15, 20, 25, 30, and 35 min. respectively, were: 650, 200, 70, 45, 25, and 20. For a normal *S* (Turner) the thresholds were 35, 30, 10, 5, 4, and 3. Turner's first reading is probably subject to an experimental error. If it is disregarded, the remaining ratios between the two *S*s' thresholds for each adaptation period are practically constant: 7, 7, 9, 6, and 7. Thus, *C*'s *relative* increase in sensitivity under dark adaptation is about normal, although his absolute threshold remains above normal.

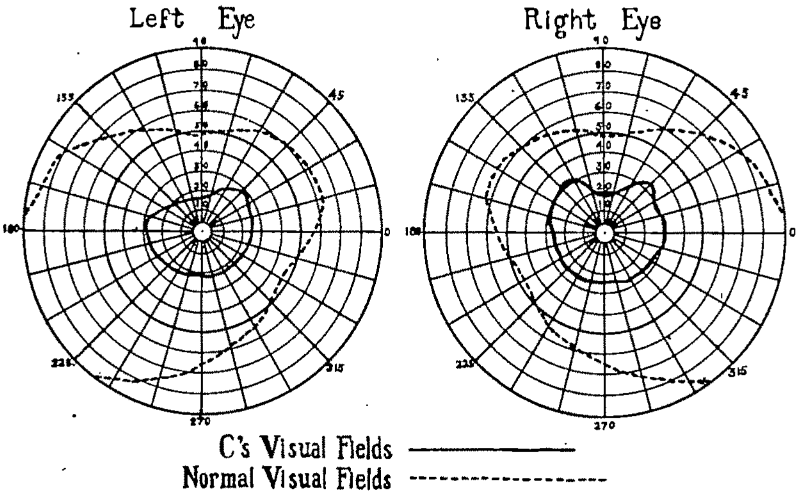

FIG. 1. *C*'S VISUAL FIELDS

(4) Flicker. C saw no flicker in a black and white disk rotating at a speed producing fine, and occasionally even coarse, flicker for a normal subject. A low light sensitivity most readily accounts for the low fusion frequency.

(5) Photopic luminosity.[4] By presenting a homogeneous field of spectral

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[4] The method described for determining spectral luminosities was adopted after three others had to be abandoned because of perceptual defects peculiar to the *S*. (1) *C* was asked to match separately for brilliance each of 5 disks of colored papers with a mixture of black and white rotating on a color wheel. He was unable to do this because he did not know, nor could he be taught, what *brightness* (even of *achromatic* surface colors) meant. (*Vide infra,* Surface colors.) (2) He was then asked to tell when the colored surface and the black-white mixture were most nearly alike. The task was difficult, but average settings (*i.e.* percentages of white, each average based on 9 to 13 judgments) were: blue, 45; green, 31; yellow, 24;

light and asking C to report changes in its brilliance as the wave-length was suddenly changed in steps of about 10 mμ, *C* gave with assurance but subnormal precision judgments indicating that the brightest part of the spectrum was in the yellow region with steadily decreasing luminosity on either side of this maximum. His maximum, although not precisely determined, was at least within 10 mμ of the normal.[5] This conclusion is supported by results obtained with the method of flicker photometry in which spectral colors were equated for brilliance with white light. Time permitted only enough measurements to establish the fact that *C*'s curve was roughly symmetrical with a maximum in the neighborhood of 580 mμ. Under the same conditions the normal *S*'s curve was approximately symmetrical with a maximum at 570 mμ. With more accurate determinations one would expect to find *C*'s luminosity curve normal with respect to shape and maximum.

C's spectrum was shortened at both ends. With very strong spectral light a normal *S* could see wave-lengths shorter than 400 mμ and longer than 800 mμ, whereas *C*'s spectral limits under the same conditions were at 413 mμ (in the violet) and 790 mμ (in the red).

(6) Scotopic luminosities. *C*'s relative sensitivities to wave-lengths were also normal in scotopic vision. After 15 min. of dark adaptation, yellow, orange, blue, and red disks were presented in weak and slowly increasing illumination. One by one they reached a normal *S*'s threshold of visibility in the order given; while with greater absolute illumination *C* detected first the yellow and green disks together, then one by one the orange, blue, and red in the order given.

(7) Differential light thresholds. *C*'s differential sensitivity was tested by presenting a gray paper figure on a gray background, and by varying the illumination of the figure until he could perceive it as distinct from the ground. With short fixations he could differentiate figures from backgrounds with slightly subnormal efficiency.[6]

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orange, 18; and red, 37. A normal *S* (Purdy) gave the following results: blue, 11; green, 33; yellow, 40; orange, 30; and red, 9. Thus, *C*'s curve would be practically the reverse of the normal, but further investigations (*vide infra,* The 'strikingness' of spectral colors) indicate that his settings are probably based somehow on the degree of chromaticity of the colored disks and not on their brilliance. (3) In an attempt to avoid the effects of *C*'s difficulty with surface color brilliance, he was asked to match photometrically a field of spectral color with an adjoining comparison field of white light. His defects in the perception of contour (*vide infra,* Contour vision), led to the abandonment of this method, since the two fields would "run together" into a homogeneous blur of light, especially when their briliances were anywhere near equality.

[5] With the same spectrum (that of a 6-volt incandescent lamp energized by a constant current) the maximal luminosity for a normal subject was at 570 mμ (yellow).

[6] With longer fixations his defective contour vision led to the rapid "fusion" of the figure into the background (*vide infra,* Contour vision).

PERCEPTION OF BRILLIANCE AND OF ACHROMATIC QUALITY; SURFACE AND FILM COLORS

(1) The unique status of black. An effort to teach *C* the meaning of *brilliance* or *brightness* disclosed a new anomaly: while looking at a gray and a black paper he protested that the statement, "The gray is *brighter* than the black," was meaningless to him. He preferred to call ***black*** the ***brighter*** color; in fact, black was the "brightest," strongest," or "most intense" of all his colors. Black paper gave him a "stronger" sensation than white paper or even the direct view of a bright electric light which was for him "unpleasantly glaring."[7]

C's black had not only striking phenomenal properties but also very positive functional ones. In the normal irradiation experiment a white square on a black ground looks larger than a black square of corresponding size on a white ground; but to *C* the black square looked definitely larger.[8] The section on Achromatic Contrast (*vide infra*), further illustrates the strength of *C's black process.*[9]

(2) Surface colors.[10] *C* manifested a disturbance of achromatic color perception which differed strikingly between film and surface colors. In studying these peculiar differences achromatic surface colors were first employed: *C* was shown a number of neutral papers ranging in shade from white to black. He called the lightest paper "white" and darkest paper "black." For the intermediate shades he had no name; ***the word "gray" was one he never used and could not understand.*** If the papers were arranged side by side in order of brilliance, *C* could detect ***no serial relationship*** between the colors. He said: "I see black at one end of the row, and white at the other end, but why you put them there with the other colors I do not understand." When the white and black papers were removed he said: "I see some colors which are somewhat different from one another. How they

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[7] In addition, it was later found that in making judgments of brightness *C* spontaneously used terms which were the reverse of the normal. When the intensity of a field of surface or film color was diminished, he most naturally described the change as one of "increasing brightness." He was, however, aware of his terminological peculiarity and could, when requested, report in "our [normal] language;" he would then characteristically pause a moment to "translate."

[8] J. B. Miner (*Psychol. Monog.*, 6, 1905, 103-118; 110 f.) reports a case of reversed irradiation which has, however, nothing else in common with *C*.

[9] If one still needed evidence to prove that *black* sensation corresponds to real physiological excitation (rather than to an absence of excitation), the foregoing facts would almost suffice for the proof.

[10] D. Katz (Der Aufbau der Farbwelt, *Zsch. f. Psychol.*, Ergbd. 7, 2 Aufl., 1930) has distinguished between "surface" and "film" colors (*Oberflächenfarben* and *Flächenfarben*), and though he admits that colors may pass by imperceptible degrees from one form to the other, the denotation of "filminess" and localization is important, since for normal *Ss* colors behave very differently when these attributes change.

differ I cannot tell. I cannot see why you group them as you do, or even why you put them together at all."[11] Viewing the three darkest papers alone he called the darkest one "black," and could say that the darkest one was "more like black" than the third color. Of the three lightest papers, he called the lightest "white," and, though he saw them as "different from each other," he could not say which was more like the white. It is well to note that these 'comparisons' were difficult for him.

At this point a discussion of normal perception may illuminate the peculiarities just described. Although some visual theorists regard achromatic color as unidimensional, there are reasons for distinguishing at least two aspects of achromatic color.[12] *First,* to normal *Ss* an ordered row of gray papers (like that presented to *C*) exhibits a series of *intensities* or brilliances, and at the same time a series of *qualities* progressing from white through the various nuances of gray to black. *Secondly,* consider that in the phenomena of color constancy the quality of black or white objects survives almost completely changes in illumination by white light, and especially that, while a black object (perhaps reflecting even more light than a particular white object) remains black, it still appears more *intense,* more 'impressive,' or even 'brighter' than the white. Here the quality of achromatic color remains practically constant; its intensity varies.[13] *Thirdly,* achromatic *quality* as such seems to appertain primarily to surface colors. Gelb[14] describes a *S* with a cerebral injury who perceived all colors as film colors, who could not perceive any "white" or "black," nor remember what these terms had previously meant to him. Krauss[15] maintains that totally color-blind *Ss* manifesting no constancy do not see even achromatic qualities, because they do not perceive illumination as such.

One is led to conclude from *C*'s inability to match chromatic and achromatic surface colors with respect to intensity (*vide supra,* Photopic luminosities) that *intensity* as a *dimension* could not be abstracted from the total *surface color* situation, and that quality and intensity were inextricably knit together in an unanalyzable, global or totalized perception. *C* was apparently encountering this same difficulty with the achromatic surface colors mentioned above in this section. When one considers that the end members of the gray series (the white and the black) were each more "intense" or "strong" than any other colors he knew, one can readily appreciate the obstacles hindering his perception of any serial or 'family' relationship in the black-gray-white series. Further, assuming the unanalyzable status of surface color intensity, and the ambiguity of the term *intensity* created by the unique status of black and white, the anomalous position of the grays in *C*'s surface color system and their lack of clear cut relations to each other and to black and white become comprehensible. Surface 'grays' were to him isolated colors with no abstractable or identifying dimensions.

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[11] Questioning and testing *C* disclosed that his difficulty was not one of inability to grasp the concept of series as such, but was due to the absence of a real perceptual series in this particular case. Lack of training with the particular sense-material also fails to explain his defect, since a three-year-old child was able to construct a gray series after having once seen one.

[12] It is implied by the following authors that there are *more* than two dimensions of achromatic color: D. Katz, *op. cit.,* 162 ff.; A. Gelb, Über die "Farbenkonstanz" der Sehdinge, Bethes *Handb. d. Physiol.,* 12, (1), 1929, 594-678, especially 616 f.; and G. E. Müller, Über die Farbenempfindungen, *Zsch. f. Psychol.,* Ergbde. 17 and 18, 1930, especially 25 f. E. Hering, *Grundzüge der Lehre vom Lichtsinn,* 1920, 11, distinguishes two dimensions of achromatic color.

[13] Note that the quality, not the brilliance or brightness, is constant; and that the commonly used term, "constancy of brilliance" (*Helligkeitskonstanz*), is misleading, and had best be supplanted by a term such as "constancy of achromatic quality."

[14] A. Gelb, Über den Wegfall der Wahrnehmung von Oberflächenfarben," *Zsch. f. Psychol.,* 84, 1920, 193-257.

[15] S. Krauss, Die Beleuchtung im Sehen des total-Farbenblinden, *ibid.,* 102, 1927, 219-264.

(3) Film colors. The behavior of *C*'s film colors sharply contrasts with that of his surface colors. He could *easily* judge the intensity (or 'brightness') of film colors. Spectral fields could readily be judged and compared in this respect even when they were heterochromatic.

However, with regard to achromatic film colors *C* was (with one exception) *unable* to use the words *white* and *black;* he described them as being simply "brighter" or "darker" ("duller," "dimmer"). The normal person uses the word *white* to describe a brilliantly illuminated white surface seen through a reduction screen, a brilliant, frosted electric lamp, the moon, the sun, and clouds; but *C* refused to call the first two objects "white," and could think of no color name for the last three. Likewise a normal *S* called "black" a small hole in the white screen just mentioned, and also the shadow of a hand in the field of a projection lantern. *C*, however, could agree to none of these appellations; except for the field of his closed eyes he never saw film colors or shadows as black, no matter what their darkness. With the one exception *C* perceived achromatic film colors entirely as intensities and not as qualities, and he thus agreed with Gelb's case in indicating that achromatic *quality* attaches primarily to *surface* colors, *intensity* or brilliance to *film* colors.[16]

(4) The 'constancy' of achromatic color. A piece of black paper illuminated by a nearby, strong, electric light looked "black" to both a normal *S* and *C*, when they were allowed to view the total situation; and a piece of white paper illuminated by a distant, weak lamp was called "white" by both, even though the black paper actually reflected more light than

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[16] There is often a tendency for normal *S*s to speak of the brilliance of achromatic surface colors rather than of their quality. A cloudy, night sky is often "dark" rather than "black;" the sun is often "brilliant" rather than "white;" and the faintly illuminated field of the photometer is "dim" rather than "gray." Even a normal *S*'s perception seems to indicate the lability of *achromatic quality* as a dimension of film color.

the white one. When the papers were viewed through a reduction screen both *S*s called the black paper "brighter" ("dimmer" in *C*'s natural terminology). Thus, for *C*, as for normal *S*s, "whiteness" or "blackness" is relatively independent of the physical intensity stimulating the retina.

(5) *Achromatic contrast.* Two rotating disks, each containing originally 50% white and 50% black, were presented in front of black and light gray backgrounds respectively. The disk in front of black was held constant at 50% each of white and black and the percentage of white in the disk in front of the gray background was increased until *C* reported that the two disks looked "the same." With short exposures, and when the disks were separated sharply in the third dimension from their respective backgrounds, *C*'s contrast effects proved to be approximately normal.[17]

TABLE I

RESULTS OF THE ISHIHARA TEST

Plate no.	*C*'s performance		Normal *S*	Totally color-blind *S*	Red-green blind *S*
	Standard plates	Enlarged plates			
1	12	12	12	12	12
2	0 or 8	0	8	Difficult	3
3	0 (doubtful)	Cannot read	6	Difficult	5
4	Cannot read	6 (doubtful)	5	Difficult	2
5	Cannot read	9	74	Difficult	21
6	Cannot read	8	2	Difficult	Difficult
7	Cannot read	0	6	Difficult	Difficult
8	Cannot read	Cannot read	5	Difficult	Difficult
9	Cannot read	9	7	Difficult	Difficult
10	Cannot read	Cannot read	Difficult	Difficult	5
11	Cannot read	Cannot read	Difficult	Difficult	2
12	Cannot read	Cannot read	26	Difficult (?)	(26)
13	Cannot read	Cannot read	42	Difficult (?)	(42)
14	Cannot trace	Cannot trace	Traces red	Cannot trace	Traces blue
15	Cannot trace	Cannot trace	Traces	Cannot trace	Difficult
16	Cannot trace	Cannot trace	Difficult	Difficult	Easy

(6) *After-images.* Positive after-images of a brilliantly illuminated white cross on a black ground were obtained, as were also negative after-images of longer duration. The latter persisted for shorter times than did those of normal *S*s. No satisfactory evidence was obtained for chromatic after-images or contrast; these phenomena would obviously be difficult to demonstrate.

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[17] With long exposures and with the disks close to their backgrounds, the white in the disk in front of the gray had to be *reduced* to 20%, since the other disk was darkened by the mixture of its background with itself. Such 'fusions' of the color of figure and background were one aspect of *C*'s defective contour vision (*vide infra,* Contour vision). It is interesting to note that a more extended investigation may disclose in *C* a supernormal contrast effect compensating for such defects of contour vision.

CHROMATIC VISION

(1) The Ishihara and Nagel tests. *C* was given the Ishihara Test for color blindness first in the usual fashion, and later with the aid of an epidiascope which projected well saturated figures enlarged to 70 cm. in breadth. Under both sets of conditions *C* reported on what he saw during the first few seconds of observation. In viewing the enlarged figures he wore glasses correcting his refractive defects, and stood at a distance of 180 cm. Table I itemizes *C*'s performances and those of a normal, a totally color-blind, and a red-green blind subject.[18] It will be seen that *C* might casually be diagnosed as totally color-blind. However, he was completely unable to cope with Plates 10, 11, and 16, whereas a totally color-blind person would have had only *some* difficulty with them. A diagnosis of C as *almost* totally color-blind is not only inadequate but utterly misleading, both in the light of his peculiarities already mentioned and those about to be discussed. No ordinary test is adequate to his case.

With the Nagel Cards, all the dots in each ring looked alike with the exception of those normally designated as 'red;' these dots *C* designated as "standing out from the others." Since the Nagel Test requires the use of color names, it was obviously beyond *C*'s powers.

(2) The 'strikingness' of spectral colors. While matching spectral colors with white light in respect of brilliance (*vide supra,* Photopic luminosity), *C* remarked that one color (a spectral blue) was especially hard to match. This color he described as being "peculiar" or "striking." Later he reported that the spectral red was also "striking" but less so than blue. Greenish hues were rarely, yellow and orange-yellow were never, called "striking." His reports upon the "strikingness" of a number of wave-lengths presented haphazardly as shown are given in Table II.

C continuously regarded a field of spectral color while its wave-length was progressively increased, beginning at a wave-length in the middle of the spectrum. He began at a fairly delimited transitional region to report definite "strikingness." (Seven trials yielded the wave-lengths of 656, 703, 682, 710, 684, 664, and 660 mμ.) Decreasing the wave-length from the original starting point disclosed another such transitional region (located in six trials at 480, 478, 478, 504, 478, and 480 mμ respectively). (The individual determinations of long-wave and short-wave transitional regions were made alternately.)

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[18] In Plates 1-13 the patterns to be perceived are *numbers;* in Plates 14-16 they are *wavy lines* which the subject is asked to trace. Note that *C*'s performance with the enlarged plates might be judged as slightly better than that with the plates of standard size.

What did "strikingness" mean? Since for normal *S*s the end regions of the spectrum are more saturated than the middle, one might conjecture that strikingness is simply 'coloredness' or saturation, and that *C*'s system of chromatic quality comprehended not the two dimensions of hue and saturation, but only one of 'coloredness.' That strikingness was related to saturation was indicated by *C*'s report of 'more striking,' when a second blue filter was inserted into a projection lantern which was already giving a rich blue field. But the status of spectral violet for *C* detracts from the 'one-dimension hypothesis.' Normally violet is more saturated than spectral blue, but *C* described it as *"not striking, but unpleasant."*[19] If one assumed that violet had a special 'chroma value' for *C*, the foregoing hypothesis would remain tenable; but such logic bases one assumption on another, and evidence from the experiments on color matching tend to invalidate the hypothesis anyway.

TABLE II

The Strikingness of Spectral Colors

Wave-length. mμ	Normal color	Degree of strikingness
470	blue	striking
530	yellow green	not so striking
650	orange	rather striking
570	yellow	not striking
540	yellow green	striking
470	blue	striking
600	orange	not striking
500	blue green	not striking
520	yellow green	not striking
550	yellow green	not striking
450	violet	not striking, very unpleasant
490	blue green	not very striking
680	red	fairly striking
700	red	fairly striking
580	yellow	not striking
630	red	fairly striking
660	red	fairly striking
480	blue	striking
440	violet	unpleasant
430	violet	unpleasant

(3) *Color matching.* The following experiments reaffirm previous indications that *C* had some chromatic perception, and that he could under certain conditions distinguish chromatic colors as a class from achromatic colors.

To test *C*'s ability in color matching he was handed in "Series I" a disk of colored paper (6.5 cm. diam.) and asked to find one like it among a group of disks of the same size and texture which were spread out in a haphazard spatial order on a black cloth. (The cloth lay on a table near a window, and was illuminated by light from a clear sky; the spatial arrangement of the disks was changed from trial to trial.) Besides one or more chromatic disks[20] the group included 18 neutral disks ranging by small steps of color from a very dark to a very light gray.[21] In "Series II" a white cloth background was used.[22]

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[19] *C* said that the unpleasantness was in the appearance of the color itself, and did not come from any feeling of eye-strain.

Tables III presents the results of Series I and II. The second column shows the chromatic colors present in the group from which the choice was made during the various "color situations" (numbered "1" to "32" in the first column).[23] The third column contains the various samples which the *S* had to match, and his choices are recorded in the fourth and fifth columns for Series I and II respectively.[24] Footnotes contain significant comments made by the subject during the matching.

First, a B disk was placed among the neutral disks on the cloth, and *S* was given an exactly similar disk to match. Without hesitation he chose correctly—a performance no doubt easily explainable by the 'strikingness' of B. However, under analogous conditions he readily matched a R, and even a Y, a G and other colors![25] Chromatic colors resembled their corresponding samples more than they did the grays![26]

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[20] The following abbreviations will be used to designate the various colors used: R = red, O = orange, Y = yellow, G = green, B = blue, P = purple, L = lavender, YG = yellowish green, BG = bluish green, RP = reddish purple, BP = bluish purple, and Gy = gray. The prefix "u" means "unsaturated," and "vu" means "very unsaturated." The R, B, and P disks were, to the normal *O*, very highly saturated, the other disks considerably less so, and the disks qualified by "u" or "vu" had very weak saturation.

[21] Black and white disks were excluded because there was no uncertainty about their recognition and discrimination.

[22] The minor differences between Series I and II were quite possibly due to fatigue.

[23] Except in Situations I 31, 32, the 18 neutral colors were, of course, also present.

[24] "No match" indicates that the *S* was unable to find any color in the group to match the sample he held.

[25] Cf. Table II, Choices: I 1-5; II 1, 6, 11. In one case (uP, Choice II 10) the *S* chose *gray* to match the (weakly saturated) sample, although elsewhere (II 18) the same sample was recognized as chromatic. In three other cases (G, Y, and P; Choices: II 3, 4, and 12) the *S* could not decide upon either a chromatic or neutral color for a match; yet at other times (I 3, 4, 5) a chromatic color was chosen to match these same samples. Surely these confusions do not result from an absolute lack of physiological color response to these particular stimuli—especially since, when the *S* was given a *gray* to be matched, he always matched it with other grays, never with chromatic colors!

[26] Exceptions to this performance were found with O, R, P, L, and uP samples; Choices: II 7-10.

TABLE III

RESULTS OF THE EXPERIMENT IN COLOR MATCHING

(See footnote 20 for the meaning of the symbols)

Color situation No.	Colors presented	Sample to be matched	C's choice	
			Series I	Series II
1	B	B	B	B
2	R	R	R	
3	G	G	G	G or Gy
4	Y	Y	Y	Y or Gy
5	P	P	P	
6	BP	BP		BP
7	RP	RP		No match (f)
8	O	O		No match (g)
9	L	L		No match
10	uP	uP		Many Gy's
11	vuB	vuB		vuB
12	B, uB	B		B
13	P, uP	P		uP or Gy
14	R, uB	R		uB
15	R, uB	vuB		R or vuB
16	R, vuB	R		vuB
17	G, uP	G		uP
18	G, uP	uP		uP or G
19	G, Y	G	G or Y (a)	
20	G, Y	Y	G or Y (b)	
21	B, R, G, Y	B	B (or G?)(c)	
22	B, R, G, Y	R	B (d)	
23	B, R, G, Y	G	B	
24	B, R, G, Y	Y	B	
25	B, G, Y, P	P	B (e)	
26	B, YG, L, P	P	B	
27	B, P, G, O, Y, RP	RP	B	
28	G, P, RP, YG	B	G	
29	G, P, RP, YG	R	No match	
30	L, P, RP, YG, BP	BP	No match	
31	B, R, G, Y	R		B
32	B, R, G, Y	G		B

(a) "Neither Y nor G looks like the Gy's, but I cannot say which of them looks more like the sample.

(b) Same remark as in (a).

(c) "Either B or G would match. B is more striking."

(d) "R, G, or Y would match. I chose B because I like it best."

(e) "P was the second best match."

(f) "RP is not like the sample, but I like it more than any of the others."

(g) "O is not like the sample, but is nearest."

But when the *four* chromatic colors, R, Y, G, and B were present among the group of grays on the cloth and the *S* was given a R to match, he said, "The sample is hard to match." He pointed out the four chromatic colors as being more like the sample than were the others, but when asked to choose the one of the four most like the sample, he chose, not the R, *but the B,* saying, "Any one of the four would do, but I choose this one, though I do not know why." The same result was obtained with a Y or G sample! Why?

When asked to point out the most *striking* color in the group he indicated the B. In every experiment in which the highly saturated B was present among the group from which he had to choose, he chose it, regardless of the hue of the chromatic sample![27] Accompanying this predilection for B was his pronounced and peculiar reluctance to refer to the sample even during difficult matches. After one glance at it he would lay it face down on the table and ignore it. Frequently, when a match was apparently impossible, and when he was forced to choose, he would select the B, saying, "I do not know why I pick this; perhaps it is because I like it." Under these conditions, he would even refuse as a match the disk exactly similar to the sample, even when it was offered to him! *He was not matching colors.* He was making, not "relative," but *"absolute"* choices.[28]

A most significant fact is that *C*'s preference for B still manifested itself even when it was presented in unsaturated or highly unsaturated form, and in company with a well saturated R (which was itself equal in strikingness to Y and G, and more striking than the grays). This result agrees with the anomalous status of violet (*vide supra,* The 'strikingness' of spectral colors) in casting doubt on the hypothesis that the *C*'s chromatic system was confined strictly to the single dimension of saturation. If his 'saturations' were roughly normal, then on the bases of the unsaturated status of violet and his preference of vuB as against a saturated R, one must conclude that his perception was not only a perception of "so *much* strikingness," but also a perception of a qualitatively more or less specific 'strikingness,' a vague and unformulated awareness of *hue.* It is difficult to see how he could have chosen uB and vuB except on the basis of a qualitative resemblance to B.

Why then was he unable to *match* colors with respect to this attribute, or to recognize the difference between hues, or their identities? Present knowledge permits only the comment that what perception of hue existed did so in a curiously 'latent' form.

In *C*'s unusual performance while taking the Holmgren Wool Test (the results of which are given in Table IV), one sees a decided tendency to 'match' a group of colors *similar to each other in hue,* although *quite*

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[27] Cf. Table III, Choices: I 12, 21-27; II 12, 31, 32.

[28] When the *S* was asked to arrange in decreasing order of "strikingness" the four chromatic disks, R, Y, G, and B, together with the achromatic white, black, and grays; he placed the black first, white second, B third, then R, Y and G in a group, and finally the grays. When asked to arrange the disks in decreasing order of aesthetic preference, he indicated exactly the same sequence. Evidently "strikingness" *in its most general sense* meant something like "impressiveness" or *Eindringlichkeit.* (It is interesting to note, but difficult to explain, why the red pigment was not striking; spectral red was denoted as striking.)

different from the sample. The best account of these results would involve *C*'s latent capacity for hue discrimination, assisted by the close juxtaposition of a great number of highly saturated colors of different hues. But here, as before, his failure to *match* the sample by the group chosen remains enigmatic.[29]

(4) Discussion of C's color vision. A final analysis of *C*'s color perception is not possible with the data at hand. His defect certainly differs from simple color blindness, for a totally color-blind person would not have distinguished the "colored" from the "colorless." To account for his behavior one needs to assume a vestigial perception of hue, even though his introspections and color matches show that he could not judge hue as normal persons would have done. His anomalies of judgment cannot be attributed to retinal or cerebral mechanisms implied by the name *color blindness;* they must be ascribed to abnormal functioning of 'higher' levels of the visual nervous system.

TABLE IV
Results of the Holmgren Wool Test

Sample	C matches the sample with
red	6 greens
medium blue	5 browns
saturated blue	2 purples, 2 greens
tan	no match (C said it was white, and could find no others like it)
green	7 reds and pinks
brown	2 lavenders, 5 tans
light blue	light tans, light oranges, and light lavenders
pink	no match

Of course, some 'lower level' color blindness or color weakness might coexist and operate with the higher level anomaly. But how can it be discovered? No suitable test for the defect is yet at hand.[30] Yet even now one can say that *C* is not red-green blind; a red-green blind *S* would have chosen red and green Holmgren wools indiscriminantly to match either red or green. Moreover, *C* reported no figures in the Ishihara Test which a red-green blind *S* can read and a normal *S* cannot. Nor is the spectrum shortened at the red end *alone,* as in protanopia (the most frequent subtype of red-green blindness). *C* is certainly not red-green blind.[31]

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[29] *C* remarked as he made his choices, "I have no idea why I pick these colors."

[30] Tests requiring the matching or naming of colors are obviously unsuitable. A test demanding only the perception of patterns or figures, discriminated from their backgrounds on the basis of hue, would be adequate were it not for *C*'s defects of contour vision.

[31] Could it have been that *C*'s color processes were all greatly reduced in degree, so that colors appeared extremely unsaturated to him? A. Ackermann (Farbschwelle und Feldstruktur, *Psychol. Forsch.*, 5, 1924, 44-84) notes that normal *S*s may recog-

CONTOUR VISION

(1) General characteristics. Under certain conditions, when *C* maintained fixation of figures for a few seconds, four sorts of changes occurred in the figures, which seem in principle to be distinct: *(1) alterations of form; (2) disintegration of contour; (3) metamorphosis of surface to film color;* and *(4) leveling out of brilliance differences* between parts of the figure or between the figure and its background.

If steadily fixated, a 7.6-cm. white square lying 150 cm. distant on a black background[32] rounded off its corners after a few seconds and became a smaller circular figure with sharp contours; then after a few seconds more there resulted a blurred figure whose color was more like that of the background and was filmy in appearance. The change in color appeared over the whole figure all at once and its onset was sudden.

Neighboring figures, when steadily fixated, would suffer peculiar changes in form (to be noted below) and would even coalesce. Two 7.6-cm. squares with their sides in alignment and lying 2 or 3 cm. apart would after a few seconds join to form a *solid white rectangle with sharp contours.*

C could somewhat control the rate of progress of these phenomena, shortening or lengthening the interval preceding their onset, but he could rarely prevent their ultimate occurrence, if he maintained fixation. When he made an effort to coalesce two 1.3-cm. squares, 1.3 cm. apart, they coalesced after 3 sec. When he tried to keep them apart, the time was increased to 7.5 sec. During the experiments described below he maintained a passive attitude towards the phenomena, except where it is otherwise stated.

(2) Factors in C's contour vision. In a dimly lighted room *C* was allowed to look with his right eye through a small camera shutter at figures 150 cm. distant and illuminated by two 40-watt 'daylite' lamps. When the camera shutter was opened he started a stop-watch, and stopped it when he experienced a predesignated figural change. The effectiveness of a given factor for such changes was stated in terms of the rapidity with which it induced the change. Thus the *rounding-off time* (or the interval elapsing before the corners of a square rounded off), the *blurring time* (or the

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nize some chromaticity in low chroma stimuli, and yet be unable to characterize the hue. Could this difficulty have existed in an exaggerated form in *C?* If so, one would expect him to have confused consistently the less saturated color with grays, as he did not do. Moreover, one would not expect him to have recognized the chromaticity of a blue not far removed in saturation from the color threshold of a normal *S.*

[32] Except where otherwise stated, the figures mentioned in this section were white, and were placed on or before black backgrounds.

interval elapsing before the contours of a figure disintegrated), *or* the *coalescing time* (the interval elapsing before two squares coalesced) was taken as a measure according to the stimulus-situation or convenience. Except where it is otherwise stated, the time-averages given below are based on 5 observations each.[33]

(a) Size of fields (figures). Smaller figures blurred sooner, and a unit change in figural size affected the blurring time of smaller figures more than that of larger ones. Thus squares 2.55, 5.05, 7.6, 10.15, 12.70, 15.30, 17.70 and 20.30 cm. wide blurred respectively after 4.8, 6.4, 7.7, 8.1, 9.4, 12.4, 12.4, and 14.2 sec.

(b) Locus of fixation. Figural changes occurred more readily in central than in peripheral vision. *C* fixated a dot in the center of the background and attended to a 7.6 cm. square placed beside it at various distances. At visual angles (from the fixation dot) of 30°50′, 7°40′, 11°0′, and 14°55′,[34] the square rounded off after 3.9, 6.5, 8.1, and 12.2 sec. respectively.[35]

(c) Relative brilliance of figure and background. Figural changes occurred sooner with smaller brilliance-differences between the figure and background. Six Hering grays (7.6-cm. squares; Nos. 47, 43, 31, 25, 20, and 10—from dark to light gray) rounded off after 3.0, 3.6, 3.7, 4.4, 4.8, and 4.8 sec. respectively. They blurred after 5.8, 6.0, 7.3, 8.1, 8.6, and 9.1 sec. respectively.

(d) Proximity of figures. Two 7.6-cm. squares coalesced more rapidly when they were closer together. Thus, when respectively 1.3, 2.55, 3.8, 5.05, 6.35, and 7.6 cm. apart, they coalesced after 2.8, 3.5, 4.5, 5.5, 10.0, and 17.0 sec. With greater separations the rectangle formed was "not so solid" in appearance.

(e) Perception of depth. Figures did not change as a rule when they were binocularly perceived as being some distance in front of the background; only occasionally very slight changes appeared. A single square 31 cm. before the background did not change at all. Two squares 1.3 cm. apart and 2.55 cm. before the background coalesced, but the rectangle formed was not solid. Two squares did not coalesce when one was on the background and the other was "close beside" it but 10.2 cm. in front of the ground.

(f) Contour regularity. Excepting circles, figures with regular contours changed more readily than those having sharp angles in their contours. Thus, briefly: *Circle:* became smaller after a few seconds; contour and original color maintained. *Square-like figure* (with one side sharply concave): became darker; form maintained. *Equilateral triangle:* form maintained until contour disintegrated. *Square:* became circular before contour disintegrated. *Octagon:* became circular immediately; contour disintegrated relatively much later. *Letter R:* form and contour maintained even after 30 sec. *Human profile* (upright): "looked like a stubby cucumber" after 30 sec.; contour disintegrated 15 sec. later. (When inverted): changed in exactly the same fashion.

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[33] In a good share of the cases the probable errors of the averages, if calculated, would be as large as or larger than the averages themselves, and the differences between the *adjacent* averages would not be statistically significant; but in no single case was there an inversion in the series of averages.

[34] At visual angles greater than 15° figural changes were not readily observable.

[35] These averages were based on two observations each.

(g) Figure-ground relation. Parts of a field seen as *figure* changed *less* readily than those parts seen as *ground.* Fig. 2 was presented on a black background. When regarded as a *black ring on white paper,* the small white square blurred and became black after 25 sec. When the small white square was regarded as *figure,* it blurred after 31 sec. When Fig. 3 was also presented on a black background, and was regarded as a *white ring on a black ground,* the black square receded to half its original size after 27 sec. When the black center was made *figure* it became only slightly smaller after 30 sec.

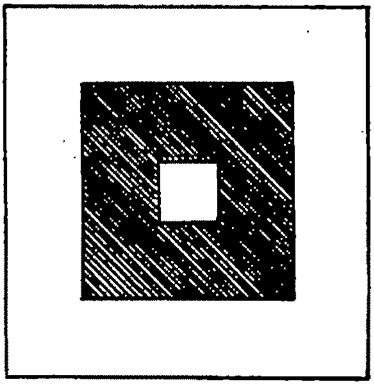

FIG. 2

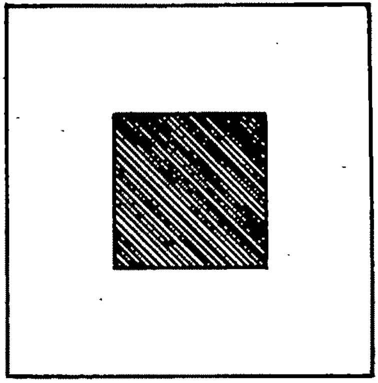

FIG. 3

(h) Participation in totalities. Adjacent areas which belonged to larger totalities either *coalesced* or *failed to coalesce* so that the characteristic totality was maintained.

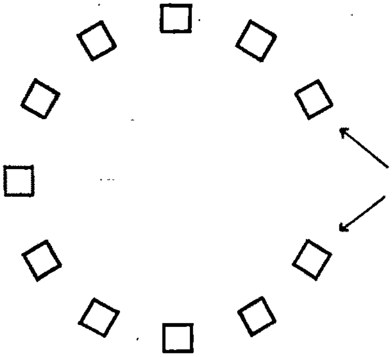

FIG. 4

Thus when *one* of two 7.6-cm. squares, 3.8 cm. apart, was fixated and regarded as *figure* it became circular; when it was again fixated, but *both* were made *figure,* the squares coalesced. When the two 1.3-cm squares previously mentioned were incorporated in Fig. 4 (as shown by arrows), and when the figure was regarded as a *circle,* they coalesced after 5 sec.; but when the figure was regarded as a *letter C,* they coalesced after 14 sec. When these same squares participated in Fig. 5 (as shown by arrows), the squares composing the *separate* circles coalesced rather solidly after 14 sec., but the squares under consideration did not coalesce, though they were as close to each other as they were to the adjoining members of their respective circles.

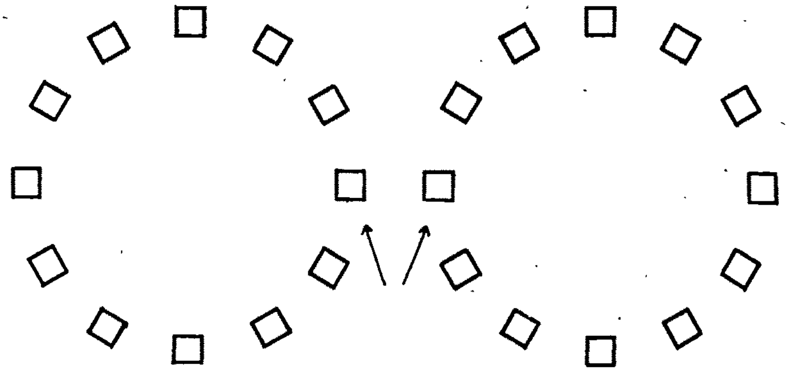

FIG. 5

(*i*) *Special changes in contour with proximity of larger figures.* When the figures shown at the left of Fig. 6 were placed a few centimeters to the left of a larger circular disk of the same brilliance, *C* represented their appearance by drawing the corresponding figures at the right of Fig. 6. When the large disk was removed, he drew them as they really were.

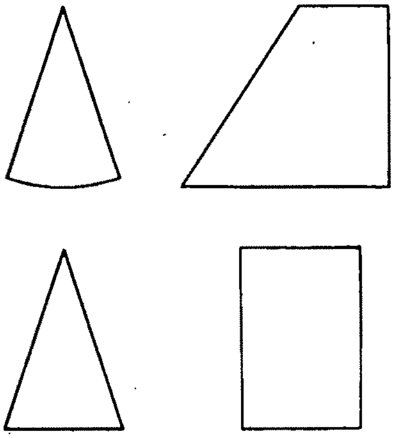

FIG. 6

(3) Relation to normal visual phenomena. With continuous fixation normal, local adaptation or 'fatigue' levels out differences of brilliance and blurs contours. But *C*'s 'blurring effects' differ in being much more rapid and in involving apparently an interaction between adjoining visual areas such that there is an actual mixture of spatially separated processes. Thus, a gray area on a black ground appeared darker than an identical area on a light gray ground.[36] Furthermore, the effects upon *C*'s 'blurring' of such factors as the perception of depth, of form, and of *figure* and *ground,* and the operation of his volition in those functions indicate that the effects are *not retinal* in nature.

AUDITORY PERCEPTION

Although *C*'s absolute threshold for sound was approximately normal, he was practically tone deaf. In discrimination of pitch, as determined by the Seashore Test of Musical Ability, he ranked below 97%, and in tonal memory below 95% of the population sample given in the norms. At present these results cannot be unequivocally interpreted.

DISCUSSION

Is there some common factor underlying *C*'s various perceptual peculiarities? One might suppose that the blurring contours and their attendant equalization of color fields might interfere seriously with the adequate recognition and discrimination of colors. However, it must be realized that the factors of gross surface texture, solidity, tridimensional separation of objects from backgrounds, and eye-movements prevent the blurring effects so marked in the foregoing experiments, and that thereby *C*'s perception of contours and forms would suffice to offset the mutual destruction of adjacent color fields. No relation between his perceptual defects of color and contour seems to exist.

Again, the equivocal meaning of term *perceptual defect* must be made clear. *C* differentiates chromatic and achromatic colors, and some of his color matches indicate that in some sense hues exist in his color world; in this sense he 'perceives' color or hue. However, his failure actually to match colors or to judge certain of their attributes implies that in another sense he does not 'perceive' hue. To describe his defect adequately and precisely one must differentiate between 'seeing' as a primitive 'sensing,' and analytic perception with its sharply defined categories and explicit judgments. It is *C*'s color *perception* which is disturbed.

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[36] *Vide supra,* Achromatic contrast. P. F. Swindle (Effect of strychnine on visual reliability, *Amer. J. Physiol. Optics.,* 6, 1925, 3-22) notes that a mixture of adjoining colors takes place under continuous fixation in normal peripheral vision. The relation of such effects to *C*'s cannot yet be pointed out; it is well to note especially that *C*'s blurring is more pronounced in central vision.

SUMMARY

(1) The subject of this investigation manifests a unique anomaly of color perception, which is not simple color blindness, but a more central defect. Color names (with the exception of black and white) convey no meaning to the *S*, and in other respects he gives the appearance of total color blindness. Nevertheless, color matching tests demonstrate that he has color vision, inasmuch as he can distinguish chromatic colors as a group from whites, grays and blacks, although he is quite unable to match colors with respect to hue. Verbally he can differentiate the various chromatic colors only in respect of their "strikingness," a characteristic which seems to correspond more or less closely to the normal *S*'s concept of saturation. Yet his behavior in color-matching experiments suggests a rudimentary discrimination of hue—even though he can render no judgments on the identity or difference of colors with respect to this attribute.

(2) There are also peculiarities in this *S*'s perception of achromatic colors. Among these is an abnormal intensity of the *black* sensation. According to the *S*'s statement, *black* is for him the "strongest" or "most intense" of all visual experiences. It is his natural tendency to describe the brilliances of colors in a reversed terminology (*i.e.* using "brighter" in place of the normal person's "darker," and *vice versa*). The exceptional intensity of the physiological 'black' process is indicated not only by the *phenomenal* properties of black, but by its very positive *functional* properties.

(3) An ordered row of achromatic *surface colors* (neutral papers) is not perceived *as a series*. The *S* discriminates neighboring members of the row and identifies the extreme colors (black and white). Nevertheless the intermediate colors are almost devoid of perceived relationships to each other and to white and black. The word *gray* is one the *S* never uses and cannot understand.

(4) Achromatic *film colors,* on the other hand, can be readily compared by this *S* in respect of their *brilliance* or intensity. However, with such colors there is no perception of achromatic quality: the words *white* and *black* are not applied to film colors. This result confirms other lines of evidence which indicate that achromatic quality belongs primarily to surface colors, while brilliance is the predominant attribute of achromatic film colors.

(5) The *S* is unable to equate chromatic with achromatic colors with re-

spect of brilliance: the words *brilliance* or *intensity* convey no meaning to him in such a situation. On the other hand, when film colors are to be compared, he makes heterochromatic judgments of brilliance without difficulty.

(6) The *S* also shows a defect in the perception of contours and patterns. Contours, when steadily fixated, blur very rapidly and often disintegrate completely, in spite of an initially large difference in brilliance.

When a small white figure placed on a dark ground is fixated, the figure first changes its apparent form (*e.g.* a square appears to become a circle); later its contour becomes blurred, its surface texture becomes filmy, and all definite form is lost. Meanwhile, the color of the figure tends to approximate that of the ground, so that there is an effect which opposes that of simultaneous contrast. Moreover, the changes in form within a given figure seem to be affected by the mere proximity of a larger figure.

The effects are more pronounced in central than in peripheral vision. The changes in form, the blurring of contours, and the joining of figures are influenced in marked fashion by (a) the perception of these figures as members of larger coherent wholes, (b) the perception of figures as tridimensionally separated from their backgrounds, (c) a change in the mode of perception of ambiguous figure-ground patterns, and (d) the volitional attitude of the *S*. It would therefore appear that the phenomena are of cerebral rather than retinal origin.

(7) In addition to the visual anomalies there is a profound weakness of tonal perception.

THE TIME-ERROR AS A FUNCTION OF CONTINUED EXPERIMENTATION*

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Recent interest in the time-error centers around the work of Köhler[1] and the trace theory which he advanced in explanation of the over-estimation (negative time-error) or under-estimation (positive time-error) of the second of two stimulus-objects presented in the method of successive comparison. The phenomenon is observed in an asymmetry in the direction of a certain judgment-category which appears with a relatively greater frequency in a series of judgments. The usual experimental procedures encounter this asymmetry as it changes with the variation of the length of the time-interval between the successive presentations of the stimuli. Thus, when the time interval is the only experimental variable, it is possible to attribute observed differences in the asymmetry of judgments to the effect of the time elapsing between the two observations.

The term *p-function* of the time-error has been introduced to express the variation of the time-error, in both direction (or sign) and magnitude, with the variation of the time interval *(Pause)*.

Köhler's investigation resulted in the following generalization. When the time-interval between the two stimuli is brief (0-3 sec.), there appears a positive time-error. With an interval of approximately 3 sec. the time-error is minimal or absent. As the time-interval becomes longer (3-12 sec.) the time-error is increasingly negative. The theoretical discussion presented by Köhler considers the time-error in its negative phase as the effect of the "sinking" of a physiological trace of the first stimulus, which therefore is less effective in the dynamic process or transition from which the comparison judgment evolves. The variation of the time-error with the increase in the length of the interval is assumed to serve as the "beobachtete Schicksal der Spur."[2] With the very short intervals, the trace has an effective value greater than the objective value of the stimulus to which it corresponds; and later its effectiveness becomes less and less.

Köhler made an important qualification with respect to his experimental findings. Reporting the fact that his *Os* were all "naïve," or inexperienced in the psychophysical comparison of sound intensities, he noted further that he and his assistant, "Veteranen für Telephonknacke, . . . hatten für denselben Schallpaaren . . . den entgegensetzten Eindruck."[3] Briefly testing the hypothesis that there might

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* Accepted for publication October 31, 1933.

[1] W. Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psychol. Forsch.*, 4, 1923, 115-175.

[2] Köhler, *op. cit.*, 158.

[3] *Ibid.*, 163.

be marked practice effects upon the p-function of the time-error, Köhler repeated his experiment for three experimental hours and noted that the regularity implied in his generalization obtained only for the naïve *O* and that the curve of the p-function was changed markedly with "amassed experimentation." The results were, however, too meager to warrant serious theoretical consideration.[4]

The present study is an attempt to observe more fully any effects of prolonged repetition of experimentation upon the p-function of the time-error. Certain changes might be anticipated from Köhler's results. He suggested that the most probable effect would be a transposition of the extent of the positive time-error into the longer time-intervals, and a decrease in the magnitude of the negative time-error at the longest intervals.[5]

METHOD AND PROCEDURE

For the most part Köhler's conditions were retained. The stimuli were sound intensities, produced by an ordinary telegraph sounder placed in a circuit involving variable resistances. A telechron-driven contact arm actuated a make-relay which closed the telegraph sounder circuit for 0.2 sec.[6]

A series of 5 comparison stimuli was employed. These stimuli were of medium loudness and were separated by small steps equal in physical intensity. A standard stimulus was equal to the mid-point of the comparison series. The standard was always presented first, so that the time-error was in the first order. The instructions required a judgment upon the second stimulus as *stronger than, weaker than* or *equal to* the first with respect to intensity.

The determination of a p-function of the time-error under constant conditions required that each experimental hour involve the presentation of the comparison series an equal number of times with each of a number of time-intervals between the members of an experimental pair. In the present study 4 intervals were employed, 1, 2, 4, and 8 sec. Ten series (50 judgments) with each time-interval made up the experimental hour. The order in which the intervals were employed was varied haphazardly among the various *O*s and the successive experimental sittings.

The *O*s conformed to Köhler's criterion of naïveté: all reported that they had never made judgments on sound intensities. Further to satisfy this criterion no practice period was introduced prior to the recorded experiment. The only exception was the presentation of three pairs of stimuli in each part of the experiment to accustom the *O* to the rate of presentation.

The *O*s were divided into two groups, A and B. The groups were differentiated on the basis of the time which elapsed between successive experimental sittings. For Group A an average of 2.6 days (A.D. = 0.4) separated successive experimental sittings; for Group B the average was 7.3 days (A.D. = 0.2). The *O*s were: Group A: Mr. H. Odbert (*Od*), Mr. S. S. Stevens (*St*), and Mr. G. Crapo (*Cr*); in Group B were Mr. A. Wolbarsht (*Wo*), Miss B. White (*Wb*), and Mr. R. Sargent (*Sa*). *St* and *Od* were graduate students and *Wo* and *Wb* were undergraduates in the Harvard Psychological Laboratory; *Cr* and *Sa* were laymen without especial psychological interests.

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[4] *Ibid.*, 159.

[5] *Ibid.*, 161.

[6] The present experiment was conducted in the Harvard Psychological Laboratory in 1932-33.

The procedure for determining the time-error statistically (sign and amount) starts at the same point as does that for the determination of differential sensitivity. Judgments are made upon a series of stimuli, standard and comparison, and the customary categories of judgment, either two or three, are employed. Rather than centering in the calculation of limens, the interest lies in the relative numbers of judgments in the positive categories, *i.e. stronger* and *weaker*. Preponderance of the *stronger* judgments, when the comparison stimuli are arranged symmetrically above and below the standard stimulus, indicates an over-estimation of the second stimulus, *i.e.* a negative time-error; likewise a preponderance of the judgments *lighter* indicates a positive time-error. Köhler merely cited the frequencies of the various judgments; Lauenstein[7] employed a measure which he termed the D% (*Differenz-prozent*), the percentage of preponderance of the one or the other judgment-category. In the present study the function was found by dividing the *equal* judgments equally between the two positive categories and then calculating a point of subjective equality. The difference between this point of subjective equality and the objective value of the standard stimulus (the mid-point of the comparison series) was a suitable measure of the amount of the time-error, while the position of the point of subjective equality above or below the standard stimulus indicated the sign of the time-error (positive if above, negative if below). The correlation of this measure with those of Köhler and Lauenstein was nearly perfect.

Because of the small number of judgments involved in a single group of results in this experiment, the usual determinations of a point of subjective equality, as by the Urban method, were not applicable. The Spearman formulae proved a satisfactory substitute.[8]

RESULTS

(1) The first matter of interest in the present results concerns the agreement which they show with those of Köhler in regard to the curves of the p-function for the naïve *O*. Fig. 1 presents these curves for the 6 *O*s of the present experiment. The results for *Wo, Wh,* and *Sa* correspond reasonably well to Köhler's generalization. However, in the case of four *O*s (*Od, St, Cr* and *Wh*) the curves rise as the time-interval increases from 4 to 8 sec. Such a suggestion of a tendency to periodicity was not observed or considered by Köhler, although it is supported by a certain amount of historical evidence.[9]

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[7] O. Lauenstein, Ansatz zu einer physiologischen Theorie des Vergleichs und der Zeitfehler, *Psychol. Forsch.*, 17, 1932, 156.

[8] C. Spearman, The method of 'right and wrong cases' without Gauss' formula, *Brit. J. Psychol.*, 2, 1908, 227-242.

[9] Typical of investigators who have observed a cyclic asymmetry in comparable experiments are: H. K. Wolfe, Untersuchungen über das Tongedachtnis, *Philos. Stud.*, 3, 1886, 534-571; A. Lehmann, Kritische und experimentelle Studien über das Wiedererkennen, *ibid.*, 7, 1891, 169-212; G. M. Whipple, An analytic study of the memory image and the process of judgment in the discrimination of clangs and tones, this JOURNAL, 12, 1901, 409-457; 13, 1902, 219-268; Y. Wada, Der Zeitfehler beim Sukzessivvergleich der Tonhöhen, *Jap. Jour. Psychol.*, 7, 1932, 505-

A second point should be noted. The various curves of Fig. 1, while not strictly parallel, tend to lie at characteristic levels: *e.g.* those for *St* and *Wh*, or those for *Cr* and *Od*. As the time-error is relatively great in the positive direction at the short intervals, it tends to be less negative at the longer intervals than is the case where the positive error at the short intervals is less. This generalization appears to be more valid for the 4-sec. than for the 8-sec. interval, but this discrepancy probably is due to the inversions discussed in the preceding paragraph.

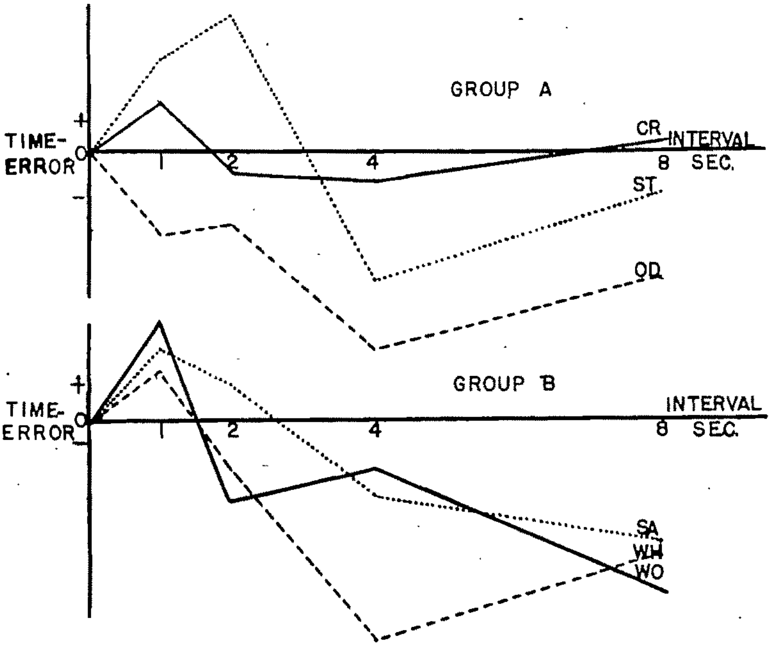

FIG. 1. SHOWING THE CURVES OF THE P-FUNCTION ON THE FIRST EXPERIMENTAL DAY

Köhler also suggested that the point of transition from positive to negative time-error would be at an interval of approximately 3 sec. He based this conclusion upon experimental results obtained with intervals of 1½, 3, 6, and 12 sec. The present results, for 4 of the 6 *O*s, involve a negative time-error at an interval of 2 sec.

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537. The last reports an experiment conducted along the lines suggested by Köhler and extends the time-interval to 59 sec., at which point the time-error became positive again.

It might be suggested that this result is consistent with Wada's suggestion that the selection of intervals may be itself a partial determinant of the curve of the p-function.[19] However, the individual differences in the present results are too extreme to make accurate generalizations.

(2) The lack of regularity which is apparent in the results involved in Fig. 1 is much less prevalent in the changes which took place with the repetition of the experiment. Fig. 2 indicates the nature of the various individual p-function curves after eight or nine complete determinations. Four of the curves, those for *Wo, Wh, Od,* and *Cr,* have shifted in such a manner that they are approximate inversions of the original curves or of Köhler's generalized curve.

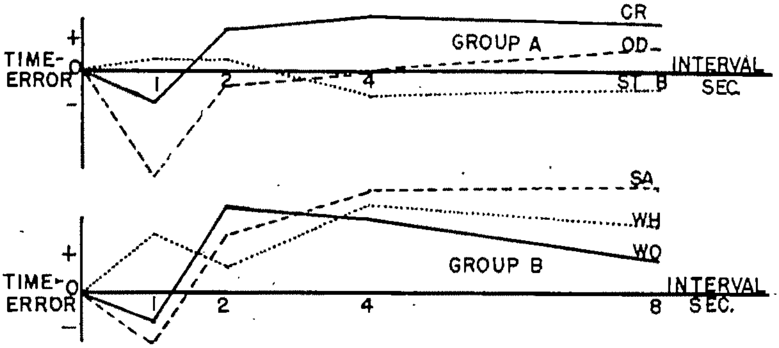

Fig. 2. Showing the Curves of the P-Function on the Last Experimental Day

Indeed, this development is but a continuation of the shift suggested by Köhler, although it has extended beyond the limits which he suggested and which are cited in interpretations of his work. At the shorter time-intervals, the time-error not only becomes less positive but continues, in the same direction, to become increasingly negative; at the long intervals, the time-error not only is less negative but also becomes positive.

The generalizations concerning the displacement must be in relative rather than absolute terms. For example, in the case of *St*, after the eighth repetition the time-error was still positive, although only slightly so, with the short intervals. But it must be taken into account that for this *O* the original time-error with the shorter intervals was relatively greater than for the other *O*s, so that the actual downward displacement is strictly comparable to that for the other *O*s, although the time-error did not become negative.

(3) The progressive comparison of p-functions for successive experimental days is too unwieldy a method of presentation of the displacement

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[19] Wada, *op. cit.*, 519 ff.

of the various time-errors with repetition of experimentation. Instead the time-errors for each of the four time-intervals have been plotted with respect to the progressive repetition. For each time-interval the time-error on successive days is represented on a scale in which the ordinate measures the amount and sign of the time-error, and the units of the abscissa are the successive experimental periods. The curves drawn in this manner are presented in Figs. 3 and 4, and in inspecting them, the original p-functions from which they evolve should be taken into consideration.

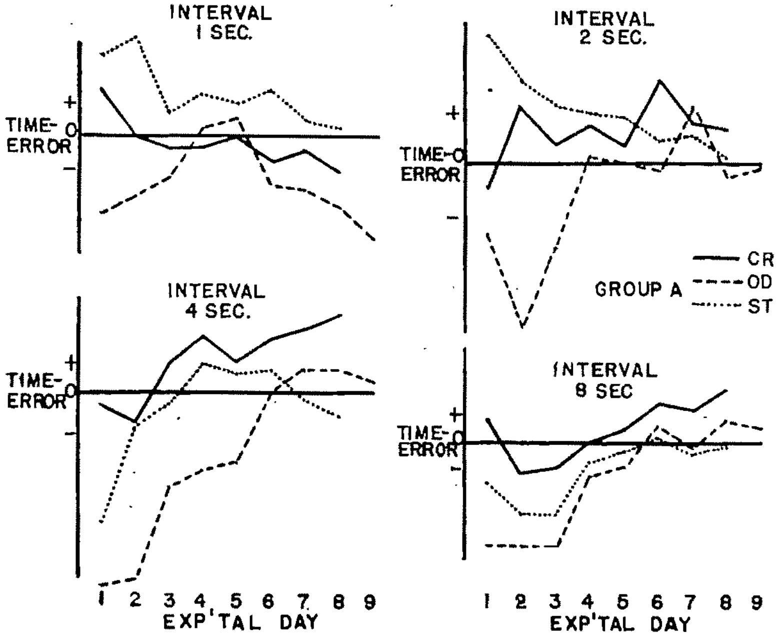

FIG. 3. SHOWING FOR GROUP A THE TIME-ERRORS AT FOUR INTERVALS WITH CONTINUED EXPERIMENTATION

These curves are distorted in the case of Group A (Fig. 3), especially for the 8-sec. interval, because of the fact that the first values are extracted from p-functions in which inversions appeared. Both Figs. 3 and 4 indicate great irregularity with respect to the 2-sec. interval; but these anomalies are less significant in the light of the wide individual differences in the original p-functions with respect to the sign of the time-error at the 2-sec. interval. Indeed, it is apparent that when the time-error for this interval is originally positive, the shift with repetition is downward (toward a negative error), whereas when the original error is negative the change is upward (toward a positive error). In other words, the time-error for the 2-sec. interval in its displacement with repetition corresponds in direction to that of either a longer (4-sec.) or shorter (1-sec.) interval, according as its original sign agrees with the one or the other.

Comparing the curves in Figs. 3 and 4 among themselves, it appears that in general the most extensive shifts with repetition take place at the extreme intervals (1- and 8-sec.).[11] The displacement is less with the 4-sec. interval, and least with the 2-sec. interval.

There are but two minor irregularities. In the case of *Wh*, for the 4-sec. interval, the time-error on the first day was exceptionally large in the negative direction, so that the repetition curve (Fig. 4) ascends too rapidly for the above generalization. In the case of *Od* (Fig. 3) with the 1-sec. interval the time-error was at first negative instead of positive. Here the effect of repetition was first an upward displacement to a point where the time-error was positive, and then a regular change in the direction of an increasingly positive error.

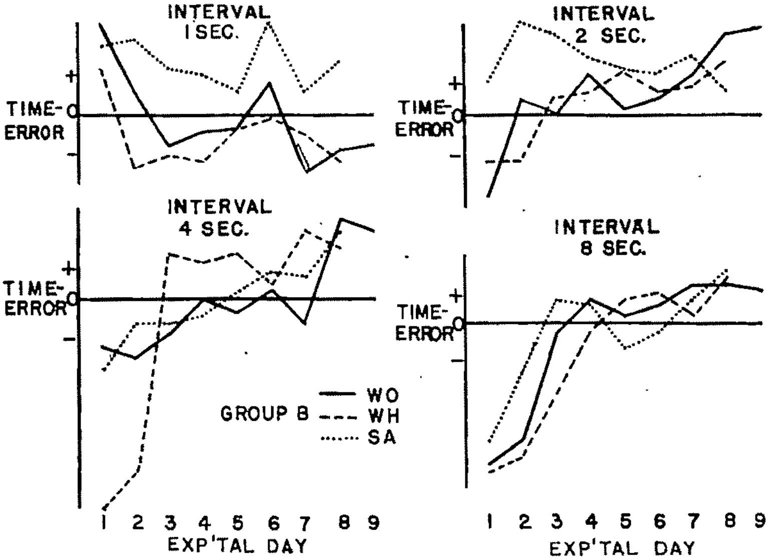

FIG. 4. SHOWING FOR GROUP B THE TIME-ERRORS AT FOUR INTERVALS WITH CONTINUED EXPERIMENTATION

(4) A comparison of Group A with Group B—the experimental distinction between the groups was based on the rate at which repetition progressed—indicates certain possible generalizations. Subject to the qualifications suggested above (especially with regard to the irregularities at the 8-sec. interval for Group A), Group A exhibits three characteristics as compared with Group B. (a) The several displacements of A are more

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[11] This statement must be qualified by the recognition of the distortion at the 8-sec. interval noted for Group A.

rapid than for B; they proceed a given distance in a smaller number of repetitions. This relationship appears especially in the earlier parts of the experiment. (b) The total extent of the displacement for A is greater than for B. (c) In the later stages of the experiment, with an equal amount of repetition for the two groups, there is exhibited for A less of a tendency toward further change than for B. The A-curves level off toward the close of the experiment much more than the curves for B.

(5) Minor experiments were carried out to study still further the nature of the displacement which appeared in the work just reported. A reversal of the order of presentation of the standard and comparison stimuli (so that the time-error was present in the second order), with new and equally naïve *Os* (Messrs. D. McGregor and B. Shevach, *Mc* and *Sh,* graduate students in the Harvard Psychological Laboratory) and a complete repetition of the experiment indicated, first, a general agreement with the original results in the matter of the form of the p-function at the beginning of the experiment, and secondly, a marked difference in the nature of the displacements of the time-errors at the different intervals. Fig. 5 represents the results obtained. It should be noted that with both the 4-sec. and 8-sec. intervals the displacement upward appeared at a noticeably later stage in the experiment than was the case when the time-error was present in the first order.

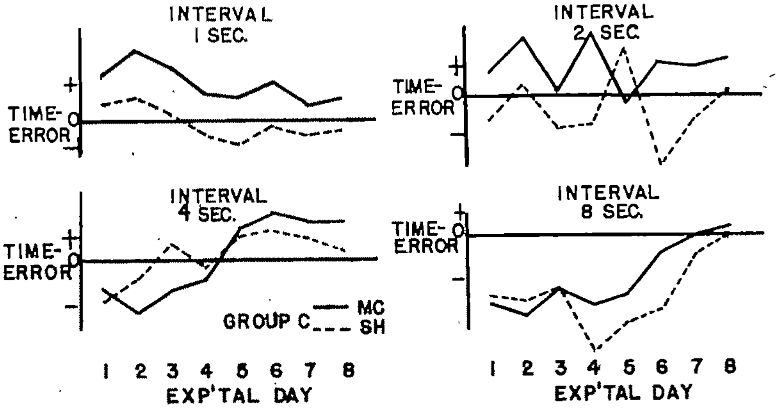

Fig. 5. Showing for Group C the Time-Errors at Four Intervals with Continued Experimentation

An attempt was made to test the hypothesis that the displacement which has been reported indicates a progressive shift of some general 'level.' Two of the *Os* in the main experiment, *Od* and *St,* were recalled about one month after they had completed their regular observation, and the usual determination of the p-function of the time-error was made. Each of the 8 time-errors (4 time-intervals for each of two *O*s) which were thus determined had a value intermediate between those of the first and last experimental sessions for the corresponding time-interval and *O*; *i.e.* during a period of no experimentation the p-function had tended to return toward its original form.

DISCUSSION

Lauenstein's hypothesis of the assimilation of physiological traces as the cause of the time-error[12] has been criticized by Pratt[13] on the grounds that the experimental evidence cited concerns only a limited situation; a suitable hypothesis must be more widely applicable. The present results apply to Köhler's hypothesis of a sinking trace in the same manner. In order to explain the shift which he observed with repetition, Köhler proposed a subsidiary hypothesis of "sediment"[14] to explain the decrease in the amount of the various time-errors. However, a sediment-building to such an extent that the time-error not only decreases but also changes its sign, so that the curve which represents the 'fate of the trace' is exactly reversed, is not consistent with the trace theory itself.

The correspondence of the curves in Figs. 3 and 4 to various learning curves seems a reasonable basis for the suggestion that, until the comparison judgment itself is understood more adequately, the most profitable hypothesis-building may be in terms of perceptual rather than physiological processes. Results such as those of the present experiment indicate that physiological hypotheses of the time-error are over-simplified. The possibility of periodicity in the curve of the p-function and the operation of the various factors which modify the displacement, such as rate of repetition and order of presentation, suggest that there are more conditions involved in the time-error than can be explained in terms of a single physiological trace or of a process of assimilation between two adjacent brain areas.

Protocols were obtained from the *O*s, and these indicate that the judgment was phenomenally different at the different time-intervals; there are different perceptual patterns as the interval is different. It might be said that the p-function of the time-error, as seen in the present work, included four typically different judgments. At a certain theoretical optimal time-interval, the time-error is least; as the time-interval is shortened or lengthened, the perceptual organization on which the judgment is based is altered, and a characteristic error appears in a series of such judgments. Moreover, the effect of repetition is a modification of each typical pattern or organization; and this modification is greatest at the extreme intervals, where the time-error is also greatest originally, and is least near the optimal point. Thus the p-function suggests the need for the consideration of a temporal dimension of variation of the perceptual organization such that a change in this dimension would lead to a rearrangement of the pattern and a characteristically different result would appear.[15]

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[12] O. Lauenstein, *op. cit.*, esp. 147 ff.

[13] C. C. Pratt, The time-error in psychophysical judgments, this JOURNAL, 45, 1933, 294.

[14] Köhler, *op. cit.*, 161 ff.

Such a hypothesis implies the revision rather than the discard of physiological assumptions. For example, the 'sinking trace' and the 'assimilation of traces' may represent neural correlates of different phenomenal organizations. It is probable that still other physiological processes have a similar status. A certain perceptual pattern has, as a fundamental characteristic, certain corresponding physiological events. An example of the manner in which this principle may operate is implied in the following statement: "When the background or the intermediate stimulus is considerably less than the standard, the process of assimilation is in the direction producing preponderance in the direction of increase. When, however, there is no intermediate stimulus, the trace merely sinks."[16] It is quite possible that, within a limited range, different physiological processes may function for different patterns of phenomenal organization. Since the total situations are many and varied and their determinants equally numerous, a complete hypothesis along the lines suggested here is impossible at present; we must await a more sufficient knowledge of the comparison judgment than is now available.

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[15] For a treatment of the manner in which the perceptual pattern involved in the comparison judgment may change with "progressive practice," see S. W. Fernberger, An introspective analysis of the process of comparing, *Psychol. Monog.*, 26, 1919, (no. 117), 1-161.

[16] Pratt, *op. cit.*, 297.

THE RÔLES OF THE SITUATION AND ADRENALIN IN THE INDUCTION OF EMOTION

By HADLEY CANTRIL, Harvard University

Since Marañon reported the subjective effects of the injection of adrenalin[1] there has been considerable discussion and some experimentation regarding the relationship of the adrenalin syndrome to the problem of emotions. The majority of Marañon's *O*s reported "cold" emotions or "as if" feelings, although a few *O*s experienced genuine emotions which Marañon accounted for by predispositions based on associative content. Some authors[2] have accepted his interpretation, but recent experiments[3] seem to indicate that the emotions occasionally elicited by the adrenalin syndrome are due primarily to the attitude of the *O*. In general, however, these studies support Marañon's finding that a "cold" emotion is usually aroused by the adrenalin syndrome.

Landis and Hunt[4] have pointed out that adrenalin "is by no means as perfect an instrument as the Spanish school has enthusiastically proclaimed," and yet its dependability in the production of certain organic-kinaesthetic reactions and the similarity of these reactions to those aroused by the sympathetic division of the autonomic nervous system[5] warrant the continued use of the drug in various phases of experimentation on emotion.

The present investigation was undertaken for two reasons. Since the injection of adrenalin seems to arouse the autonomic reaction thought to be characteristic of emotions, a revealing comparison might be made between the emotional experiences aroused when the same emotional stimulus situation was presented to *O*s both with and without the adrenalin syndrome. The second purpose of the study was to discover whether or not there are any *affective dimensions* common to various emotions. If any such components

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[1] G. Marañon, Contribution à l'étude de l'action émotive de l'adrénaline, *Rev. fr. d'endocrinol.*, 2, 1934, 301-325.

[2] Cf. R. S. Woodworth, *Psychology*, rev. ed., 1929, 308; P. Bard, The neuro-humoral basis of emotional reactions, *Foundations of Experimental Psychology*, 1929, 480 f.

[3] Cf. H. Cantril and W. A. Hunt, Emotional effects produced by the injection of adrenalin, this JOURNAL, 44, 1932, 300-307; C. Landis and W. A. Hunt, Adrenalin and emotion, *Psychol. Rev.*, 39, 1932, 467-485.

[4] Landis and Hunt, *loc. cit.*

[5] W. B. Cannon, *Bodily Changes in Pain, Hunger, Fear and Rage*, rev. ed., 1929, 34-36.

are involved in different emotions, then we might find a typical pattern of affective dimensions for each type of emotional experience and thereby obtain more specific information concerning emotional quality.[6] Eight affective dimensions were selected for study: *bright and dull pressure, tension-relaxation,* the feeling of *warmth or cold,* localization of reactions as *high or low,* reactions as *dense or spread, excitement-depression, pleasantness-unpleasantness,* and the duration of the reactions designated by *fleeting-lingering.*

TECHNIQUE

Twelve *Os*, 10 men and 2 women, individually participated in the 4 situations involved in the experiment. No *O* had any knowledge of the relation of adrenalin to emotions. All *Os* were pledged to secrecy. The experimental sessions were held in the evening in the Harvard Psychological Laboratory. In each situation, stimuli were presented which were meant to arouse an emotion in the *O*. Situations 1 and 3 were planned to create *disgust*;[7] situations 2 and 4 to elicit *fear*. In *Situation 1* five extremely distasteful photographs of mutilated war victims[8] were successively presented to the *O*. At the beginning of *Situation 2* the experimenters (designated as *E1* and *E2*[9]) were out of the experimental room. *E1* turned off all the lights in the laboratory by means of a control switch and *E2* then ran a distance of about 20 ft. down the hall to the experimental room and pounded vigorously on the door for about 2 sec., shrieking all the while. The lights were then turned on and *E2* went immediately into the room. In *Situation 3* 5 photographs similar to those in Situation 1 were employed. In *Situation 4* a loudspeaker, which was connected to a beat-frequency oscillator controlled by *E1* in another room, was concealed behind the *O*. Simultaneously the lights were turned off and the oscillator turned on and operated for about 3 sec., producing a siren-like effect. All situations were rotated in such a way that each situation was presented first to three *Os*, *viz.* 1-2-3-4, 2-3-4-1, 3-4-1-2, 4-1-2-3.

In the first three sessions, each *O* was given a 1.5 cc. injection of 2¼ grains of chloretone in one ounce of physiological salt solution.[10] This injection produces the same smarting effect as adrenalin. In the fourth session, the *O* was given a 1.5 cc. injection of 1-1000 solution of Parke, Davis Adrenalin Chloride. Since the adrenalin syndrome is always quite marked, the control solution was employed in the first three sessions to insure complete naïveté of the *O*. This rotation of the situations and the administration of adrenalin in the fourth session provided three *O*s with the adrenalin syndrome in each of the four situations. Six of the *O*s also took part in a fifth experimental session in which they were simply given an injection of adrenalin chloride.

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[6] Dunlap has recently criticized experimental work on emotions because of its concern with separate emotions rather than with any basic "affective elements" which might be discovered for all emotions (cf. K. Dunlap, Are emotions teleological constructs? This JOURNAL, 44, 1932, 572-576). Although the present investigation was conceived before the publication of Dunlap's paper, the procedure employed seems to satisfy this criticsm to some extent. The writer wishes to thank Dr. J. G. Beebe-Center for suggesting some of the affective dimensions studied in this experiment.

[7] The psychologist's acceptance of "disgust" as an emotion goes back at least to William James (cf. his *Principles of Psychology,* II, 1890, 445).

[8] E. Friedrich, *Krieg dem Kriege,* 1926.

[9] The writer wishes to thank Mr. C. M. Schloss for his valuable assistance in conducting the experiment and in analyzing the material. Mr. Schloss is referred to as *E2,* the writer as *E1.*

[10] This control solution was used by Landis and Hunt, *op. cit.,* 473.

Before each session an attempt was made to determine whether or not the *O* was emotionally predisposed. The following questions were asked:[11]

> Do you at present feel any disposition toward any particular emotional state?
> Has anything (either physically or mentally) within the last few days or quite recently happened to disturb your normal state?
> Can you recall any dreams that you have had in the past few nights?

If the answers to the first two questions were negative and if the third question failed to disclose any disturbing dream content, the experiment proceeded. The *O* was then seated in a comfortable arm chair, bright and dull pressures were described to him both objectively and subjectively by *E2*,[12] and after a 5 min. rest period the injection was given and the following instructions presented:

> You have been given an injection of adrenalin. You are now asked to report orally to the experimenter and as fully as possible any physical or mental symptoms which develop. Do not omit anything, no matter how irrelevant it may seem to you. Try not to become impatient and try to keep your mind free from any material not called up by the actual situation.

E1 (who had given the injection) then left the room.

In the first three sessions, the stimulus was then presented to the *O* 10 min. after the injection had been given. In the fourth session, the stimulus was not presented until *E2* felt the adrenalin syndrome had reached its maximum intensity. This could be judged both from the introspections and from objective signs such as paleness and trembling of the limbs. The interval in the fourth experimental session was usually about 18 min. No *O* seemed to notice the temporal discrepancy. Situations 1 and 3 were presented by *E2* who handed the *O* the heretofore concealed pictures with the comment: "Look at each picture thoroughly and as long as you care to and then report to me any reactions which you have." Before Situations 2 and 4 could be given, *E2* had to leave the experimental room on some pretext.

After presentation of the stimulus, *E2* made a verbatim record of the introspections and then, if the *O* had not already reported some emotion, asked if any emotion had been experienced, and, if so, what emotion it was, and how intense it was. Intensity was rated on a 5-to-0 scale, 5 designating "extremely intense." The *O* was then asked to report on each of the variables mentioned above. Those *O*s who participated in the fifth session made a similar report.

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[11] The questions asked the *O*s in the present experiment, as well as the directions given after the injection, were identical to those used by Cantril and Hunt, *op. cit.*, 302.

[12] The subjective descriptions of bright and dull pressures were taken from Hunt (cf. The relation of bright and dull pressure to affectivity, this JOURNAL, 43, 1931, 89); while the objective illustrations of these pressures consisted in asking the *O* to note the differences in the experiences when his arm was tickled and when it was firmly pressed.

RESULTS

(1) Intensity of emotional experiences with and without adrenalin. This comparison was obtained by averaging the *Os*' ratings on the intensity of the emotions aroused in the first three sessions and comparing the average to the average intensity of the emotional experiences aroused in session 4. The averages are as follows:

	Without adrenalin	*With adrenalin*
Situations 1 and 3	1.01	0.65
Situations 2 and 4	2.20	3.40

Thus it appears that, on the whole, the intensity of emotions was considerably increased in the situations for fear when the adrenalin syndrome was present, while in the situations for disgust the emotional experience was slightly less intense with the syndrome.

TABLE I

SHOWING THE RESULTS WITHOUT ADRENALIN

(The figures represent the percentage of total number of reports possible.)

		Affective dimensions															
Situation	No. cases	Bright	Dull	Tension	Relaxation	Warm	Cold	High	Low	Dense	Spread	Excitement	Depression	Pleasant	Unpleasant	Fleeting	Lingering
1 (disgust)	9	0	44	66	0	22	0	0	44	0	44	0	44	0	66	66	0
3 (disgust)	9	0	0	22	0	33	0	0	22	0	22	0	33	0	66	55	0
2 (fear)	9	77	0	66	0	33	0	44	0	55	0	77	0	0	66	100	0
4 (fear)	9	55	0	77	0	0	22	44	0	33	22	88	0	0	33	88	0

(2) Comparison of reports on affective dimensions without adrenalin. An analysis of the affective dimensions reported in the two types of situations and *without* the operation of the adrenalin syndrome will show any distinct differences in the dimensions involved in situations for disgust and fear. Table I shows the results obtained in all cases without adrenalin.

Particularly significant is the characteristic pattern of affective dimensions found in Situations 1 and 3 (disgust) and the other consistent phenomenological pattern found in Situations 2 and 4 (fear). Although the large number of cases of "no report" qualifies the results to some extent,[18] still the consistent predominance of some of the dimensions in the two types of situations seems sufficient to be predictable. In terms of these dimensions and as aroused by our particular stimuli, we may conclude that a *disgust* situation generally evokes *depression* and *dull pressures spread out* in the *lower* somatic regions, while *fear* characteristically involves *excitement* and *bright pressures* fairly well *localized* in the *upper* regions of the body.

(3) Comparison of emotional states with and without adrenalin. Table II represents a summary of the phenomenological differences which were reported when the adrenalin syndrome was present and when it was lacking. Although the differences of these percentages is often not large, still in each type of situation a few changes will be seen.

In the *disgust* situations we note: (a) With adrenalin 33% of the reports indicate *excitement* and none indicates *depression,* whereas the same situations without adrenalin yielded reports of *depression* in 39% of the cases and no reports of *excitement.* (b) In the situations where the adrenalin syndrome was present, the organic-kinaesthetic reactions noted by the *O*s persisted for a considerable time after the situation had been presented, although in the majority of cases no genuine emotions were reported. In the same situations without adrenalin the reactions observed were in all cases of very short duration.

In the *fear* situations: (a) With adrenalin all *O*s who were able to report on the dimension of *warmth-cold* noted a "cold" reaction, whereas without adrenalin "cold" was reported by only 11% and "warm" by 22%. (b) As in the disgust situations, the bodily reactions observed tended to linger after the presentation of the fear situations when the adrenalin syndrome was present. (c) With adrenalin, all *O*s reported that the reaction was *unpleasant,* whereas only 44% reported *unpleasantness* without the syndrome. Introspections show that this unpleasantness is due (1) to the increased intensity of the emotional experiences in these situations when the syndrome is present, and (2) to the longer duration of the effects.

(4) Comparison of reports with adrenalin with and without situations. From Table III we see that: (a) bright pressures, tension, excitement, unpleasantness, and high, spread, lingering reactions are the characteristic experiences reported with the adrenalin syndrome; and (b) no genuine

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[18] The cases of "no report" were due primarily (1) to the fact that in some cases the *situation* demanded all the *O*'s attention and thereby overwhelmed any awareness of bodily reactions, or (2) to the highly critical attitude of a few *O*s who found the laboratory situations too artificial in some cases. It is suggested that a larger percentage of reports might be obtained if similar experiments were confined to a *single* dimension at a time.

TABLE II

Comparison of Results with and without Adrenalin

(The figures represent the percentage of total number of reports possible.)

				Affective dimensions															
Situation	No. cases	Emotion reported	No. times	Bright	Dull	Tension	Relaxation	Warm	Cold	High	Low	Dense	Spread	Excitemen	Depression	Pleasant	Unpleasant	Fleeting	Lingering
Disgust without adrenalin	18	disgust	6	0	22	44	0	28	0	11	22	0	33	0	39	11	66	66	0
		repulsion	2																
		distaste	1																
		pity	1																
		none	8																
with adrenalin	6	disgust	1	0	17	66	0	33	17	33	17	0	50	33	0	0	66	0	66
		none	5																
Fear without adrenalin	18	fear	6	72	0	78	0	22	11	44	0	44	17	88	0	0	44	100	0
		startle	5																
		fright	4																
		surprise	1																
		exasperation	1																
with adrenalin	6	fear	2	33	17	100	0	0	50	66	0	0	50	83	0	0	100	0	83
		startle	2																
		fright	1																
		none	1																

TABLE III

COMPARISON OF RESULTS WHEN ADRENALIN WAS USED WITH AND WITHOUT SITUATIONS

(The figures represent the percentage of total number of reports possible.)

	No. cases	No. emotions reported	Affective dimensions															
			Bright	Dull	Tension	Relaxation	Warm	Cold	High	Low	Dense	Spread	Excitement	Depression	Pleasant	Unpleasant	Fleeting	Lingering
With *disgust* situations	6	1	0	17	66	0	33	17	33	17	0	50	33	0	0	66	0	66
With *fear* situations	6	5	33	17	100	0	0	50	66	0	0	50	83	0	0	100	0	83
Without situations (session 5)	6	0	50	0	50	0	33	17	33	17	33	67	33	0	0	50	0	100

emotion was reported in any case where the adrenalin syndrome alone was present.

(5) Relationship of unpleasantness, bright-dull pressures, and localization as high or low. Nafe has suggested[14] that pleasantness is dependent on bright pressures and unpleasantness on dull pressures, while Hunt[15] has found high correlations between bright pressures and pleasant emotions and between dull pressures and unpleasant emotions. In our reports we notice, however, no such relation between pressures and unpleasantness. Thus *unpleasantness* was reported in 63% of the 54 reports obtained in the investigation, while *bright pressures* were reported in 32% and *dull pressures* in 11%.[16]

Both Hunt[17] and Nafe[18] have also suggested that bright pressures are localized in the thorax while dull pressures are concentrated in the abdomen. It can be seen in Tables I and II that on the whole our results support this generalization. Our introspections show, however, that pressures are not localized exclusively in the trunk, Nafe and Hunt, like James and Cannon, seem to have emphasized visceral sensations at the expense of kinaesthetic reactions. In this experiment sensations were almost as often reported in the extremities as in the viscera or chest. This is particularly true of the localization of bright pressures as illustrated by the following introspections:

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[14] J. P. Nafe, An experimental study of the affective qualities, this JOURNAL, 35, 1924, 507-544.

[15] W. A. Hunt, The pressure correlate of emotion, this JOURNAL, 43, 1931, 600-605.

[16] The occurrence of bright pressures with fear in the present experiment and Hunt's data showing dull pressures consistently reported with fear are not quite as paradoxical as might seem. Hunt noted (cf. *ibid.*) that four *O*s (or one-third of his total number) "reported something like an instantaneous bright pressure" accompanied by no affective or emotional concomitants immediately following the situation (revolver shot) and that "it was followed at once by the usual dull pressure accompanied by fear and unpleasantness." In the present experiment the *O*s were asked to report *immediately* after the Situation had been presented, and it seems not unlikely that more of Hunt's *O*s would have reported bright pressures after the shot if introspections had been taken immediately after presentation of the stimulus. Hunt's *O*s, however, reported a consequent dull pressure, while in the present experiment no such shifts in pressures were noted. The bright pressures aroused by Situations 2 and 4 persisted with adrenalin.

[17] W. A. Hunt, Localization of bright and dull pressure, this JOURNAL, 44, 1932, 308 ff. In discussing these results with the writer, Dr. Hunt has suggested that the "bright" and "dull" pressures reported by the *O*s in the present experiment were not the same as the pressures studied in his own investigations, because of the differences in localization and the possible qualitative changes in the pressure experiences when localized in different parts of the body. Hunt's experiments were confined to pressure experiences in the trunk, while the *O*s in our study experienced "brightness" and "dullness" in the limbs and in the head region as well as in the trunk. The reports of pressures outside the trunk were probably obtained in the present experiment because the objective definition of "bright" and "dull" pressures supplemented the subjective definition used by Hunt. "Brightness" and "dullness" appear, then, to be possible characteristics of experience in various parts of the body and are only localized in the trunk when the *O* is so instructed. Both types of pressure experience seem experimentally useful but the differentiation should be noted in the interpretation of the present findings.

[18] J. P. Nafe, The psychology of felt experience, this JOURNAL, 39, 1927, 367-389.

"Tingling in arms and legs only" (*O* 9, Sit. 2).
"Feel nothing in viscera or chest, only in thighs and arms" (*O* 2, Sit. 4).
"I feel cold and pain in my wrists and elbows, nothing in viscera" (*O* 12, Sit. 5).
"Excitement primarily in wrists" (*O* 12, Sit. 4).

During the experimental sessions, the *E*s noticed that in almost every case the report on bright or dull pressures, if not spontaneous, was quickly given on questioning; whereas judgment of pleasantness or unpleasantness was *never* spontaneous and questioning by *E2* was followed by hesitation before an answer. Many introspections illustrate this delay. The following are typical:

"I really can't say the feeling is pleasant or unpleasant. That sort of thing (P-U) doesn't stand out readily anyway. You have to sort of turn your attention to it and decide. I suppose when you have a genuine emotion the whole situation, especially the intellectual part of it provides the unpleasantness. But these bodily feelings, while they would probably be recognized as pleasant or unpleasant if you carried them around all the time, don't speak out and tell you of the unpleasantness when they come and go so quickly" (*O* 1).

"It was not pleasant, but not particularly unpleasant. It didn't occur to me as either" (*O* 2).

"Let me see. Well, I guess it's unpleasant" (*O* 5).

(6) Differences in the quality of the emotions aroused by the same stimuli. Table II shows that the same stimulus aroused different emotions in the various *O*s. This differentiation in the naming of the emotional experience appears from the introspections to be due to the *O*'s attitude toward the stimulus. In Situations 1 and 3, for example, *O* 8 was completely objective and kept repeating, "Isn't that too bad?" *O* 12 (a mother) named her emotional experience "pity;" *O* 1 got "disgust" and said the pictures reminded him of syphilitics; *O* 5 found the situations pleasant and remarked, "It is rather my nature to like such things." In the fear situations, *O* 12 was careful to point out that she was "startled" and not "afraid" because the element of worry was absent. *O* 1 had an aesthetic attitude in these situations and reported "exasperation" in Situation 4 because the whistling noise of the oscillator hurt his ears.

INTERPRETATIONS AND CONCLUSIONS

(1) Analysis of the emotional experiences aroused in our *O*s by the two types of stimuli shows that *there is a different pattern of affective dimensions for fear and disgust.* The variations noted between the organic-

kinaesthetic disturbances concomitant with fear and disgust, suggest that the physiology of *different types* of emotions should be made more explicit if the rôle of the autonomic nervous system in the development of emotions is to be known.[19]

(2) The adrenalin syndrome is more typical of the bodily reactions in fear than in disgust. This interpretation is supported by several of our experimental findings. (a) Reports on the affective dimensions in session 5 (with adrenalin and without stimulus) were more characteristic of the *fear* reaction than of the *disgust* reaction. (b) The adrenalin syndrome increased the intensity of the emotional experience in the fear situations but not in the disgust situations. (c) The introspections of the *O*s who reported a 'cold' emotion in session 5 show that the feeling was always 'as if' they could be or had been frightened or angered and not 'as if' they could be or had been disgusted.[20] Typical illustrations are: "Heart beating fast—the reaction one gets after being frightened" (*O* 7). "Feel as though I had just come out of a fight where I had hit hard" (*O* 3). "A kind of unpleasant exhilaration like one gets after a pseudo-fright" (*O* 4).

(3) The sensory concomitants of the emotional experience are secondary to the experience conceptualized as a whole. In relation to the concepts of pleasantness and unpleasantness, the results have shown that *fear* situations arouse bright pressures and unpleasantness[21] and that reports on the

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[19] Cannon has stated that the bodily reactions following the injections of adrenalin are identical to those aroused by the sympathetic division of the autonomic nervous system (cf. *op. cit.*, 34-36). He has also pointed out that "both from anatomical and physiological evidence the neurone relations in the sympathetic division of the autonomic system seem devised for *wide-spread diffusion* of nervous impulses" (p. 26). These two contentions have been confirmed by Bard (cf. *op. cit.*). However, the particular bodily changes noted by Cannon were found specifically in *pain, hunger, fear,* and *rage* and any interpretations of the relation between the sympathetic division and emotion should not assume that these *same* bodily changes were found in *all* types of emotional behavior. Cannon is careful to state that "the dominant emotions which we have been considering as characteristically expressed in the sympathetic division of the autonomic system are fear and rage" (p. 341), and he indicates that "so far as these two quite different emotions are concerned, present physiological evidence indicates that differences in visceral accompaniments are not noteworthy" (p. 343). Although Cannon continues by noting that "the machinery of this division (sympathetic) likewise is operated wholly or partially in emotions which are usually mild—such as joy and sorrow and disgust—*when they become sufficiently intense*" (p. 343), no rigid demarcation is made between the reactions involved in "disgust," "joy," and "fear."

[20] This is also confirmed by the introspections obtained by Cantril and Hunt, *op. cit.*

[21] The disagreement of this relation of bright pressures to unpleasantness with laboratory demonstrations that fear is accompanied by dull pressures and that dull pressures are concomitant with unpleasantness (cf. Hunt, The pressure correlates of emotion, *op. cit.*) may perhaps best be accounted for by noting (a) the quality of the stimulus situations used to arouse fear in the present experiment and (b) the importance of the *O*'s introspective attitude in the report of pleasantness and

affective concomitant of emotional experiences are not spontaneous but due to a rather forced *Einstellung* by the *O*.[22] If we consider pleasantness and unpleasantness as aroused in the usual laboratory studies on affectivity (with odors, colors, etc.), we may agree with Hunt[23] that bright and dull pressures accompany pleasantness and unpleasantness, that they can be regarded as the "sensory core of affectivity," and that "under a sensory attitude of report the presence of these pressures sets off the verbal response of 'pleasantness' or 'unpleasantness'." In more intense *emotional* situations, however, the experience aroused becomes so conceptualized about the stimulus that little attention is given to any sensory content. In many of our experimental sessions, the emotional experience was so focused on the stimulus situation that the *O*s were totally unable to report on any of the affective dimensions. When the *O* becomes set to report on the hedonic tone of the experience, the *stimulus situation* itself provides the "core" in the conceptualization of "pleasantness" and "unpleasantness," and, as Köhler would point out, the process becomes "determined by the actual process in the whole field."[24] Some of our *O*s reported that the *situation* was distinctly unpleasant, although they felt a "pleasant" tingle in the arms, neck, or chest *if* they shifted their attention to it. Thus the potentially intrinsic pleasantness of the bright pressures aroused in the *fear* situations was completely overwhelmed and disregarded because of the inevitable attention-demanding-nature of the "unpleasant" *stimulus*.

(4) The awareness of some object or situation around which the emotion is intellectually organized is the immediate cause of the emotional experiences as reported by our *O*s. Cantril and Hunt have suggested that "the usual emotional state" involves a primary autonomic reaction and an intellectual content.[25] The genuine emotions elicited by the injection of adrenalin in the experiments of Marañon, Landis and Hunt, and Cantril and Hunt may be due to the fact that the autonomic reactions themselves were conceptualized by the *O*s as emotional *situations*.

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unpleasantness. Both *fear* situations employed in the experiment might be described as "bright" rather than "dull," in that shrill noises were used in both. Thus from the standpoint of *Gestalttheorie* a "bright" phenomenological report would be necessarily anticipated since a "bright" sensory field would arouse a "bright" interoceptive pattern (cf. W. Köhler, *Gestalt Psychology*, 1929, 264).

[22] The relation of attitude to hedonic tone and its pressure correlates has been pointed out by E. F. Wells (cf. The effect of attitude upon feeling, this JOURNAL, 42, 1930, 573-580) and P. T. Young (cf. Studies in affective psychology, *ibid.*, 38, 1927, 175-185).

[23] W. A. Hunt, The meaning of pleasantness and unpleasantness, *ibid.*, 45, 1933, 345-348.

[24] Köhler, *op. cit.*, 193.

[25] Cantril and Hunt, *op. cit.*

(5) The quality of an emotion is primarily dependent upon the attitude aroused in the O by the stimulus. It may be implied from the introspections that the *O*'s attitude is partially due to his experiential background. It is also suggested that this attitude is somewhat determined by characteristics of temperament.[26]

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[26] In order to determine whether or not there was any relation between characteristics of temperament and emotional experience, the *Clark Revision* of the *Thurstone Neurotic Inventory* was given to each *O* and the scores were correlated (product-moments) with the average intensity of the emotional experience for each *O*. A coefficient of + 0.822 ± .068 was obtained. This dimension of temperament might be called "strength."

STIMULUS-PRESSURE AND THERMAL SENSATION

By F. HEISER and W. K. MCNAIR, Yale University

The problem of the present study is to determine the effect of varying the stimulus-pressure upon the temperature sensations elicited from human skin. Heiser found that increasing the stimulus-temperature, while other factors of stimulation were held constant, increased the number of temperature sensations within a given area of skin, and believes that the various factors of stimulation, such as pressure, duration, size and quality of stimulus, interval between stimulations, location of area of skin tested and air and skin-temperature each have an independent effect upon temperature sensitivity.[1]

Apparatus. The apparatus used is shown in Fig. 1, and is an improvement of the apparatus used by Heiser.[2] It consists of an insulated brass stimulator, tapered to a point at one end, and fastened to the end of a fiber rod, 11 in. long, which has pivot points about halfway between the two ends. The stimulator is counterbalanced by a brass weight which slides on the other end of the fiber rod, thus varying the weight or pressure of the stimulus between 1 and 50 grm. It is heated to the desired temperature by a 110-volt current brought through the variable resistance of a rheostat to the steel pivot points on the fiber rod from which small wires run along the rod to a Nichrome wire heating coil contained in it. A small thermometer is inserted into the side wall with its bulb ¼ in. from the stimulus point. The thermometer accurately indicates the temperature of the stimulus point. The stimulus temperature may be varied by changing the amperage in the heating coil by means of the variable resistance.

The fiber rod, holding the stimulator, is pivoted to two fiber and brass arms, extending beneath a brass stage which is moved horizontally by means of a threaded rod or screw. This stage is fastened beneath another stage moving horizontally and at right angles to the first stage by means of another screw. Using these movable stages the stimulator may be applied to any desired point upon the skin. One turn of either of the stage screws moves it 1.1 mm., the diameter of the stimulus point. The two movable stages are mounted beneath a frame which moves in a vertical direction to allow for unevenness in the surface of the skin stimulated.

The stimulator was held up away from the skin by a magnet fastened to a brass arm extending from the same stage which carried the stimulator. The magnet was activated by current from a 6-volt storage battery and was in series with a roller contact switch which was turned off and on by the swing of a counterbalanced

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[1] F. Heiser. Stimulus temperature and thermal sensation, *Arch. Psychol.*, 21, 1932 (no. 138), 40 ff.

[2] *Op. cit.*, pp. 28-31.

pendulum. The pendulum was released by *E* and on its forward swing broke the contact in the roller-switch and the current in the magnet, thus allowing the stimulator to fall 3 mm. to the skin. On the return swing of the pendulum the roller contact and current in the magnet were turned on and the magnet lifted the stimulator from the skin.

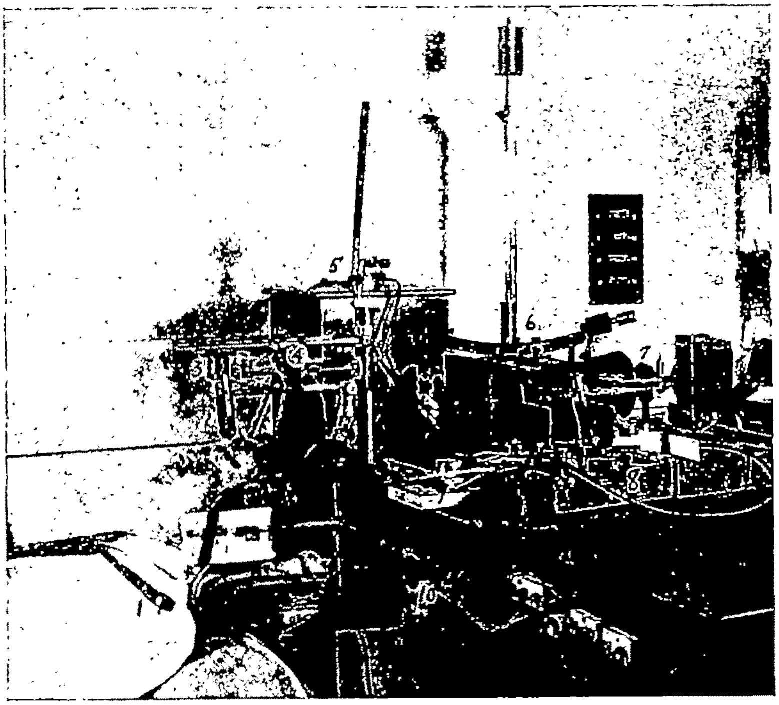

FIG. 1. SHOWING APPARATUS USED IN CUTANEOUS TEMPERATURE-STIMULATION

(1) 'Hot' junction of skin-temperature thermocouple; (2) Stimulator; (3) Magnet for dropping stimulator and lifting it away from the skin; (4) Screw-wheel for moving stimulator-stage; (5) Thermostat for 'cold' junction of skin-temperature thermocouple; (6) Pendulum for controlling duration of stimulus; (7) Mechanism for controlling duration of stimulus and interval between stimulations (not used in this experiment); (8) Galvanometer for skin-temperature thermocouple measurements; (9) Experimenter's key for releasing timing pendulum; (10) Rheostat for controlling temperature of stimulator.

Since the areas explored were upon the forehead, *O* reclined upon a bed with his head in a plaster and leather headrest in which it could be held quite immovable during the course of the experiment.

Observers. Fifteen *O*s, including 10 undergraduate students of psychology, 1 graduate student, 2 research assistants, and the 2 authors, served in the study. With the exception of the authors, none had previously served in cutaneous work. Consequently the untrained *O*s were given preliminary series of stimulations to familiarize them with the different cutaneous experiences. The preliminary series of stimulations were given upon the forehead but not within the area of skin to be used in the main experiments in which 1 to 9 areas were explored on each *O*.

Procedure. At the beginning of each of the main experiments a small ink-dot was placed upon the forehead to serve as a point of reference for the upper left corner of the area to be explored. This was the only necessary marking done upon the skin, as each point within the area could be located by moving the stimulator so many diameters to the right or below the reference point.

The *O*s were instructed to report the quality of the sensation elicited, immediately at the end of each stimulation, by raising the fingers of one hand. One finger indicated a sensation of pressure, 2 fingers meant a sensation of warmth and 3 fingers indicated heat; 4 fingers were to be raised for cold and 5 fingers for pain, but these sensations were not reported.

Record sheets were prepared in advance representing the areas of skin explored. Each spot stimulated had a corresponding position on the record sheet.[3] The first stimulation was given to a spot just to the right of the reference ink-dot. The report to this stimulation was recorded and the stimulator moved down 1.1 mm. to the spot below, Spot 2 of Column 1, and the second stimulation was given. Then the first spot was restimulated, following which the stimulator was applied to spots 3, 2, 4, 3, 5, 4, 6, 5, 7, 6, and 7, all in Column 1. Thus each spot in the column was stimulated twice with 2 intervening stimulations to adjacent spots. Then Column 2, the first column to the right of the reference dot was stimulated, beginning at Spot 7 and proceeding up to Spot 1. The first, third and fifth columns were stimulated from Spot 1 down to Spot 7 and the second and fourth columns were explored from Spot 7 up to Spot 1. Every spot was stimulated twice to provide an indication of correspondence between the responses and to show any difference in sensitivity to the first and second stimulations.

Thirty-eight of the areas contained 35 spots each in 5 columns of 7 spots each. Each of the other 26 areas contained 30 spots in 5 columns.

After an area was explored, stimulating every spot twice, with one stimulus-pressure, a rest-interval of 5 min. was given before reëxploring with a stimulus of another pressure. The stimulus-pressures used were 1, 2, 4 and 6 grm. The order of using the pressures was unknown to *O* and the same order was never used twice in succession with the same *O*. Thus an area may have been explored first with a stimulus of 4 grm. pressure, then reëxplored with the stimulus at 1 grm., then with the stimulus at 6 grm. and finally with the stimulus at 2 grm. pressure.

Each stimulation lasted 2 sec. and the interval between stimulations was 8 sec. with the exception of the first 2 and last 2 stimulations in each column which were 16 sec. apart to allow 26 or 28 sec. between successive stimulations of identical spots. In their work on successive stimulation of cold spots, Drury and Dallenbach found that "the longer time intervals (1–20 min.) are to be preferred (to intervals of

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[3] 'Spot' refers to the small area of skin, 1.1 mm. in diam., which was covered and stimulated by one application of the stimulator. An area of 30 spots is simply an area within which 30 different locations may be stimulated. A 'warm spot' is a spot at which a warm sensation was elicited and in our usage has no relation to specialization of underlying sense organs.

5–20 sec.) when a high degree of correspondence between successive stimulations is desirable" though the difference in favor of the longer interval was quite small.[4] It is a question, of course, whether their results obtained for cold-sensations hold also for warmth.

For the whole series of experiments the mean stimulus-temperature was 43.1°, with S.D. of 0.29°. This temperature was used because it is approximately the optimal point in regard to the arousal of warm sensations and the critical temperature of heat.[5] The effect of varying stimulus-pressure on both warm and heat sensations may be found.

The work was done between January and March, 1931, in an experimental room in which a fairly constant air temperature could be maintained with adequate ventilation though without air currents blowing over the *O*'s head. The mean room temperature was 22.13°, with S.D. of 0.7°.

Results. At any one stimulus-pressure there was great variability in the percentage of spots within an area giving temperature reports. There were consistent differences between different *O*s, some giving many more reports per area than others.

TABLE I

RESULTS FROM A TRAINED *O*, SHOWING FOR EVERY STIMULUS-PRESSURE THE PERCENTAGE OF STIMULATIONS AROUSING TEMPERATURE SENSATIONS

Stimulus pressure	Trial	% reports of		Total temperature reports
		warmth	heat	
1 grm.	1st	11	0	11
	2nd	9	0	9
2 grm.	1st	9	0	9
	2nd	9	0	9
4 grm.	1st	11	0	11
	2nd	14	0	14
6 grm.	1st	14	0	14
	2nd	14	0	14

A high positive correlation was found between the sensitivity of areas to first and second stimulations at all stimulus pressures. The correlations were 0.94, 0.95, 0.96 and 0.94 between the total temperature reports to first and second stimulations at the stimulus-pressures of 1, 2, 4 and 6 grm., respectively. The correlations for warmth and heat reports separately were also similarly high (see Table V).

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[4] M. B. Drury and K. M. Dallenbach, The responses of cold spots under successive stimulation, this JOURNAL, 43, 1931, 496.

[5] Heiser, *loc. cit.*, 43. At 43° 33.73% of the stimulated spots gave warmth and 10.91% gave heat. Cf. also E. Lowenstein and K. M. Dallenbach, The critical temperatures for heat and for burning heat, this JOURNAL, 42, 1930, 425 ff.

As measured by Dallenbach's "absolute" method the correspondence between the sensations of individual spots was fairly high.[6] At the stimulus-pressure of 1 grm. the correspondence between reports to first and second stimulations ranged from 37% to 100% with the mean at 76%. This measure compares favorably with those obtained by Dallenbach, between 73% and 87%, with three trained *O*s.

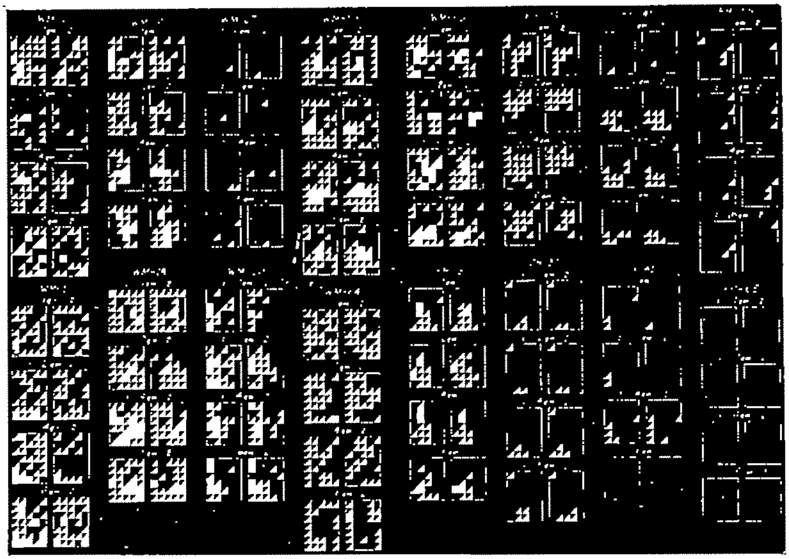

FIG. 2. MAPS OF REPORTS IN 16 AREAS ON 2 TRAINED *O*S

Each map is proportional in shape to the area of skin represented. The temperature-reports are shown according to kind and location within the area. The triangular and square symbols represent warmth and heat respectively. The blank portions of the areas gave only touch or pressure reports. Above each group of 8 maps are the initials of the *O* and area number and above each pair of maps the stimulus-pressure is given. Maps 1 and 2 show the reports of the area to first and second stimulations respectively.[7]

The temperature reports of a sample area are shown in Table I, below. The area contained 35 spots. The reports are shown (in Table I and all following Tables) in terms of percentages of the total spots stimulated within the area. For example, at 1 grm. pressure 4 or 11% of the stimulated spots reported warmth to the first stimulation, while 3 or 9% gave warmth to the second stimulation.

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[6] K. M. Dallenbach, The temperature spots and end organs, this JOURNAL, 39, 1927, 421. Correspondence is measured by taking the percentage of total spots stimulated which give the same reports to each of 2 stimulations.

[7] The complete tables and photographs of reports in all 64 areas are on file with the senior author.

Fig. 2 shows a group of maps of the reports of 16 different areas.

(1) Influence of an increase in stimulus pressure upon temperature sensations. Since each of the 64 areas was explored with 4 different stimulus-pressures, 192 comparisons may be made between the sensations at one pressure and a pressure one step (1 or 2 grm.) higher.[8] Table II shows these comparisons of warmth, heat, and total temperature-reports. The results given in Table II are based upon the reports to first stimulations only as the reports to second stimulations are not indicative of the true sensitivity of the skin because of the fatigue or adaptation between the first and second stimulations of a particular spot. The reports to second stimulations are treated below. The increase in stimulus-pressure causes a fairly reliable increase in the percentage of spots reporting warmth but there is too much variability in the data on heat to warrant any conclusions regarding it.

TABLE II

RELATION BETWEEN AN INCREASE IN STIMULUS-PRESSURE AND TEMPERATURE REPORTS
(192 comparisons)

	Warmth		Heat		Total	
	Lower pressure	Higher pressure	Lower pressure	Higher pressure	Lower pressure	Higher pressure
Mean	31.33	33.45	10.05	10.13	41.4	43.65
S.D.	22.93	23.90	12.82	13.06	26.6	28.03
Correlation	0.76		0.80		0.82	
Difference	2.12		0.08		2.25	
*S.D.$_{\text{diff.}}$	1.88		0.59		2.14	
Critical ratio	1.13		0.14		1.05	

* The formula $[\sigma \text{ av. } 1^2 + \sigma \text{ av. } 2^2 - 2(\sigma \text{ av. } 1)(\sigma \text{ av. } 2)(r_{1,2})]^{1/2}$ is used in Tables II to VI in finding the S.D.$_{\text{diff.}}$. The product-moment method of correlation is used throughout.

(2) Temperature-sensations at the different stimulus pressures. Table III shows the mean temperature-reports at the different stimulus-pressures. These results are computed from reports to first stimulations. It is clear that the number of temperature-reports increased as the stimulus-pressure increased. The only reliable differences between the mean temperature reports occurred, however, between stimulus-pressures which were 2 or more steps apart, that is, between pressures of 1 and 4 grm., or 1 and 6 grm., or 2 and 6 grm. Table IV shows the reliability of the differences between the mean temperature reports given in Table III.

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[8] There are 64 comparisons of the sensations or reports to 1 and 2 grm., 64 comparisons of reports to 2 and 4 grm., and 64 comparisons at 4 and 6 grm.

(3) The difference between temperature reports to the first and second stimulations at each of the different stimulus-pressures. Table V shows the difference in the temperature-reports to the first and second explorations of the 64 areas at each stimulus-pressure. The reports of warmth and heat were more numerous to the first stimulations at each of the different stimulus-pressures. The differences are quite reliable with the exception of warmth-reports at 4 and 6 grm. Although this decrease in sensitivity to the second stimulations has generally been interpreted as demonstrating the phenomenon of adaptation, fatigue or "tuning" of the temperature receptors, it is quite possible that other factors are operating. Our stimulus combined pressure with the delivery of heat to the skin. Such stimulation may change the conductivity of the skin for heat flow to the receptors.

TABLE III

MEAN TEMPERATURE-REPORTS AT DIFFERENT STIMULUS-PRESSURES
(64 areas)

Reports		Stimulus-pressures (in grm.)			
		1	2	4	6
Warmth	Mean	29.38	31.64	33.2	35.78
	S.D.	22.44	23.30	22.89	25.00
Heat	Mean	10.39	9.92	9.84	10.63
	S.D.	12.29	13.5	12.62	13.04
Total	Mean	39.75	41.1	43.36	46.49
	S.D.	24.99	26.77	26.46	29.12

TABLE IV

RELIABILITY OF DIFFERENCES BETWEEN MEAN TEMPERATURE-REPORTS GIVEN IN TABLE III

Reports	Stimulus-pressure (in grm.)					
	1 & 2	1 & 4	1 & 6	2 & 4	2 & 6	4 & 6
Warmth						
Correlation	.64	.80	.73	.77	.83	.84
Difference	2.26	3.82	6.4	1.87	4.45	2.58
$\text{S.D.}_{\text{diff.}}$	2.43	1.79	2.25	1.96	1.46	1.19
Crit. ratio	.93	2.13	2.84	.95	3.05	2.17
Heat						
Correlation	.60	.72	.83	.76	.79	.84
Difference	.47	.55	.24	.08	.71	.79
$\text{S.D.}_{\text{diff.}}$	1.45	1.17	.93	1.14	1.08	.91
Crit. ratio	.32	.47	.26	.07	.66	.87
Total						
Correlation	.72	.84	.68	.93	.88	.94
Difference	1.35	3.61	6.74	2.26	5.39	3.13
$\text{S.D.}_{\text{diff.}}$	2.43	1.83	2.75	1.25	1.75	1.25
Crit. ratio	.56	1.97	2.45	1.81	3.08	2.50

(4) Comparison of temperature-reports in the first and fourth quarters of areas. The first quarter of an area refers to the first fourth of the spots that were stimulated within the area. In areas of 35 spots, for example, the first quarter includes the first 9 spots stimulated, and the fourth quarter includes the last 9 spots.

In the study of stimulus-temperatures, the senior author found fairly reliable differences between the temperature-reports in the first and fourth quarters of his areas and the direction of the difference was related to the stimulus-temperature used.[9]

Table VI gives the comparisons of the reports in the first and fourth quarters of all areas at all stimulus-pressures. The reports are given as percentages of ¼ area. There is a reliable difference between the reports of the first and fourth quarters of the areas explored but this difference is not in the same direction for both warmth- and heat-sensations. The warmth-sensations are more numerous in the first quarters while the heat-sensations are more numerous in the fourth quarters. It appears that some process is active during the course of stimulation which lowers the threshold for heat-sensation or raises the threshold for warmth.

TABLE V

Mean Temperature-Reports to First and Second Stimulations at Different Stimulus-Pressures

Reports	Stimulus-pressure							
	1 grm.		2 grm.		4 grm.		6 grm.	
	1st	2nd	1st	2nd	1st	2nd	1st	2nd
Warmth								
Mean	29.38	24.92	31.33	28.05	33.2	32.11	35.78	33.91
Correlation	.95		.89		.80		.94	
Difference	4.46		3.25		1.09		1.87	
S.D.$_{\text{diff.}}$	.91		1.34		1.80		1.06	
Crit. ratio	4.90		2.45		.61		1.76	
Heat								
Mean	10.39	7.89	9.92	7.11	9.84	7.27	10.63	7.89
Correlation	.89		.87		.89		.93	
Difference	2.50		2.81		2.57		2.74	
S.D.$_{\text{diff.}}$	.70		.83		.74		.67	
Crit. ratio	3.57		3.39		3.47		4.09	
Total								
Mean	39.75	32.34	41.10	35.16	43.36	37.81	47.49	41.72
Correlation	.94		.95		.96		.94	
Difference	7.41		5.94		5.55		4.77	
S.D.$_{\text{diff.}}$	1.08		1.07		.94		1.24	
Crit. ratio	6.86		5.55		5.90		3.85	

The reports to the stimulus of 2 grm. are directly comparable to the reports of the first and fourth quarters of areas at the stimulus-temperature of 43° shown in the previous study.[10] In both studies the warmth-reports were more numerous in the first quarters of the areas and the heat reports were more numerous in the fourth

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[9] F. Heiser, *op. cit.*, 49-53.

[10] F. Heiser, *op. cit.* Table XIII, 52.

TABLE VI

MEAN TEMPERATURE-REPORTS IN THE FIRST AND FOURTH QUARTERS OF ALL AREAS AT EVERY STIMULUS-PRESSURE

Reports	Stimulus-pressure							
	1 grm.		2 grm.		4 grm.		6 grm.	
	1st	4th	1st	4th	1st	4th	1st	4th
Warmth								
Mean	31.64	24.75	32.53	27.14	38.05	26.68	41.29	29.57
Correlation	.60		.41		.51		.55	
Difference	6.89		5.39		11.37		11.72	
S.D.$_{\text{diff.}}$	2.03		2.67		2.43		2.43	
Crit. ratio	3.39		2.02		4.68		4.82	
Heat								
Mean	8.28	14.26	7.55	9.77	6.72	11.48	8.63	10.55
Correlation	.33		.39		.41		.16	
Difference	5.98		2.22		4.76		1.92	
S.D.$_{\text{diff.}}$	1.86		1.51		1.79		1.94	
Crit. ratio	3.21		1.47		2.66		.99	
Total								
Mean	39.92	33.83	39.41	37.03	45.20	38.20	49.80	39.53
Correlation	.53		.41		.47		.49	
Difference	6.09		2.38		7.00		10.27	
S.D.$_{\text{diff.}}$	2.54		3.19		2.99		3.02	
Crit. ratio	2.40		.75		2.34		3.40	

quarters. This agreement between the comparable results of both studies affords fairly reliable evidence for the assumption that during the exploration of an area there are certain changes occurring either in the temperature-receptors or in surrounding local conditions which cause the fourth quarters of the areas to give many more heat-reports than they would have given if the same spots had been stimulated earlier in the exploration of the area. There is the phenomenon, then, of heat-sensations given at spots which would probably have given warmth-sensations were it not for the effects of the preceding 9 or 10 min. of stimulation in the area.

SUMMARY

(1) Sixty-four areas of skin on the foreheads of 15 *O*s were explored for temperature-sensations. Stimulus-pressures of 1, 2, 4, and 6 grm. were used. Other factors of stimulation such as temperature, duration, size, interval between stimuli and between restimulation of identical spots, room-temperature, and general condition of the *O*s, were held constant so far as possible.

(2) Different areas varied greatly in the number of sensations of temperature elicited within them.

(3) The procedure was such as to give each spot 2 applications of the same stimulus at an interval of 26 or 28 sec. The correspondence between the reports of single spots was high, averaging about 75%.

(4) The correlation between the reports from whole areas to the first and second stimulations was high, showing great consistency in the differences between the sensitivity of different areas.

(5) The number of temperature-sensations elicited within an area increased with an increase in stimulus-pressure. The increases in pressure from 1 to 2 grm., from 2 to 4 grm., and from 4 to 6 grm. resulted in increases in temperature-reports, though the increases were neither large nor reliable. The differences in the temperature reports to stimulus-pressures of 1 and 4 grm., 1 and 6 grm., and 2 and 6 grm. were relatively large and quite reliable.

(6) The number of heat-sensations elicited within an area was approximately the same at each stimulus-pressure.

(7) The number of temperature-reports to the first stimulations was reliably greater than the number to the second stimulations at all stimulus-pressures.

(8) At each of the stimulus-pressures the number of warmth reports was greater in the first than in the fourth quarters of the areas while the heat-reports were more numerous in the fourth than in the first quarters.

THE RELATION OF EMOTION TO SEX, PRIMOGENITURE, AND DISEASE

By GEORGE M. STRATTON, University of California

The present report is concerned with the following questions.[*] Does the connection between emotion and the incidence of disease, indicated in earlier reports by the present writer,[1] hold alike for women and for men? Does the relation between emotion and primogeniture described by him elsewhere[2] exist both in women and in men? Among the first-born, does the fact of being an only child have the same result for men as for women? Finally, given like external occasions for fear or anger, are men and women in general alike in the strength of their emotional responses? As the method, by which the evidence presented here was gathered, is described at length in an earlier study,[3] it will not be necessary to report it here.

I. EMOTION AND THE INCIDENCE OF DISEASE

With regard to the first of these questions, as to emotion and the incidence of disease, the earlier finding was that persons generally who have had certain diseases show a somewhat more intense emotional reaction than do persons whose histories are free from these diseases. This connection between emotion and disease was found to hold for anger more clearly than for fear.

With a larger number of persons than was studied earlier and with men and women now considered separately (Table I), there is evidence of a connection between emotion and the personal history with respect to disease both in women and in men, although this connection is not the same in the two sexes.

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[*] Accepted for publication June 26, 1933.

[1] Emotion and the incidence of disease, *J. Abn. & Soc. Psychol.*, 21, 1926, 19-23; Emotion and the incidence of disease: The influence of the number of diseases and the age at which they occur, *Psychol. Rev.*, 36, 1929, 242-253.

[2] Anger and fear: Their probable relation to each other, to intellectual work, and to primogeniture, this JOURNAL, 39, 1927, 125-140. The method briefly described was, that each member of a group of students was given a printed description of 19 situations likely to cause anger, and 6 types of anger response, and was asked to report as soon as any of the situations had occurred, what the situation had been during the interval and what type of response his reaction had most closely resembled. A similar method was followed for fear, with 21 type situations. Records were available of the disease histories, family histories, mental and physical measurements of the *Ss*.

[3] *Op. cit.*, this JOURNAL, 39, 1927, 125-140.

In the case of fear, a history of disease is of clear significance in women, but not in men. The mean fear-response given by women *with* a history of disease is appreciably more intense than that given by the women with *no* such history, the increase of intensity with disease being more than 3 times the probable error of the increase. With men, in the case of fear, there is no evidence in our table that a history of disease is of any significance.

In the case of anger, however, there is clear evidence that in both sexes there is an emotional effect connected with a history of disease. This influence is in the same direction in both the sexes; namely, to make more intense the emotional response. The reactions of the women with a history of disease are higher than those of the women whose history is without disease, the difference between the two means being more than 8 times the probable error of the difference. For the men the increase in reaction connected with disease is more than 5 times the probable error of the increase. In the case of anger both of men and women the increment of reaction is of about the same amount.

TABLE I

EMOTION AND THE PERSONAL HISTORY WITH RESPECT TO DISEASE

	Without disease				With disease						
	No. Os	No. Responses	Mean	P.E.$_M$	No. Os	No. Responses	Mean	P.E.$_M$	Diff.	P.E.$_D$.	$\frac{\text{Diff.}}{\text{P.E.}_D}$
Fear											
Women	365	1167	100.73	.47	998	2953	102.80	.32	2.07	.57	3.66
Men	162	551	93.47	.66	350	1133	92.44	.44	1.02	.79	1.29
Anger											
Women	442	1751	98.06	.39	1157	4756	101.69	.22	3.63	.44	8.19
Men	198	927	96.78	.47	397	1793	100.19	.35	3.41	.59	5.80

No substantial meaning, probably, is to be attached to the fact that Table I reveals no connection between a history of disease and the fear-reactions of men. The evidence reported in an earlier paper[4] indicates that fear and anger are correlated with each other positively with respect to their intensity. Any constitutional change, then, which affects the one of these emotions may be expected to affect the other in a like direction. It seems improbable, then, that a history of disease is in reality connected with men's anger and is disconnected from their fear. The silence of the table, consequently, may well mean not that disease is of no significance with regard to men's fear but that the devices of investigation here used have failed to disclose the significance.

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[4] *Op. cit.,* this JOURNAL, 134 ff.

II. EMOTION AND PRIMOGENITURE

With regard to primogeniture, it was earlier reported of men and women grouped together that the first-born when compared with those of a later order of birth showed an intenser anger but no difference in fear.[5] With men and women now segregated, and with a much larger number—between two and three times the former number—of persons included in the account, the first-born men appear less fearful than other men, while the first-born women appear more irascible than other women. These differences are small, but their relation to their probable errors makes them appear significant (Table II). No other significant differences between men and women here appear.

To be the oldest child, then, does not seem to have the same emotional result in men and in women. In men the only clear effect disclosed by the present method is in the region of fear, the effect being to decrease the fear. In women the only effect revealed is in the region of anger; namely, to intensify the anger.

TABLE II

PRIMOGENITURE AND EMOTION

	First born				Not first born						
	No. Os	No. Responses	Mean	$P.E._M$	No. Os	No. Responses	Mean	$P.E._M$	Diff.	$P.E._D$	$\frac{\text{Diff.}}{P.E._D}$
Fear											
Women	484	1693	101.88	.379	879	2427	102.45	.362	.57	.515	1.11
Men	173	659	91.16	.547	339	1025	93.82	.487	2.66	.732	3.63
Anger											
Women	508	2322	101.68	.308	1091	4185	100.17	.238	1.51	.389	3.88
Men	182	864	99.06	.489	413	1856	99.23	.343	.17	.597	.29

As to the emotional effect of being an 'only' child, the earlier finding, with men and women unsegregated, was negative: the first-born child without brothers or sisters appeared not different in anger or fear from the first-born with brothers or sisters.[6] Nor does any clear effect appear from the present evidence with men and women taken separately: the man without sibs is not clearly different from other men; nor is the woman, from other women (Table III). Such differences as appear in the table are small and with relatively large probable errors.

Yet the negative evidence is slightly less pronounced than in the earlier report. There is now a faint suggestion that the first-born, if a girl with brothers and sisters, is inclined to more anger than is the only daughter—as though younger brothers and sisters taxed a girl's resources more than they did a boy's. Younger brothers with their socially-encouraged rebellions against petticoat domination perhaps seed the soil more richly for a crop of anger in the girl, whereas the *only* daughter can find in the home merely her parents to annoy her. The first-born if a boy with brothers and sisters may perhaps be a shade more courageous than the only boy—as though the first-born's high office of big brother, with the expectation of courage which it suggests, tended to make him more stalwart in the presence of danger. But all these ideas may be no more than pleasant fancies, for the present evidence speaks but indistinctly in regard to them.

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[5] *Ibid.*, 138 f.

[6] *Ibid.*, 139 f.

TABLE III

EMOTION AND THE ONLY CHILD

	First born without sibs				First born with sibs						
	No. Os	No. Responses	Mean	$P.E._M$	No. Os	No. Responses	Mean	$P.E._M$	Diff.	$P.E._D$	$\frac{Diff.}{P.E._D}$
Fear											
Women	177	568	101.86	.669	307	1125	101.90	.460	.04	.81	.005
Men	55	230	92.15	1.024	118	429	90.63	.636	1.52	1.21	1.26
Anger											
Women	166	730	100.73	.540	342	1592	102.12	.374	1.39	.66	2.12
Men	58	249	100.08	.905	124	615	98.65	.581	1.43	1.08	1.33

III. SEX DIFFERENCES

To the question whether, in situations apt to call out fear or anger, women and men in general differ in the strength of their emotional responses, the present study suggests an affirmative answer. In Table IV the mean reactions of the women both in fear and anger are seen to have been more intense than those of men. The women's reactions exceed the men's by an amount which, for anger, is more than 4 times its probable error; and which, for fear, is more than 20 times its probable error.

TABLE IV

THE INTENSITY OF FEAR AND ANGER GENERALLY

	Women				Men						
	No. Os	No. Responses	Mean	$P.E._M$	No. Os	No. Responses	Mean	$P.E._M$	Diff.	$P.E._D$	$\frac{Diff.}{P.E._D}$
Fear	1363	4120	102.22	.26	512	1684	92.78	.37	9.44	.45	20.93
Anger	1599	6507	100.71	.19	595	2720	99.17	.28	1.54	.34	4.53

But though the women's reactions exceed the men's both in fear and in anger, the women's excess is very different in fear than in anger. Men and women appear to differ in their fear far more than in their anger. In their fear the women exceed the men by a relatively large amount, 9.44. In their anger, however, the women exceed the men by only 1.54. That the women, if they exceed the men in fear, should exceed them in anger also, is in keeping with the positive correlation between the intensity of fear and the intensity of anger, already mentioned.

The general outcome just described is in accord and also at variance with common belief. The frequent opinion that women are more emotional than men is here supported. There is also support for the particular opinion that women are more timid than men. The opinion, however, that women are less inclined to anger, that women are more placid in situations which arouse men to wrath appears quite unsupported.

If what is here indicated be confirmed, we might seek our explanation both in nature and in culture. As for culture, there is held up before the male's eyes from early boyhood, the ideal of fearlessness, with timidity rebuked and ridiculed. Anger, however, is permitted him and condoned; indeed in many a situation his fellows vehemently demand anger in the male. The boy's young companions, male and female, support this ideal as heartily as do his elders. The social ideal for the conduct of girls and women, on the other hand, has been quite different, less ready to countenance anger in them, while viewing with leniency or approval some show of fear.

These opposite standards for women and men may have been of a double effect in the region of the present study. They may have tended to change the actual intensity of the emotions, reducing the fear in men and the anger in women. But also they may have tended to introduce a systematic error by our men and women in observing, scaling, and recording their own reactions. The men may have been led to underestimate the intensity of the fear they felt, as an unintended act of self-elevation; while the women, from a like motive, may have underestimated their own anger.

The appeal to cultural forces does not, however, exclude the appeal also to natural forces. Indeed it seems probable, from our present evidence, that men and women are by nature unlike in their emotional constitution. For were they of the same psychophysical constitution with respect to fear and anger, the double cultural standard described above would have tended to reduce women's anger and men's fear, with the net result that women's average responses would be lower than men's for anger, and higher for fear. But the actual findings are that women's reactions are higher than the men's for *both* fear and anger. It seems probable that the cultural situation, then, has worked upon a native difference in constitution whereby the thresholds of fear and of anger are lower for women than for men, and has modified this difference only to the extent that the difference has been increased for fear and diminished for anger, the general direction of the difference, however, remaining unchanged.

As to the meaning of this fundamental difference between men and women, if its existence should be verified, psychologists will at present of course disagree. By those who see in emotion chiefly weakness and breakdown, a sign chiefly that the individual is inadequate to his situation and becomes by his emotions still less adequate, the present findings will be entered on the debit side of women's account. Woman's lower threshold for fear and anger these would incline to count as a part of her weakness, along with her inferior muscular strength. Those other psychologists who see in emotion a means generally to be more nearly adequate to one's situation, usually an addition to one's equipment to *meet* situations, rather than to *fail* to meet them—a view which the present writer shares[7]—the difference in emotional threshold between men and women would be quite in keeping with her elements of strength; with, for example, her greater power to resist disease, her greater constitutional vigor. The sthenic emotions, as the papers just cited have attempted to show, are connected with the invigorating not only of our muscular system, as Cannon has made clear, but also of our powers of thought, of impulse, and of desire. The sthenic emotions, such as excitement, make available at the instant a larger array of pertinent ideas and organize these around the central desire now needing their service. Not only are our voluntary muscles fortified, but our thoughts, feelings, and impulses are selected, accelerated, intensified, and integrated by sthenic emotion, with the result that the individual can the better meet the emergency. The positive correlation between fear and anger, already mentioned more than once, suggests that these two emotions are largely of a common quality and function, and that the sthenic character of anger is shared in some degree by fear. It seems quite possible, then, that the lower average thresholds for fear and anger in women are normally matters not of weakness, but of strength; they are part of an endowment, modified by social standards, for a fuller meeting of important biological and cultural needs.

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[7] An experience during danger, and the wider functions of emotion, *Problems of Personality; Studies in Honor of Dr. Morton Prince*, 1925, 47-62; The function of emotion as shown particularly in excitement, *Psychol. Rev.* 35, 1928, 351-366.

HIGHER ORDER CONDITIONING WITH CONSTANT MOTIVATION

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Pavlov, citing the experimental data of Foursikov, declares that conditioned reflexes cannot be established beyond the third order. "It was," he says, "found impossible in the case of alimentary reflexes to press the secondary stimulus into our service to help us in the establishment of a new conditioned stimulus of the third order. Conditioned reflexes of the third order can however be obtained with the help of the second order of conditioned reflexes in defence reactions such as that against stimulation of the skin by a strong electric current. But even in this case we cannot proceed further than a conditioned reflex of the third order."[1]

The question arises, whether this failure to establish conditioned responses beyond the third order may be due to failure to maintain adequate "motivation" in the animal rather than to any inherent difficulty of the problem itself. Consider the experiment of Foursikov: Stimulus A (electric stimulus to the skin over the front paw) is the primary or unconditioned stimulus which evokes a defence reaction in the dog; stimuli B (mechanical irritation of the skin over the hind paw), C (sound of bubbling water), and D (760-cycle tone) are indifferent stimuli which evoke no defence response in the animal. The stimulus-combination B + A is repeatedly presented to the animal until B is converted into a conditioned stimulus of the first order. Now the combination C + B is repeatedly presented until C alone evokes the defence reaction (C thus becomes a conditioned stimulus of the second order) ; in the same way the repetition of D + C converts D into a conditioned stimulus of the third order. Repetition of a fifth stimulus E with D does not result in the establishment of E as a conditioned stimulus. The establishment of B as a conditioned stimulus depended upon its becoming related to the defence response of the animal evoked by stimulus A through the repeated presentation of B along with A. In the same way C and D were able to evoke the defence reaction only by virtue of their relationship to A through B. Thus C is one step more remote from A than is B,

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[1] I. P. Pavlov, *Conditioned Reflexes*, 1927, 34.

and D is one step more remote than is C; it seems reasonable to assume that the kinship between A and D is less than that between A and C, which is also less than that between A and B. The failure to establish E as a conditioned stimulus might then very well be attributed to the fact that its relationship to A through D, C, and B is too weak for it to become effective in educing the response natively elicited by A. Plausibility is loaned to this explanation by the fact that Foursikov noted a definite gradient of the strength of the responses to the different orders of conditioning; he found that the response to B was stronger than that to C, and that the response to C was stronger than that to D.

The present investigation was designed to determine whether conditioning beyond the third order is possible when the *motivation* of the animal is maintained through the various orders. Our hypothesis, in brief, is that failure to condition at higher levels is due to a failure of the incentives which normally serve to activate the animal's response.

Procedure. Two dogs were used in this experiment; one was a male German Shepherd (age 18 mo.), the other a male mongrel German Shepherd (age 12 mo.).

The unconditioned response consists in withdrawal (flexion) of right foreleg when shock is applied to paw; conditioned stimulus of the first order is a loud 1000-cycle tone (intensity about 70 decibels above the limen); of second order, electric light; of third order, stream of water on nose; of fourth order, electric bell; and of fifth order, electric fan playing on the animal. In addition to these, a general energizing stimulus was employed (shock to left thorax,[2] administered whenever animal fails to flex leg and omitted upon each correct response). The animal was confined in a stock in a soundproofed test-room. All stimuli were automatically delivered to the animal by electric controls; their duration was timed by means of a synchronous motor which operated the contact-apparatus. Every one of the 5 unconditioned stimuli was 2 sec. in duration and each of the electric shock-stimuli about ¼ sec.

The conditioned response to the first order stimulus (tone) was first thoroughly established in the usual way (*i.e.* 2-sec. sound immediately followed by shock to paw, until animal learned to flex the leg before the grid was charged). Next the second-order stimulus (light) was presented concurrently with the first-order; but now the shock was applied, not to the forepaw as heretofore, but to the left side of thorax. Just as the animal could escape the shock-to-paw by lifting the foot, so now it could escape the thoracic shock by lifting the foot and thus breaking the shock-circuit. After the response had been established to the light alone, the third-order stimulus (stream of water on nose) was presented together with second-order (followed by thoracic shock) until the animal responded to the stream of water alone. The fourth-order stimulus (electric bell) was next presented with the third, and the fifth (electric fan playing on animal) with the fourth. It should be emphasized that shock-to-paw was the *only* one of all these stimuli that was capable of evoking the flexion-response prior to training.

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[2] The thoracic shock was administered quite automatically, not by a decision of *E.* The animal's forelegs were fastened each to the end of a short lever with but one degree of freedom (up-down). A leather strap, provided with two metal contact-points which pressed into the skin, was bound around the thorax. Leads passed from these contacts to a mercury-in-glass switch so attached to the right lever that the right leg could not be flexed without breaking the mercury-contact and thus opening the shock-circuit. In this way a very definite and uniform incentive was provided at all levels: punishment upon failure to respond. This incentive did not become, as in Foursikov's case, increasingly remote from the higher-order stimuli, but persisted throughout as a *constant* energizing agent.

TABLE I

SHOWING RESULTS FOR DOG I

Column A, test-period; Column B, stimulus-combination; Column C, ratio of correct responses to total trials for test-period, *e.g.* 24/25 means the animal responded 24 times in 25 trials.

A	B	C	A	B	C	A	B	C
1	SL	21/25	14	SL	5/5	25	WB	5/5
	L	0/14		L	21/25		B	17/25
2	SL	25/25	15	SL	5/5	26	WB	10/10
	L	5/12		L	25/25		B	12/25
3	SL	25/25	16	SL	12/12	27	WB	10/10
	L	10/10		LW	5/25		B	23/25
4	SL	25/25	17	SL	11/11	28	B	21/25
	L	10/10		LW	9/25	29	B	25/25
5	L	17/20	18	SL	5/5	30	B	25/25
6	L	20/25		LW	9/10	31	BF	10/10
7	L	17/25		W	7/10		F	11/20
8	L	19/25	19	LW	5/5	32	F	25/25
9	L	17/25		W	21/25	33	F	25/25
10	SL	24/25	20	LW	7/10	34	F	25/25
	L	10/10		W	18/25	35	F	25/25
11	SL	5/5	21	LW	5/5	36	F	25/25
	L	24/25		W	24/25	37	F	25/25
12	SL	5/5	22	W	23/25	38	F	25/25
	L	19/25	23	W	25/25			
13	SL	5/5	24	WB	5/5			
	L	25/25		B	3/15			

Results. Table I presents the material from Dog I. In the second column, "S" signifies the 1000-cycle sound stimulus, "L" indicates electric light-stimulus, "W" indicates the squirt of water on the animal's nose, "B" indicates electric bell-stimulus, and "F" represents the electric fan-stimulus. The stimulus-pattern "SL" indicates that the tone and light were presented simultaneously, "LW" that the light and water were presented together, and so on. The third column shows the ratio of responses to the number of times the given stimulus pattern was presented during one training period. Table II presents the data of Dog II.

Discussion of results. The most obvious difference between our procedure in the present experiment and that of Foursikov is the introduction of the thoracic shock. It is here that our discussion must center. Two fundamental questions arise. (1) Is the thoracic shock a native or unconditioned stimulus to flexion of the right foreleg? From careful and repeated observation of various animals, we find that the thoracic shock when applied to an utterly naïve dog which has never been in the training situation before, evokes a response which includes these elements: quick gasp or yelp; wincing away from electrodes; sudden sag or slump of hind limbs, particularly the homolateral one; strong adduction of tail. One or more of these will occur at once, directly followed by whining or barking, biting or gnawing at nearby objects, and frequently a general struggle to escape (twisting, jerking, pulling). In this struggle the dog will often try to free both forelegs from the attached levers; but these vigorous flexions are of course determined by the general somatic activity and are in no sense a specialized response. The exact pattern depends on the force of the shock; but we have yet to see a single case which could be interpreted as a specific or localized response of either foreleg to the thoracic shock. We feel warranted therefore in answering this question with an assured negative. (2) But is not the thoracic shock, more probably, a conditioned or substitute stimulus? The best answer to this is found in a note from our protocols, made over six weeks after the two animals, Dogs I and II, had been working with the thoracic shock. At this time, Dog I had received, under test-conditions, by

TABLE II

Showing Results for Dog II

A	B	C	A	B	C	A	B	C
1	SL	17/20	15	SL	8/8	26	WB	5/5
	L	4/8		L	20/25		B	1/15
2	SL	24/25	16	SL	5/5	27	WB	5/5
	L	9/14		L	25/25		B	18/25
3	SL	25/25	17	SL	8/8	28	WB	10/10
	L	10/10		LW	18/25		B	7/25
4	SL	25/25	18	SL	20/20	29	WB	10/10
	L	10/10		LW	12/25		B	17/25
5	L	25/25	19	SL	5/5	30	B	19/25
6	L	23/25		LW	10/10	31	B	19/25
7	L	18/25		W	10/10	32	B	15/25
8	L	21/25	20	SL	5/5	33	B	25/25
9	L	11/25		LW	10/10	34	BF	6/10
10	L	16/25		W	10/10		F	19/50
11	SL	25/25	21	LW	5/5	35	F	24/25
	L	10/10		W	16/25	36	F	24/25
12	L	11/25	22	LW	5/5	37	F	24/25
13	SL	5/5		W	21/25	38	F	25/25
	L	14/25	23	LW	5/5	39	F	25/25
14	SL	5/5		W	24/25	40	F	25/25
	L	25/25	24	W	25/25	41	F	25/25
			25	W	25/25	42	F	25/25

actual count 193 thoracic shocks, and had brought his responses to all four conditioned stimuli (second, third, fourth, fifth) up to 100%. Dog II had received 217 thoracic shocks and had reached 100% on second, third, fourth orders and 68% on fifth order. At this stage the following test-observations were made and recorded: "*Neither Dog, I nor II, lift right forepaw upon receiving shock on left thorax* even though they have by this time received it many times. Their reactions to thoracic shock alone are: quick gasp, crouching of whole body (hind legs slump), general bodily tremor, occasional urination and vomiting." In view of this evidence we feel justified in stating that the thoracic shock *does not tend to become* a conditioned or substitute stimulus for withdrawal of the paw. This evidence has been fully confirmed on at least 5 other dogs used in a later problem, which have been specifically tested on this point many times. In no case have they developed any tendency to react to the thoracic shock alone by foot-withdrawal. Accordingly we answer this second question also with a definite negative. To the question, why did not the thoracic shock become a substitute stimulus for flexion, the most obvious answer is, that it always followed the other stimuli and that this temporal position does not favor conditioning.

There is every indication that we might proceed indefinitely to still higher orders. Dog I needed only 10 trials for conditioning to the fifth stimulus (fan); thereafter he reacted 186 times in 195 trials. Dog II likewise after 10 combinations (B + F) reacted 216 times in 250 trials with the fifth stimulus. This proves beyond any doubt that the conditioning is not slowing down at the higher levels; on the contrary it seems to be speeding up. There is every reason to believe that higher order conditioning by this method is limited only by the number of available stimuli.

It should be noted that the primary stimulus (shock to paw) was *never* applied to either animal during the entire higher order conditioning period (42 days), and the fifth order conditioning was maintained at maximal strength for 175 trials in the one dog and for 125 trials in the other without reënforcement of any kind other than the thoracic shock. In view of these facts and since it has been demonstrated that the thoracic shock never acquired the status of a conditioned stimulus; and since the reactions elicited by its application alone (quick gasping intake of breath, crouching of whole body, somewhat general bodily tremor) are apparently not confined to any particular muscle group but are more general in character; the conclusion seems justified that the thoracic shock functions as a general motivating or activating stimulus.

In conclusion, we may remark that this material seems to clarify certain alleged discrepancies between the mechanisms of everyday learning and those which operate in animal conditioning. Theorists have long been disturbed by the need for repeated, well-nigh incessant, reënforcement of the conditioned response by the original stimulus. This one fact seems sharply to differentiate the two phenomena of learning and conditioning. It is clear that the growing child builds up a complex super-structure of acquired knowledge and skills, adding one substitute stimulus to another in endless sequence, with no need for repeated backward reference to some early avoidance or acceptance, no constant revival of some positive or negative response which stands at the head of the line. A conditioned animal, on the contrary, cannot seem to get away by more than one or two steps from the unconditioned stimulus which initiated the series. In consequence, there is something undeniably meager and tenuous about ordinary conditioned responses; they lack range, stability, and depth.

We feel that the simple experiment herein described has provided a satisfying explanation of this difficulty. Consider a simple, schematic case.

(A) Contact with candle-flame→withdrawal: original unconditioned.
(B) Look out! (spoken): first order conditioned.
(C) Look out! (printed): second order conditioned.
(D) Wet paint: third order conditioned.
(E) *Achtung!* fourth order conditioned.
(F) Sterile surgical field: fifth order conditioned.

The process continues step by step: A + B eventuates in a negative response to the warning cry, "Look out;" B + C gives the printed words "Look out" similar negative value; C + D keeps the child from sitting on wet paint; D + E gives negative significance to the German warning, *Achtung!* E + F finally makes the child careful of a sterile field. It is clear that the painful heat of the candle-flame years ago has nothing to do with this later continuance of the negative responses. We do not avoid wet paint because we fear being burned by it. The factors which serve to keep these responses alive are directly analogous to the thoracic shock in the dog; such things as parental stimulation, the code among playmates, emulation and rivalry, the rewards and penalties imposed by social discipline—all the many incentives commonly used to promote learning in children. The person whose hand carelessly violates a surgical field has future avoidance just as thoroughly reënforced by social devices as though his fingers were burned again. If the process of learning in the human had to draw its whole sustenance from the primitive response to pain or to food, it would fade out precisely as conditioning does in the dog when we proceed too far from the source of drive or energy. As soon as activation becomes subliminal, the response fails in human as in dog. Though the child be exposed in turn to countless substitute stimuli as it grows and learns, the incentives which society maintains are the really significant factors. It is these incentives which, like the thoracic shock, enable learning to continue for years. In our procedure the only critical place is the transfer from direct shock-to-paw to the more indirect thoracic shock. Some animals require time to get over this gap; but the gap once bridged, everything proceeds routinely thereafter. Likewise we see children who have difficulty in passing from the immediate incentives of pain or food to the less obvious incentives of social approval or rejection.

CONCLUSIONS

(1) Motor conditioning (flexion of paw) has been carried as far as the fifth order when animal is adequately energized. There is every reason to infer that the process could be carried on to any desired level.

(2) A shock to the left thorax provides the incentive or motivation needed to keep the animal responding. This activating shock is not an unconditioned stimulus, nor does it become a conditioned stimulus, for the motor response.

(3) The flexion response can, under these conditions, be maintained for long periods of time (at least for six weeks) without a single reënforcement by the primary stimulus (shock-to-paw).

(4) The thoracic shock serves much the same purpose in the dog as do ordinary social incentives in everyday human learning.

RETENTION AND THE INTERPOLATED TASK

By ELEANOR J. GIBSON and JAMES J. GIBSON, Smith College

The similarity problem in retroactive inhibition arises from the fact that the integrity of retention of memorized material is a function of the similarity between the learning and the interpolated activity. According to the Skaggs-Robinson theory,[1] the function is of the following nature. The curve of retention starts at a high level with maximum similarity, which Robinson took to mean that the interpolated task is identical with, or a continuation of, the original learning. The curve falls as the similarity of interpolated material is "reduced from identity" until retention reaches a minimum at the stage which, presumably, other investigators have called a "similar" interpolation. The curve then rises until, at the stage of dissimilar or rest interpolation, retention is at the level usually considered normal.

The latter portion of this curve embodies the type of results usually obtained in experiments on the similarity problem. The first part, showing decreasing retention with decreasing similarity, has been verified by Robinson[2] and Dreis[3] with two experiments wherein the interpolations consisted of varying amounts of rehearsal to which were added complementary amounts of new memorization. The greater the proportion of rehearsal, the greater was the similarity of the interpolated activity to the original learning. These experiments, along with some others,[4] arose from Robinson's tentative assumption that *improvement through practice* and the *disintegrating effect of interpolation* are related phenomena; in other words, that *practice-effect* passes over into "interpolation-effect" without break.

It should be noted that, in this attack on the similarity problem, the interpolated activity is not a single *task,* as it can be only if it is separate and distinct from the original learning task. In other words, the inter-

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[1] See E. S. Robinson, The 'similarity' factor in retroaction, this JOURNAL, 39, 1927, 297-312. For Skagg's earlier formulation, see E. B. Skaggs, Further studies in retroactive inhibition, *Psychol. Monog.*, 34, 1925, (no. 161), 1-60.

[2] E. S. Robinson, *op. cit.*

[3] T. A. Dreis, Two studies in retroaction, *J. Gen. Psychol.*, 8, 1933, 157-172.

[4] L. Harden, A quantitative study of the similarity factor in retroactive inhibition, *J. Gen. Psychol.*, 2, 1929, 421-432; and N. Y. Cheng, Retroactive effect and degree of similarity, *J. Exper. Psychol.*, 12, 1929, 444-449.

polations of Robinson and Dreis were not activities having an effect (either facilitatory or inhibitory) on the integrity of retention of the primary learning. They were instead continuations of the primary learning in varying amounts, combined with new learning. Likewise, the retention which was measured in these experiments was not retention of a primary learning, but of that *plus* some continuation of it.

The position can be taken, of course, that the retroaction problem need not be defined in terms of discrete primary and secondary tasks. The interpolated activity does not have to be a separate and unique task, the argument might run, since it is precisely the factor of indistinctness and confusion between tasks which is most likely to be the explanation of retroactive inhibition. This hypothetical argument, however, begs the question in favor of a radical form of the "transfer" theory of retroaction. There is as yet no proof that it is the single and complete explanation of the phenomenon.

The theoretical approach of the experiment to be described differs from that expressed by the Skaggs-Robinson function in that retroaction is thought of as one of the problems of the interrelations of tasks—that problem dealing with the effect of a second task on the tested retention of a first. This emphasis on task-characteristics carries with it its own mode of dealing with the similarity factor in retroaction. Two tasks are designated as similar with reference to definite features which the two have in common. Such features would not be independent elements but aspects by means of which two tasks could be compared. There are undoubtedly many such which could be used as a basis of comparison, but certain features of a task will stand out as being more important than others for a genuine description of it and consequently for a significant statement of similarity. The similarity of two tasks, then, will be expressed in terms of the *significant features which the two tasks have in common.*

The two most obvious features of a task are the *operation* which the subject is instructed to perform and the *material* with which he must perform it. The term *operation* will be used to mean that aspect of a task best indicated by the word *Aufgabe,* and the term *material* will be used to indicate what could be called the perceptual data. These two features are not distinct components. They are at least partly interdependent, since a change in the material of a task would be accompanied by some change in the operation, and vice versa. For example, the operation in memorizing nonsense syllables is not the same as in memorizing nonsense forms, although both are 'memorial' in nature.

This distinction is not new. There have been related ones ranging from the distinction between act and content to Tolman's characterization of behavior as goal-seeking and as involving "behavior-supports."[5] Robinson himself, in an earlier experiment, distinguished between the "process" and the "content" of a task and found that "the degree of retroactive inhibition is a function of similarity of process as well as similarity of content."[6] He further demonstrated that another feature, which he called "form of presentation," was important for retroactive inhibition. His data, however, did not show that these features were independently capable of causing retroaction. As a consequence, perhaps, he did not follow up this line of thought in his subsequent research. With the exception of this experiment of Robinson's, all researches on the similarity factor have worked with similarity of material alone. When variations in the operation of the interpolated task have been included in an experiment, this factor has not been specified nor has its effect been isolated and measured. Little is known, then, about the influence of operational or functional similarity on retroaction.

THE EXPERIMENT

In the experiment to be described, our problem was to decide upon a set of interpolated tasks which would enable us to isolate and compare the decrements in retention separately caused by similar *operation* and by similar *material*. The relative importance of the two factors for retroaction could thus be determined. Specifically, we wished to compare degrees of retention of the primary learning in the following cases: (1) when the secondary task is like the primary task in both operation and material; (2) when it is like in operation but different in material; (3) when it is different in operation but like in material; (4) when it is different in both operation and material.

In order to make the above comparisons it was necessary to select a primary learning task and four kinds of interpolated tasks fulfilling the above requirements. On the basis of a preliminary experiment, learning a list of paired consonants was chosen for the primary task. The interpolated tasks were (1) learning another list of paired consonants, (2) learning a list of paired digits, (3) cancelling a specified pair of consonants whenever it appeared in a sheet of pied type, and (4) cancelling a pair of digits in the same manner. A fifth interpolated task, of the sort generally employed as a control or 'rest' condition, was also included. It consisted of looking at pictures (moving-picture 'stills') and it was believed to differ from the primary task even more than did cancelling digits—in fact it was designed to be as different a task as possible. Among other features, it lacked the motivational or 'work' characteristics of the other four tasks.

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[5] E. C. Tolman, *Purposive Behavior in Animals and Men*, 1932, 10 f.

[6] E. S. Robinson, Some factors determining the degree of retroactive inhibition, *Psychol. Monog.*, 28, 1920, (no. 128), 28.

For the sake of simplicity, we shall call the tasks, in order, *Learning Letters, Learning Numbers, Cancelling Letters, Cancelling Numbers,* and *Pictures.* The following schema designates by capital letters the arrangement of tasks in the five interpolated conditions. There were 5 groups of *S*s, one group corresponding to each condition.

Interpolated Condition (Group)	Primary Learning	Secondary Task
I	LL	LL
II	LL	LN
III	LL	CL
IV	LL	CN
V	LL	P

Experimental materials. For primary learning, all groups studied a list of 10 pairs of consonants. No pairs were included which made familiar abbreviations (such as PM); no pair consisted of consecutive letters of the alphabet; and nearly all the consonants of the alphabet were used in one list. For the secondary task, Group I learned another list of consonants made up like the one just described, Group II learned a list of 10 pairs of digits. No digit appeared with disproportionate frequency, nor did consecutive pairs contain a digit more than once. Group III had to read through a sheet of pied consonants striking out the combination KS whenever it appeared. The combination occurred about three times in every two lines. Group IV cancelled a digit combination in the same manner. The pictures secured for Group V were photographs (8 x 11 in.) of dramatic situations obtained from a motion picture distributor.

Procedure. Five laboratory sections of a large course in psychology served as the 5 groups of *S*s. They averaged about 26 *S*s each. In view of the absence of any selective factor in the assigning of students to sections, the groups were probably as nearly equal in ability as any that could be found. The experiment was administered in the manner of a group mental test with the reading of definite instructions by the *E* and emphasis on careful adherence to them. The material for each group was bound in pamphlet form and passed out to the *S*s. The first sheet contained a warning not to open, the second sheet had on it the list for primary learning, the third was blank, the fourth contained the material for the interpolated task, the fifth had a column of 10 short lines for the written recall of the primary list, and for Groups I and II the sixth had another set of lines for the recall of the interpolated list. The pictures for Group V could not be incorporated in the booklets, so they were placed face down on the *S*s' tables before the experiment began.

The experiment was run off in the same way for all groups, apart from the differences in the interpolated activity. Two minutes were allowed for the study of the primary list with a view to reproducing it, in correct order, later. *S*s then turned to the blank page for 30 sec., during which they were given instructions for the interpolated task. Three minutes were allotted to the interpolated task; then the *S*s turned to the fifth sheet and, after brief instructions, wrote as many pairs of consonants from the first list as they remembered during 1½ min. Finally, for Groups I and II, the *S*s turned to the sixth page and reproduced the secondary list. The time-intervals allowed for primary learning, for interpolated task, and for recall, were determined after preliminary experimentation.

Scoring. The following system was used in scoring the recalls written by the *S*s. One credit was given if a consonant-pair was reproduced correctly in the same position which it occupied in the primary list, or if it was reproduced correctly in its relatively correct position (*i.e.* relative to the pair of consonants which preceded it on the list). One-half credit was given if a pair was correctly reproduced in a position one place removed from its correct position (relative or absolute). One-half credit was given if one-half a pair was reproduced in its correct position. No other recall received credit.

TABLE I

SUMMARY OF RESULTS

	Group				
	I LL-LL	II LL-LN	III LL-CL	IV LL-CN	V LL-P
No. Ss	27	22	25	30	28
Range	0–9	0.5–10	0.5–10	3–10	4–10
Av.	3.7	6.2	6.3	7.6	8.6
P.E.$_{av.}$	±.28	±.36	±.38	±.25	±.21

Results. Table I is a summary of the results, showing the number of *S*s in each group, the range of the individual recall scores in each group, the average number of consonant-pairs recalled, and the probable errors of the averages. As one would expect, Condition I, learning a second list of consonants, results in poorer retention than any of the other interpolated conditions. Conditions II and III show about the same average number of pairs recalled. The loss resulting from either common operation alone, or common material alone, seems to be about the same, *i.e.* similarity of operation and similarity of material are apparently of about equal importance in determining decrements of retention.

In Condition IV (LL-CN), where neither operation nor material is like that in the first task, recall is better than it is in the two conditions in which one of these features is similar. This improvement is manifest in both the average recall and the range of the individual scores. Condition V (P), where the interpolated task was simply looking at pictures, gives the best recall of all 5 conditions. Both the average recall and the range of the individual scores show that there is an improvement even over LL-CN, where neither operation nor material were common to the first and second tasks.

Fig. 1 shows graphically the different percentages of the total possible recall which resulted from the 5 interpolated tasks. It should be remembered that the maximum possible recall was 10 consonant-pairs. Using these percentages, we may make the comparisons indicated as the purpose of our experiment. Condition IV (LL-CN) was different from the primary task in both material and operation, and 76% of the maximum recall was obtained by this group. Condition III (LL-CL) was like IV, except that the material was similar to that used in the primary task. Its recall was 63%. Subtracting this from 76% we find a difference of 13 units. Hence we may say that *similarity to the primary task in material only* causes a loss in retention, in this situation, of 13 units of percentage. Condition II (LL-LN) was like IV (LL-CN) in material, but in operation was similar to the primary task. The average recall for this group was 62%. Subtracting from 76% (recall for IV) we find a difference of 14 units. Hence in this condition we may say that *similarity to the primary task in operation only* causes a loss in retention of 14 units of percentage.

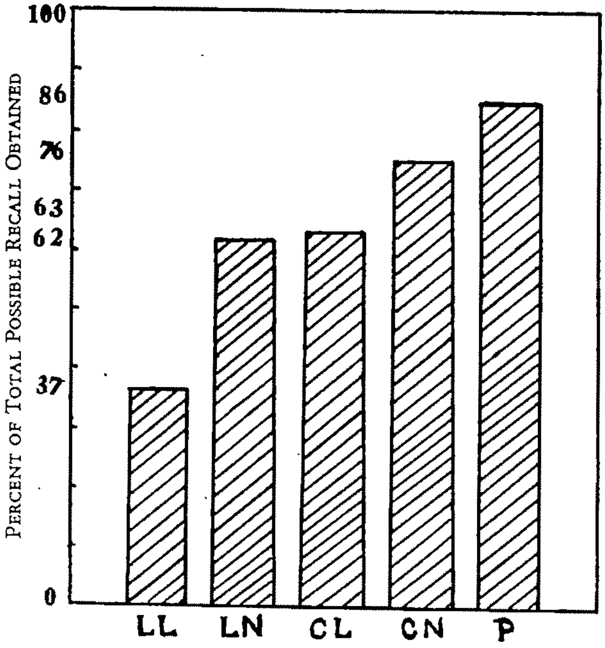

Fig. 1. Effect of Different Interpolated Tasks on Retention of a List of 10 Paired Consonants

Reading from left to right, the five interpolated tasks are LL (learning a second-list of letters), LN (learning a list of numbers), CL (cancelling letters), CN (cancelling numbers), and P (looking at pictures).

Now, if it were assumed that a task is made up of two actually independent and separable components, material on the one hand, and an operation on the other, it might then be expected that with an interpolated task having both operation and material similar to the first task, the loss would be a sum of the losses caused by these two independently. Since there is a loss of 13 with similarity of material and 14 with similarity of operation, the sum of the losses, in this case, might be supposed to be about 27. Actually, however, comparing Condition I (both material and operation in common) with Condition IV (neither in common) the loss is 39 units of percentage. The presumption is that the effect of an interpolated task in which similarity of material and operation is combined is greater than the sum of the effects of two tasks each similar in one of these features respectively.[7] If this inference is correct, it would indicate that the material and the operation within a single task interact, each enhancing to some extent the effect of the other. Consequently one can be separated out only with violence to the other, and in varying them independently, as we have done, the remaining common feature is no longer *as similar as it would be if both remained the same.*

In Condition V (P) the interpolated task was different from the first task in a variety of features. The percentage recalled (86) was greater than in Condition IV (CN), where the interpolated task was different in operation and in material. The gain in V as compared to IV suggests that there are other features than these two with reference to which comparisons may be made, and that one or more of these features, *e.g.* the 'work' attitude, remained common to the primary and interpolated tasks in IV, but not in V.

The above comparisons can be made, with at least equal logic, by examining *gains* in retention as a function of *non-resemblance* of the interpolated task. The gains are 25 units with a difference in material only, 26 units with a difference in operation only, and 39 units with a difference in both. As before, the non-additive character of the latter gain can be explained by supposing material and operation to be interdependent.

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[7] This line of reasoning is valid only if the average retention scores (or percentages) being compared are close enough to the middle of the range of possible scores to be relatively unaffected by the distorting effect of the limits of this range, *i.e.* if the average scores are really proportional to the average degrees of retention. The experimental difficulty in obtaining averages which are proportional is a great source of error in research on retroaction. The writers believe, from an inspection of the distributions of individual scores and the ranges of these scores (Table I), that the averages for Conditions I and IV are not sufficiently distorted to invalidate the above inference.

SUMMARY

Our experiment shows that comparison with reference to *features* of two tasks is useful in understanding the relation between similarity and retroactive inhibition.

It has been demonstrated that under the conditions of this experiment the interpolation of a task which is similar to the primary learning either in *operation* or in *material* results in poorer retention than does the interpolation of a task similar in neither. In this situation, the two features, *operation* and *material,* seem to be about equally important in the effect on retention.

Operation and *material* were chosen as the two most obvious features or characteristics by means of which two tasks might be compared. That these are not the *only* features on which a comparison may be based is suggested by the fact that Condition V, which differed in other respects, gave better retention than Condition IV which differed in the two features of material and operation.

Finally, the original suggestion that features of a task are interdependent seems to be upheld by the probability that the sum of decrements due to *operation* and *material* does not equal the loss caused by similarity in both.

INSTABILITY IN COLOR-NAMING IN NORMAL AND ANOMALOUS OBSERVERS

By DOROTHY KATZIN and ELSIE MURRAY, Cornell University

The data below reported are drawn from a more extended research, directed toward the isolation and grading of marginal cases of aberrant color vision. The effect of instruction and limitation as to color terms to be employed is the special feature here selected for survey.

Throughout the study, emphasis was upon exact standardization of exposure and adaptation factors, through control of illumination, distance, timing, preëxposure and the use of certified materials. Systematic variation of instructions and of position in series rather than repetition under fixed conditions was the order of procedure. Along with the anomalous *Os*,[1] a pair of normal-visioned were utilized, both as a check on the former, and with a view to gathering further evidence against an oft-refuted yet recurrent fallacy—the illusion that 'pure' or 'simple' hues (red, yellow, green, and blue) are intuitively identified as such, while intermediates (orange, olive, peacock, and crimson) spontaneously announce themselves to naïve and sophisticated alike as 'mixed;' a position long and ardently defended by Ladd-Franklin.[2]

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* Accepted for publication October 2, 1933. This phase of the investigation was motivated in part by the desirability of isolating unlike types of 'color weakness,' and determining the nature of their report on the 'dark limits' for yellow signal lights as set by railway engineers.

[1] The term 'anomalous' is here used in its generic sense, to indicate individuals neither normal visioned nor clearly classifiable under the caption 'red-green blind.' It has no reference to the dubious division into protanomalous and deuteranomalous sponsored by Nagel and others on the ground of shifts in the Rayleigh equation toward red or green. In the clinical experience of the senior writer, anomaly appears to be linked to unequal absorption at the two ends of the spectrum, rather than to variable sensitivity in red *vs.* green receptors; possibly also to high thresholds in the red-green mechanism as a whole, rather than to complete abrogation of function.

[2] Christine Ladd-Franklin, Practical logic and color theories, *Psychol. Rev.*, 29, 1922, 182: "The terms unitary color and color-blend explain themselves, and they are indispensable. A unitary red is the same thing as a red which is a psychological element, but that a given red is neither bluish nor greenish is the primary, direct deliverance of consciousness—that it is an element contains the addition of theory." See also Color-introspection on the part of the Eskimo, *idem*, 8, 1901, 396-402, in which there is an attempt to refute Scripture's statement that "this whole seeing of primary and secondary colors and their relation is a matter of education" by reference to Rivers' findings; and *Colour and Colour Theories*, 1929, 8, 25, 36, 120-121, 145 (covering the years 1892 to 1916). In the article on Vision in Baldwin's *Dictionary of Philosophy and Psychology*, 1902, the author refers to "the beautiful reliance upon our power to distinguish, by immediate introspection, between a 'mixed' and a 'non-mixed' color"—a doctrine which she erroneously ascribes to Hering (*ibid.*, 32).

Apparatus. *O* sat in a dark-room, with his head in a headrest attached to the front edge of a modified campimeter or hood. The latter was a semi-cylindrical surface (radius 15 in., height 30 in.), with flat semi-circular roof and floor, and 17-in. wing extensions to right and left. The whole, lined with white blotting-paper, was designed to provide a fairly uniform background coextensive with the visual field. Halfway up, a circular exposure aperture 1 in. in diam. was centered opposite *O*'s pupils. Fixation was binocular.

Two 'daylite' lamps, with bell-reflectors and 200-w. inside-frosted bulbs, were located on either side of *O*'s stool, just within the wings; the rondels opposite and about 30 in. from the exposure aperture. These rondels, 7 in. in diam., were of Corning glass, chosen to produce, with the bulbs specified, the equivalent of average daylight; *i.e.* a spectral energy distribution similar to that of a black body at 6000° K. The illumination was controlled by maintaining the current at 107 v., yielding a reading with the holophane lightmeter of 14 f.c. or 12.05 ml. To insure uniform reflection, a tall medium gray screen was placed behind *O* and the lamps.

Materials. Ten hues, representing both Hering and Helmholtz primaries and intermediates, and arbitrarily designated as yellow, yellow-green, green, blue-green, green-blue, blue, violet, purple, red, and orange,[8] were chosen. Four variants of every hue (a high and low chroma, a tint and a shade) were selected from Munsell Standard Colors. Owing to peculiarities of the Munsell system and its deficiency in certain desaturated colors, completely satisfactory continua could not be obtained; hence data on tints and low saturations are excluded from the discussion following.

The Munsell designations for the saturated and shade stimuli are: *y* = Y 8/12, 7/6; *y-g* = GY 7/10, 5/8; *g* = GYG 6/10, 4/6; *b-g* = G 5/8, 4/4; *g-b* = BG 5/6, 4/6; *b* = BPB 4/8, 3/6; *v* = PBP 5/10, 3/10; *p* = RP 4/12, 2/6; *r* = R 4/14, 3/10; *o* = YR 6/12, 5/8. The numerators refer to value or brightness, the denominators to chroma (the higher numbers indicating lighter colors or stronger chromas).

Each stimulus-color, protected by cellophane, was mounted on a separate card, and exposed behind the circular 1-in. aperture, ordinarily masked by a white blotting-paper shield. The stimulus-size was chosen to subtend at the fixation-distance (15 in.), a visual angle of about 4°, well within, therefore, the macula lutea. The stimulus-cards were filed in slotted boxes for ready handling.

Five neutral grays, *gy* 4, 5, 6, 7, and 8 in the Munsell value scale, approximating the gradations of tint in the colors, were interpolated between the latter to minimize adaptation and after-image effects, and to disclose any tendency in the color-weak to apply color names to what is presumably neutral to them as well as to the normal.

Procedure. (1) Series. Every group of stimuli (saturated, desaturated, tints and shades) was arranged in a continuum, running from *y* through *y-g*, etc., to *o*; the colors alternated with grays of a similar brightness value, as *y*, *gy* 8, *y-g*, *gy* 8, etc. In addition to these four series (Series 1-4) of 20 exposures each, there were two mixed series (Series 5 and 6) of 25 exposures each, in which the four variants for each hue followed in sequence with a neutral (gray) interposed between shade and tint, as *y* sat., desat., tint, *gy* 4, *y* shade, *g-b* shade, *gy* 5, *g-b* tint, desat., sat., etc. Series 5 and 6 were planned to afford *O* opportunity to observe variants of a single hue in close succession, and thus form a concept of hue as such. The hues were ordered, not in a continuum, but in a contrasting sequence.

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[8] Hereafter indicated by *y*, *y-g*, etc.

(2) Instructions. Every *O* observed for four 50-min. periods spaced (with certain deviations) a week apart. In the first period (A, Free Naming) *O* was instructed to use his every-day system of naming. In the second (B, Partial Instruction) he was given as points of reference, 6 color-circle terms (yellow, orange, red, purple, blue, and green) and an ordered series of intermediates (yellow-orange, yellowish orange, orangish red, etc.) making a total of 24 designations for the circle. These were presented by means of a typed card and diagrams. On the third and fourth days (C, Complete Instruction) two circles or continua of 10 hues each (saturated and desaturated) were exposed simultaneously for study and matching; followed (after neutral adaptation) by the two remaining continua (tints and shades).

A 3-min. adaptation-period with head in the rest preceded every set of observations. The 1-sec. exposures of color disks were spaced 12 sec. apart, an interval determined by preliminary experimentation to eliminate noticeable after-images. A period of from 1½ to 2 min. was allowed between series for rest, and for the arrangement of the next series by *E*. To promote uniformity of eye conditions during exposure, the signals "Ready, Wink, Fixate, Name," timed to a metronome beat, were adopted. The series were reversed or their order altered in each day's observations.

(3) Observers. The two normal *O*s (*G* and *B*) were both freshmen, without training in the use of color terms. The two color-deficient (*H* and *R*) were cases of relatively mild red-green defect, chosen for probable blue deficiency and hypersensitivity, respectively. Both gave typical Ishihara results; reporting the left (scarlet) figure in Plates 12 and 13, though distinguishing colored spots to the right, and on many other plates where no figures were discriminable. *H* was a highly trained psychology assistant; *R* unsophisticated, a senior student of forestry.

Results. (1) Total range of color terms for 10 hues. Fifty-four distinct terms were recorded, for a total of 480 exposures. Though many represent relatively fine distinctions, each is set down separately in Table I as essential to the data. For simplicity's sake, however, brightness and saturation qualifications made by *O*s are ignored, except where the term 'gray' or 'grayish' was reported. Most of the 24 gradations recognized in our first (partial) instruction (B), *e.g.* orange-red and orangish red, as well as additional terms, were utilized at one time or other by one or another *O*. Although exposures were spaced to avoid observable after-image effects, a slight shift in color-tone may occasionally have been produced by the preceding stimulus, thus increasing (in view of the variable sequence) the range in nomenclature arising from individual differences in sensitivity, early training, and degree of stabilization of terminology.

Since no marked effects of instruction were discernible, the data have been massed. Results for individual *O*s are also summated, since the division into blue-sensitive and blue-weak was not apparent. Terms unique to the normal pair are italicized, those peculiar to the anomalous enclosed in parentheses. The numbers in parentheses indicate frequencies, for 12 observations a pair.

TABLE I

COLOR TERMS USED BY TWO NORMAL AND TWO ANOMALOUS Os

Stimulus	Terms used by the Os
Yellow:	
Saturated	yellow (18), *orangish-yellow* (3), *orange yellow* (1), (greenish yellow) (1), (yellow-green) (1).
Shade	*yellow* (4), orangish yellow (5), *orange-yellow* (2), yellowish orange (2), (brownish yellow) (1), (greenish yellow) (2), (yellow-green) (2), (yellowish green) (3), (yellow-greenish gray) (1), *mustard yellow* (1), (orangish gray) (1).
Yellow-Green:	
Saturated	yellow-green (12), yellowish green (6), (greenish yellow) (1), *green* (5).
Shade	(yellow-green) (3), yellowish green (14), green (5), *yellowish blue-green* (1), *bluish green* (1).
Green:	
Saturated	green (16), yellowish green (5), (yellow-green) (2), (brownish green) (1).
Shade	green (17), (yellowish green) (3), *bluish green* (2), *olive-green* (1), (orangish gray) (1).
Blue-Green:	
Saturated	*blue-green* (2), *bluish green* (2), green (17), (yellowish green) (3).
Shade	*bluish green* (1), *greenish blue* (1), green (20), (greenish gray) (1), (yellowish green) (1).
Green-Blue:	
Saturated	*green-blue* (1), *greenish-blue* (1), blue-green (4), bluish green (9), green (7), *blue* (1), (bluish gray) (1)
Shade	greenish blue (4), blue green (6), bluish green (5), (blue-greenish gray) (1), green (7), (blue) (1).
Blue:	
Saturated	blue (18), *greenish blue* (3), green-blue (3)
Shade	blue (14), greenish blue (6), *green-blue* (2), (bluish green) (1), (violet) (1).
Violet:	
Saturated	violet (5), *violet-blue* (2), *lavender blue* (2), (blue) (7), purplish blue (3), *blue-purple* (1), *bluish purple* (2), *purple* (2).
Shade	(violet) (5), blue (9), *purplish blue* (2), *blue-purple* (2) *purple-blue* (2) *bluish purple* (2), *purple* (2).
Purple:	
Saturated	*reddish purple* (1), purple-red (2), purplish red (5), violet-red (3), (red) (5), (pinkish red) (1), *orangish red* (1), (pink) (1), *cerise* (2), (rose) (3).
Shade	purple (5), reddish purple (3), purple-red (1), purplish red (3), (red) (1), (reddish gray) (2), wine (2), (blue) (1), (black) (3), (green) (3).
Red:	
Saturated	red (20), *orangish red* (1), (yellowish red) (1), (yellow-red) (1), *scarlet* (1).
Shade	red (16), *orangish red* (1), *yellowish red* (1), (purplish red) (2), *purple red* (1), maroon (3).
Orange:	
Saturated	orange (9), (yellowish orange) (1), orange-yellow (7), (orangish yellow) (1), (yellow) (1), reddish orange (4), *red orange*(1).
Shade	orange (5), (grayish orange) (1), *reddish orange* (3), *red-orange* (3), orangish red (3), yellowish orange (3), (brownish yellow-green) (1), (brownish yellow) (1), (yellowish green) (2), russet (2).

Though possibly synonymous, b-g and g-b are listed separately. A lowercase prefix stands for a modifier, *e.g.* brn G = brownish green. Descriptive terms for neutrals are not listed, though these occasionally were given color names.

(a) Individual differences. The vocabularies for normal and anomalous total 40 and 39 respectively, with 25 of the 54 terms listed common to the two pairs. For individuals, the totals run: *B* 27, *G* 34, average 30.5; *R* 24, *H* 25, average 24.5. The slightly lower average for the anomalous pair may be factitious, a function of their greater practice in laboratory observations on color, with restricted terminology. The untrained normal *O*s under free naming (Instruction A) used many concrete expressions, such as cerise, mustard, russet, which under Instructions B and C were superseded by stereotyped color-circle terms, such as R-P, Y-G, etc. The anomalous, on the other hand, used throughout various compounds with gray or brown, as O-Gy, R-Gy, gy O, brn G; indicating weakened color valences for red and green stimuli, especially marked in the shades. The normals also used the terms violet and purple more freely than did the anomalous, the latter leaning more heavily on yellow.

(b) Overlapping from hue to hue. Overlapping, that is, using *yellow-green* for *y*, *y-g*, and *g* stimuli, occurs frequently, even with the normal pair. Thus, though the group vocabulary is limited to 54 terms, the differing judgments on the 10 hues (saturation and shade) made by the 4 *O*s total 71. The vocabulary per *O* is 27.5, but owing to the application of the same term to one or more near-lying hues (overlap from saturation to shade is excluded) his corresponding array of judgments is 42.2. The number of terms applied to *y* is 11; *y-g*, 5; *g*, 5; *b-g*, 5; *g-b*, 6; *b*, 6; *v*, 7; *p*, 6; *r*, 8; and *o*, 10.

(c) Shades vs. saturations. The total number of terms used by all *O*s in describing shades is 43, as against 38 for saturations. The normal pair, out of their total vocabulary of 40 terms, apply 22 to both shades and saturations, using in addition 9 terms uniquely for each grouping. The anomalous, on the other hand, apply only 14 in common to both groups, reserving 13 uniquely for saturated colors, 11 for shades. In the color world of the anomalous-eyed, it is apparent, differences in saturation and brilliance become individualized perforce, to meet the vocabulary imposed by the normal-eyed, who see differences in hue where the anomalous see intensive differences only. Since desaturated red, green, and yellow stimuli may all appear identical to *O*s of this type, shades of the latter are subject to much fluctuation of judgment, being classified now as greenish, now as reddish (or orangish), rarely as yellow. A certain analogy to this individualization

of desaturated colors by the anomalous is afforded by the normal-eyed, who possessing the differentiated terms *brown* and *tan,* are also apt to individualize and segregate the corresponding stimuli, as every examiner knows; refusing in sorting to place then with saturated yellows or oranges—though rarely or never placing them with reds or greens.

(d) Effect of instruction. The effect of partial and complete instruction on the number of descriptive terms used was slight. There is, of course, a shift in the actual terms employed. There is also, an increase in number of terms employed per stimulus for Instruction B in the three untrained *O*s, with occasional signs of embarrassment in adjusting to a prescribed terminology.[4] The averages for the three methods (Instructions A, B, and C), 2 observations for every stimulus, are close together, being 1.3, 1.5, 1.4 for the normal, 1.5, 1.5, 1.45 for the anomalous. There are, for the latter, as many marked deviations in terminology by one method or instruction as another. Instruction as to the color-circle and the distinctions of saturation, brightness, and hue, and simultaneous exposures of 2 examples each of the 10 hues in continuum failed utterly to dislodge the anomalous from their claim to possession of a distinct quality for red, different from any shade or saturation of yellow. As to green, the two *O*s were dubious, though they continued to use both terms, red and green, in what they conceived to be the conventional fashion, with considerable spread over near-lying hues. Whether further instruction or practice would have induced more radical changes in report is a matter for surmise.

(2) Agreement in terminology. Unanimity is greatest for primaries, probably in part because of greater drill in childhood on these stock colors of the rainbow and the dyer. Of the 10 saturated stimuli, *r* is (correctly) called 'red' 20 times in 24 observations; *b,* 'blue' 18 times; *y,* 'yellow' 18; *b-g,* 'green' 17; *g,* 'green' 16; *y-g,* 'green' 12; *o,* 'orange' 9; *g-b,* 'bluish-green' 9; *v,* 'blue' 7; *p* is called 'red' and 'purplish-red' 5 times each.

In the case of the 10 shades, *b-g* shifts to first place, being described 20 times as 'green' (a rationalized judgment on the part of the color-deficient, to whom this stimulus usually appears as neutral gray, according to their own admissions). The *y* shade drops from third to lowest place; less saturated variants of *y* suggesting 'reddish' or 'greenish' to the second pair, accustomed since childhood to associating the terms 'red' and 'green' with stimuli appearing to them merely as certain dull variants of yellow. Purple continues to occupy a place at the foot of the list. Carrying somewhat unstably balanced valences of yellow and blue, it is described by the color-deficient now as 'green,' now as 'red' (probably both guesses based on its yellow valence); and again as 'black' or as 'blue' *(H)*. An off-spectrum color, its correct title puzzles even the normal-eyed, uncertain whether the term 'purple' applies to a hue bluer or redder than does the term violet. The *v* stimulus accordingly scales low also in unanimity of judgment, the saturated tone scoring highest as 'blue' (*R* and *H*, 7 times); the desaturated shade, 'blue' 9 times (*G*, *R*, and *H*). As to *o*, whether red or green will be read into it by anomalous *O*s is obviously a toss-up.

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[4] Note, for example, the report by a normal *O* of *y-g* shade as *yellowish blue-green,* of saturated *p* as *orangish red*—presumably verbal or logical slips due to momentary mental confusion.

(3) Divergence in terminology. The total number of terms applied to each stimulus-color in the course of 6 observations reveals a similar trend (Table II). In harmony with their scores for unanimity, the saturated primaries show, for the 4 *O*s, the least divergence, *p* and *v* the greatest. In the case of shades, however, *y* heads the list, due to the wide significance of desaturated yellows in the spectrum of the color-deficient already noted.

TABLE II

NUMBER OF DIFFERING COLOR TERMS REPORTED BY 4 *O*s FOR A SATURATION AND A SHADE OF 10 HUES

Stimulus	No. of terms used								Av.
	11	10	8	7	6	5	4	3	
Saturated		p	v	o, g-b		y, r	y-g, g, b-g	b	5.7
Shade	y	p, o		v	r, g-b	y-g, g, b-g, b			7.0

As might be expected, the total for shades is slightly greater than for saturations, the differing judgments for the two mounting to 70 and 57.

(4) Consistency in color-naming in the individual O. Stability through 4, 5, or 6 judgments, under slightly varying conditions, runs higher with the anomalous pair than with the normal. The data for the 20 colors yields 12 instances of consistency (as defined) for the former, as against 9 for the latter. This may be a function of the lesser discriminative capacity in the anomalous; or merely a casual finding, outcome of the small number of *O*s, the presence of an extremely conscientious one in the normal pair, and of a highly trained one in the anomalous. The stimuli on which consistency runs highest are saturated red, and blue-green shade (see below).

The degree of consistency for specific *O*s and colors may also be deduced from the data in Tables III and IV. While the anomalous pair may confine themselves to as narrow a range of terms per hue as the normal (*e.g.* *b-g* and *v* stimuli, 3 terms each), they also show the widest spread (11 and 12 terms on *o* and *p*). The average per hue is close for normal and anomalous, 2.77 *vs.* 2.75 per *O*, or 6.1 *vs.* 5.9 per pair.

Variation in terminology for the primaries from *O* to *O* may be a function of differing color sensitivity or of training. But deviations within the 6 observations of each *O* (see Table III), while possibly the resultant of variation in sequence and instruction, indicate strongly lack of assurance even in normal *O*s as to which are 'pure' colors and which contain a trace (lean toward) those adjacent on the color circle—current illusions as to the immediate and intuitive nature of such judgments notwithstanding. Each color is apparently apprehended as a unity. Qualification with the terms greenish, bluish, reddish, etc., is not a matter of sensory analysis, but of allocating the immediate experience on an imaginary scale. Confusion may actually exist as to whether its position is toward the short or

TABLE III

Number of Terms Per Color Stimulus Used by Normal and Anomalous Os

Stimulus	Normal *O*s			Anomalous *O*s			All *O*s
	B	*G*	Total*	*R*	*H*	Total	
Yellow:							
Saturated	1	3	3	2	2	3	5
Shade	4	4	5	5	4	8	11
Yellow-Green:							
Saturated	3	2	3	3	2	3	4
Shade	2	3	4	2	2	3	5
Green:							
Saturated	1	2	2	3	3	4	4
Shade	1	3	3	3	2	3	5
Blue-Green:							
Saturated	2	3	3	2	2	2	4
Shade	1	3	3	1	3	3	5
Green-Blue:							
Saturated	5	3	6	2	3	4	7
Shade	3	3	4	3	4	6	6
Blue:							
Saturated	1	3	3	2	1	2	3
Shade	1	3	3	3	3	4	5
Violet:							
Saturated	4	3	7	3	2	3	8
Shade	3	3	6	2	1	2	7
Purple:							
Saturated	4	4	7	4	5	7	10
Shade	3	3	5	3	4	7	10
Red:							
Saturated	1	3	3	3	1	3	5
Shade	5	1	5	2	3	3	6
Orange:							
Saturated	2	4	4	4	5	6	7
Shade	4	4	6	4	4	7	9
Totals	51	60	85	56	56	83	126

* Totals do not equal the sum of the other values because of overlapping. For example G used the term applied by *B* to *y*, and in addition two others. Hence 3 color terms were used by the anomalous *O*s.

the long wave end of the spectrum, just as one may be uncertain whether a given tone is higher or lower in pitch than the standard. Hence, even under constant illumination, with minimal adaptation and induction interference, hue judgments may be affected by position in series, and frequently reversed by sequent stimulation. Such alterations were frequently reported by our *O*s, though not entered on the records.

Conclusions: Though the limited number of our *O*s and observations offers a slender base for generalizations, the following are tentatively put forward.

(1) Instability in color-naming is not confined to the anomalous visioned. No immediate sensory analysis and weighting of the component r, g, y, and b valences of a color quality is possible even for the normal-visioned. Use of qualifying terms, or of categories such as blue-green, bluish green, greenish blue, etc., implies previous experience with a graduated color continuum, plus the ability to carry this in mind, and to achieve an imaginary scaling on it of the quality presented. Any actual color experience, immediately preceding or following, is adequate to provoke a shift in judgment in one direction or the other, and this independent of sensory adaptation or induction. In the color-deficient, such allocation of hues is achieved largely by reference to quantitative rather than qualitative continua—*i.e.* those of chroma or brilliance—to the gradations of which, however, individuating qualitative terms are attached.

TABLE IV

NUMBER OF TERMS PER HUE USED BY AN ANOMALOUS AND A NORMAL-VISIONED PAIR OF *O*S
(12 observations a pair per hue)

*O*s	No. of terms							Av.
	12	11	9	6	5	4	3	
Normal			v, p, o	y-g, g-b, r	y	g, b-g	b	6.1
Anomalous	p	o	y	g-b	g, b	y-g, r	b-g, v	5.9

(2) The data for the anomalous pair are plainly in harmony with four-primary interpretations of dichromatic vision. For all their relatively extended vocabularies, the color worlds of the anomalous-visioned are probably highly simplified ones; differences in saturation and brilliance often doing duty for differences in hue. Even after careful instruction and practice, it is true, both anomalous *O*s still incline to the belief that they possess a distinct quality for red, and in fact frequently use this term correctly (as they do also the term green, though on an admittedly reasoned basis). The general drift of evidence, however, and the tendency to characterize many reds and greens as yellows or blues, low saturations of yellow

and blue as reddish or greenish indifferently, render it unlikely that their experience of either red or green is identical with that of the normal-visioned. Except possibly in the case of intensive stimulation or a wide visual angle, their spectrum is largely limited to blue and yellow, in varying degrees of vividness (chroma) and of brilliance. These observations, however, must be checked by others using pure spectral stimuli.

THE PLURIDIMENSIONALITY OF CONSCIOUSNESS

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In his experiment on abstraction Külpe showed that *O*s report far more accurately in terms of an attribute for which they have been determined than in terms of other attributes—indeed, that *O*s are frequently unable to report in terms of any but the attributes involved in the determination.[1] These results have been verified by the subsequent work of Yokoyama, Wilcocks, and Chapman.[2]

There are three classical interpretations of these results, those of Külpe, Rahn, and Titchener.[3] Although they differ in detail, all of these interpretations agree in making the distinction between psychic reality, the *inferred mediate* realm of pluridimensional conscious processes like the sensation, and psychic actuality, the *given* realm of unidimensional conscious entities such as the attribute.

The distinction, however, is by no means strictly implied by the results. Inability to report in terms of attributes other than those mentioned in the instructions may well be merely a difficulty in report, due to the fact that an *O* can only make one statement at a time. Were this so, there would be no further ground for holding that pluridimensional conscious processes—the perceptions and ideas of everyday life—are less immediate, more inferential, than the unidimensional consciousness of abstracted attributes.

This view, ascribing Külpe's results to a limitation of report, derives support from a number of sources. In his *Vorlesungen* Külpe[4] quotes with approval the work of Westphal[5] on a stage of consciousness termed *blosses Gegebensein*—a simple givenness of experience without *Aufgabe* or specific point of view in the consciousness of the passive *O*, who merely receives the impression and can say nothing more than that it is present in his awareness. Titchener's reluctance to give up entirely the notion of sensation as an actual item of experience involved his rejection of Rahn's assumption that limitation of report is proof of a like limitation of experience. Again, Boring, in describing and interpreting Yokoyama's experiment, refrained from definitely limiting actual experience to the content of attributive report. More recently he has made the point that *all* introspection is mediate, that every report is already an interpretation,[6] a view which implies that pluridimensional

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[1] O. Külpe, Versuche über Abstraktion, *Ber. u. d. I Kongr. f. exper. Psychol.*, 1904, 56-68.

[2] E. G. Boring, Attribute and sensation, this JOURNAL, 35, 1924, 301-304; R. W. Wilcocks, An examination of Külpe's experiments on abstraction, *ibid.*, 36, 1925, 324-340; and D. W. Chapman, Relative effects of determinate and indeterminate *Aufgaben*, *ibid.*, 44, 1932, 163-174.

[3] Külpe, *op. cit.:* C. Rahn, The relation of sensation to other categories in contemporary psychology, *Psychol. Monog.*, 16, 1913, (no. 67), 1-131; and E. B. Titchener, Sensation and system, this JOURNAL, 26, 1915, 258-267.

[4] Külpe, *Vorlesungen über Psychologie*, 2nd. ed., 1922, 113 f.

[5] Ernst Westphal, Über Haupt- und Nebenaufgaben bei Reaktionsversuchen, *Arch. f. d. ges. Psychol.*, 21, 1911, 219-434.

[6] Boring, The physiology of consciousness, *Science*, 75, 1932, 32-39.

consciousness (Külpe's psychic reality) and unidimensional consciousness (Külpe's psychic actuality) do not differ in kind, but only in degree. The difference is simply that the inference of pluridimensional consciousness is at present more hazardous than the inference of unidimensional consciousness, because the former requires interpretation of several successive reports while the latter requires interpretation of but a single report.

The next step obviously is to replace, if possible, report in terms of one attribute by a form of report which specifies at once several different attributive features. If it is found that such a form of report can give reasonably accurate results, then the classical interpretation of Külpe's results, with its contention that pluridimensional conscious processes are less immediate than unidimensional ones, will have been shown to rest upon an experimental artifact. The best form of report involving simultaneous specification in terms of several attributes is the report of *sameness* or *difference* upon several objects whose difference from each other can be specified only in terms of several different attributes. The following experiment is essentially a test of the general adequacy of this form of report.[7]

The tachistoscopic apparatus used was constructed for the purpose of presenting a constant fixation-field on which a stimulus may appear for any time-interval desired without superposing the stimulus-colors upon the field. Pasted on stimulus-cards so as to appear in the center of the field were 4 colored papers each 1 in. sq., arranged 1 in. from each other in the shape of a square. They were Munsell colored papers, carefully prepared and specifically calibrated according to the Munsell system in hue, brightness, and saturation.[8] The colors were chosen so that on one card one square differed from a second only in hue, from a third only in brightness, and from the fourth only in saturation. There were 20 stimulus-cards with all the colors differing in this manner, forming a D ("different") series of stimuli with a wide variety of combinations. For each D card a corresponding S ("same") card was prepared in which, however, two of the colors were identical. Forty separate stimuli were available: one test series of 20 cards, with all colors different, and a second control series, with two squares identical. The relationships of hue, brightness, and saturation of a sample pair of stimulus-cards are shown in Table I. It will be seen that the report of difference of all the colors as opposed to identity of two of the squares on the stimulus-cards was a general one, involving three aspects of the stimulus at once.

TABLE I

SAMPLE PAIR OF STIMULUS-CARDS

H indicates hue, B brilliance (on a scale of 0–10), and S saturation (on a scale of 0–14).

Color	D card			S card		
	H	B	S	H	B	S
1	Purple	6	8	Purple	6	8
2	Purple	6	4	Purple	6	4
3	Purple	3	8	Purple	3	8
4	Yellow	6	8	Purple	6	8

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[7] The present experiment was suggested by Dr. J. G. Beebe-Center, for whose constant advice and guidance the author here makes grateful acknowledgment.

[8] *The Munsell Book of Color*, 1929.

The instructions to the *Os* were as follows:

> In this experiment you will be presented with a tachistoscopic visual field containing four colored squares upon a white background. One second before each exposure of the squares the signal 'Ready' will be given by the experimenter.
>
> You are to report whether all four squares are different, or whether two of the squares are the same, your report taking the form *All different* or *Two the same,* respectively.

The 40 stimulus-cards were presented in a predetermined haphazard order which was reversed half the time, and the position of the squares in the exposure-field was shifted through one quadrant at irregular intervals in order to avoid memory effects.

TABLE II

Main Group of Experiments at 100 σ

Observers

Report	*St*			*Go*			*Sw*			*Sp*			*Bl*		
	S	D	Total	S	D	Total	S	D	Total	S	D	Total	S	D	Total
Same	177	98	275	145	80	225	124	68	192	160	94	254	48	23	71
Diff.	23	102	125	55	120	175	76	132	208	40	106	146	32	57	89
	200	200	400	200	200	400	200	200	400	200	200	400	80	80	160
C		.392			.311			.270			.392			.300	
P		.000002			.000000			.000001			.000000			.0012	

The mean square contingency, C, and P,[9] the probability that as good a differentiation between S and D stimulus-cards would be obtained by chance, have been computed for all the results. These statistical measures are presented only as secondary aids in comparing the different results; the more fundamental and primary interest is in the original data themselves. The best way to appreciate the significance of the results is merely to contrast the distribution of reports of *Same* and particularly of *Different* for S and D stimulus-cards.

Such an examination of the results with a presentation-time of 100 σ (Table II) shows a marked differentiation in report between S and D stimuli for every *O;* the obvious adequacy of report to the stimuli is further shown by the size of C and the extremely small P. The primary problem of this paper is answered by these results: general judgments of sameness or difference adequate to several aspects of the stimulus at once can be made. This conclusion is contrary to that of Külpe and Yokoyama, who held report to be adequate only to a single aspect at a time.

At this point the objection might be raised that the successful discrimination between S and D cards is based on successive judgments of different parts of the stimulus-field, since the four colors are never in consciousness together at any one time. As the time of presentation is too short for the original perception to be subjected to such an analysis, the successive comparison may be thought to be based on after-images of the colors. To test this point, a flickering fixation-field was introduced immediately following the presentation of the stimuli in order to reduce such after-images. Inspection of the resultant data (Table III) shows no loss of discrimination, even though after-images were largely eliminated. A series of observations was also made with a lengthened exposure-time of 150 σ to facilitate judgments based on a successive analysis of the different parts of the field. Again, comparison of the results of this series (Table IV) with those given in Tables II and III shows no increase in the adequacy of report. Moreover, introspective statements that the report is based upon a single, immediate perception of the stimulus and that the time is too short to allow anything like a successive analysis of the field support the incompatibility of the experimental data with the 'successive-part-judgment' view and permit us to reject this objection as invalid.

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[9] C was computed according to the formula given by G. U. Yule, *An Introduction to the Theory of Statistics,* 1927, 65. It must be remembered that C varies between the limits 0.000 for no correlation and 0.707 for a perfect correlation. In every case the correlation is positive.

P is derived from X^2 computed by the product-moment correlation formula. Cf. K. Pearson, *Tables for Statisticians and Biometricians,* 3rd. ed., Part I, xxxiv, and Table XII.

TABLE III

Series with Flickering Fixation-Field

Observers

Report	*St*			*Go*			*Sw*			*Sp*		
	S	D	Total	S	D	Total	S	D	Total	S	D	Total
Same	78	45	123	71	46	117	66	36	102	91	62	153
Different	2	35	37	9	34	43	14	44	58	9	38	47
	80	80	160	80	80	160	80	80	160	100	100	200
C		.439			.333			.363			.324	
P		.000000			.00017			.00002			.00004	

The additional data presented in Table V for *Go* with exposure-times of 125 σ and 50 σ show the rôle of the presentation-time in the perception of the stimulus-situation. The results at 125 σ (the time Külpe used in his abstraction experiment) are very similar to those of 150 and 100 σ. Within these limits, then, differences of exposure-time seem to have little effect. The results obtained at 50 σ, however, are the only ones in the whole experiment in which the adequacy of report becomes doubtful; here the small number of reports of *Different* makes the differentiation of S and D stimuli uncertain, as the relatively high value of P shows. Fifty sigma appears to be near the temporal limen for these judgments, probably because it is

TABLE IV

Series with Exposure-Time of 150 σ

Observers

Report	*St*			*Go*			*Sw*			*Sp*		
	S	D	Total	S	D	Total	S	D	Total	S	D	Total
Same	97	67	164	95	69	164	85	44	129	70	48	118
Different	3	33	36	5	31	36	15	56	71	10	32	42
	100	100	200	100	100	200	100	100	200	80	80	160
C		.364			.310			.394			.298	
P		.000001			.00004			.000000			.0014	

near the ***Anklingen***-times of the different colors. The exposure-time must, then, be long enough for the colors to reach their full value, and must be short enough not to permit successive comparisons of different parts of the field.

A fourth set of observations, with instructions for report based upon the immediate phenomenal content of consciousness, was also carried out to establish even more completely the immediacy of the significant differentiation of S and D cards.

TABLE V

SERIES WITH DIFFERING EXPOSURE-TIMES

O=*Go*

Report	Exposure time					
	125σ			*50σ*		
	S	D	Total	S	D	Total
Same	57	28	85	75	69	144
Different	23	52	75	5	11	16
	80	80	160	80	80	160
C		.342			.124	
P		.0001			.48	

The resultant data, given in Table VI, show a relatively poor, but still significant, discrimination of S and D cards. This decrease in the adequacy of report can be accounted for, in part at least, by difficulties of the attitude demanded. *Go,* for example, said that the attitude was a disagreeable and distracting strain; *Sp* responded by fixating the margin of the stimulus-field and looking at the stimuli only casually; *Sw* found an increasing tendency to report *Same* when two squares looked at all similar. Because of this tendency, the *Same* reports exceeded the *Different* reports in number throughout the experiment. This preponderance of *Same* reports must be viewed as a disturbing factor which reduces the apparent significance of the results, although it is really external and not significant for the experiment. This constant tendency resulted from the attitude towards *Same* as an easier and more decisive report, since this report follows immediately the perception of only two squares as identical; whereas the report of *Different* involves all the squares and might result from an imperfect perception of some of the colors.

TABLE VI

SERIES WITH SPECIAL ATTITUDE

Report	Observers														
	St			*Go*			*Sw*			*Sp*			*Pr*		
	S	D	Total	S	D	Total	S	D	Total	S	D	Total	S	D	Total
Same	78	55	133	78	66	144	80	66	146	79	62	141	50	28	78
Diff.	2	25	27	2	14	16	0	14	14	21	38	59	10	32	42
	80	80	160	80	80	160	80	80	160	100	100	200	60	60	120
C		.334			.222			.296			.183			.246	
P		.000035			.0186			.0016			.07			.0005	

All the results presented in Tables III-VI substantiate the conclusion drawn from Table II that, where differentiation of stimuli is necessarily based upon physical variations involving three different attributes, such differentiation is possible and can be obtained quickly and directly by single general judgments.

Two interpretations of this basic result of the experiment are possible. The first is that there is nothing more in the experience than the knowledge of *sameness* or *difference* of the stimulus. The stimulus-field of 4 colors differing in so complicated a manner is too rich and complex to be given in immediate experience; the rich pluridimensional basis of the report is rather present at some lower level than consciousness, perhaps at a purely physiological level of excitation. From such a physiological pattern emerges as conscious only the datum of report, the mere knowledge of *sameness* or *difference.*

A second, more liberal interpretation of the result is, however, more in accord with the views developed in this paper: since report is adequate to several attributes of a stimulus at once, all these attributes may be inferred to characterize the consciousness of the *O.* This view maintains that the basis of the report is psychological and experiential rather than unconscious and physiological; from the adequacy of report for the three attributes of the stimulus may be inferred the actual existence of a rich pluridimensional experience which is not ordinarily represented completely in explicit report.

The latter view is definitely favored by the introspective evidence obtained from all the *Os* that the judgments are usually based on a perception and conscious givenness of all the colors as such, and that only rarely, after much practice, do reports ever arise without experiential awareness of the basis of the report. Although all the *Os* definitely agree in this respect, the point is most clearly expressed in the following introspections:

> There is no immediate presentation of sameness and difference as a mere relation without awareness of the terms; there is a presentation of the colors, and from a perception of them arises the judgment of *Same* and of *Different.*
>
> There is no elemental difference as such given; I see *colors.* These colors are not given in words at the time of report but they are *'there.'*
>
> In almost every case the judgment is made before the flicker started [practically instantaneously]; but there is still some kind of intermediate process like apprehension or absorption of the experience—a bringing of the experience to the 'mental eye.'

Whether the differentiation be merely physiological or it be also represented in experience, the implication of the results with respect to Külpe's experiment are much the same. If a highly positivistic point of view is taken, it may be said that Külpe merely showed that under his conditions discrimination was limited mainly to one aspect of the stimuli; from the same point of view this experiment shows that under other conditions discrimination may simultaneously involve several such aspects. With more freedom of interpretation, Külpe may be considered to have shown that under his conditions discrimination is practically limited to a single conscious attribute; from this latter point of view, the present experiment shows that discrimination may simultaneously involve several different conscious attributes. In either case it is clear that there is no longer any ground for attributing less immediacy to pluridimensional consciousness than to undimensional consciousness. Both must be equally real for him who emphasizes, as does Boring, the inferences involved in establishing them; both must be actual for him who emphasizes the fact that they are supposed to be replicas of, to stand for, the original datum.

TRANSFER OF TRAINING IN READING BRAILLE

By WILBUR S. HULIN and DANIEL KATZ, Princeton University

This study presents a measurement of the ability to read the Braille alphabet with an untrained hand after the alphabet has been learned through the use of the other hand, or has been learned visually. No previous study, to the authors' knowledge, has investigated this special problem of the transfer of training in reading Braille, although other types of transfer of tactual discrimination have been reported.

Volkmann claimed that practice in judging the two-point threshold on the left arm improved the discrimination on the right arm in bilaterally symmetrical places, but not in others;[1] also practice with one finger improved all the fingers, but not the arm.[2] Dresslar[3] made similar claims concerning the transfer of improvement in dual limens from one arm to the symmetrical part of the other. Mukherjee[4] found that non-corresponding areas are improved as much as symmetrical parts following practice of two-point judgments in a given part. Friedline[5] has argued, however, that the reduction in size of the two-point limen is due to a shift in the interpretation of the cutaneous patterns on which the judgment of duality is based, rather than to an improvement through practice. Becker[6] applied simple geometrical forms to the palm of the right hand and after a period of training to the left hand. Some *Ss* received only tactual cues, others verbal or visual hints. Becker reports that "for the entire group the average number of presentations required for learning on the right palm was 8.96, on the left palm, 3.94. The difference of 5.02 presentations in favor of the left palm was 7.4 times its sigma." At a later time Becker retested the right palm and also applied the forms to the right forearm. She reports that the right palm "required an average number of 6.29 presentations for the entire group, the forearm 12.28 presentations; the difference of 5.99 presentations was 11.5 times its sigma."

PROCEDURE

The Braille alphabet seems to be suitable material upon which to base a study of transfer because of the following reasons: (1) it is not familiar to most seeing people; (2) it furnishes a number of units which are for the most part of fairly equal difficulty; (3) the measure of learning and transfer is based upon the number of letters correctly named—such a measure seems more precise than that based upon

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[1] A. W. Volkmann, Über den Einfluss der Übung auf das Erkennen räumlicher Distanzen, *Ber. d. sächs. Gesell. d. Wiss., math.-phys. Cl.*, 10, 1858, 63.

[2] *Ibid.*, 66 f.

[3] F. B. Dresslar, Studies in touch, this JOURNAL, 6, 1894, 328-330.

[4] K. C. Mukherjee, The duration of cutaneous sensation and the improvement of its sensible discrimination by practice, *J. Exper. Psychol.*, 16, 1933, 340-342.

[5] C. L. Friedline, The discrimination of cutaneous patterns below the two-point limen, this JOURNAL, 29, 1918, 414-415; 419.

[6] F. Becker, Tactual learning and transfer of geometric forms, *Clark Univ. Thesis Abstr.*, 3, 1931, 107-109.

the varying judgments of the two-point limen;[7] and (4) the reading of Braille may be considered as a perceptual process which is limited to certain bodily regions and is thus a stricter test of transfer than one based upon motor tasks which involve general postures and trunk responses.

METHODS AND RESULTS

Two methods were used in the present study. The first consisted of testing the transfer of training from one hand to the other. The second consisted of testing the ability to read tactually after a period of visual training. Each method and its results will be discussed separately.

Method I. Transfer from one hand to the other. The index finger of one hand was trained until *S* could read every letter of the Braille alphabet, then a test was made with the index finger of the other hand. Of the 5 *Ss* used in Method I, 4 who were right-handed were trained with the right hand, and the 5th, left-handed, was trained with the left hand.[8] The letters of the alphabet were presented on separate cards, on each of which a single letter was embossed by the use of the standard Braille slate. The *Ss* were blindfolded.

On the first day of training, *S* was presented with the Braille characters, one at a time, in alphabetical order, and allowed 15 sec. for the tactual exploration of each letter. This was the only presentation of the alphabet in order ever given to *S*. Immediately following this introductory exploration, at the same sitting, the letters were presented in haphazard order and *S* was given a time limit of 30 sec. in which tactually to recognize and name the letter. If the *S* responded correctly *E* said, "Correct," and proceeded to the next letter. If *S* responded incorrectly or failed to respond at all within 30 sec., *E* gave the correct name of the letter and then allowed *S* a 5-sec. period in which to make a further tactual exploration. This *prompting* method was used throughout the training series.

On each day thereafter one presentation of the alphabet, in haphazard order, was given according to the prompting method.

The test of transfer was immediately taken in the same sitting at which *S* first named every letter correctly within the time limits. The test consisted of the attempt to recognize the letters, presented in haphazard order, with the index finger of the other hand. The blindfold was then discarded and *S* was asked to draw the Braille characters as he imagined them to look.

Results of Method I. Table I shows the number of trials that were necessary for the recognition of the alphabet and the number of letters recognized per trial in the training series for each *S*; also, the number of letters recognized in the test presentations to the untrained hand.

In Table I it may be seen that there was almost complete recognition of the alphabet in the test presentation to the untrained hand. The *Ss* (*T* and *B*) who learned the alphabet most quickly correctly recognized all the letters in the test presentation; *H* and *C* with longer learning periods failed to recognize 3 and 2 letters, respectively; and *M*, the *S* with the longest learning period, was given a

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[7] Cf. E. G. Boring, The stimulus-error, this JOURNAL, 32, 1921, 465-470.

[8] There is no agreement regarding the relative ability of the two hands. Cf. J. M. Smith, The sensory function of the non-preferred hand, *J. Exper. Psychol.*, 16, 1933, footnote, 271 f.

TABLE I

Showing the Number of Trials in the Tactual Training Series and the Number of Letters Recognized per Trial, together with the Number of Letters in the Test Series Recognized by the Untrained Hand

S	Training series: Trials																Test series: Trials				
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	1	2	3	4	5
T	11	16	19	22	23	25	24	26									26				
B	8	9	8	10	14	16	15	23	22	24	26						26				
H	10	16	16	15	22	16	16	23	21	24	22	23	23	21	26		23				
C	10	8	13	15	14	18	18	22	20	18	22	23	23	24	25	26	24				
M*	4	6	8	13	11	20	19	14	19	20	24	23	18	21	20	23					
	19	23	24	22	25	25	22	19	23	23	24	22	24	24	26		23	22	20	23	26

* M required 31 trials in the training series; because of this a series of test presentations was given until all the letters were recognized.

TABLE II

Showing the Number of Trials in the Visual Training Series and the Number of Letters Recognized per Trial, together with the Number of Letters in the Test Series Recognized Tactually

S	Visual training series: Trials												Tactual test series: Trials								
	1	2	3	4	5	6	7	8	9	10	11	12	1	2	3	4	5	6	7	8	9
R	19	25	26										21	19	22	24	26				
C	22	21	24	26									17	25	25	24	26				
P	22	22	24	25	23	26							10	18	18	19	19	18	22	23	26
T	15	17	18	16	24	24	24	23	25	26			14	22	21	22	24	26			
H	11	16	16	16	22	19	21	24	21	23	25	26	16	22	20	23	25	24	26		

series of test presentations in the first of which he failed to name 3 letters correctly but in the fifth trial he recognized them all.

Following the test presentations the *S*s were asked to draw the letters. The result was that the simpler letters were quite accurately reproduced, but some of the more complex letters were always drawn incorrectly. Three of the *S*s insisted upon making two sets of drawings, one set to show what they thought the stimulus must really be like, and a second set to show upon what they actually based their judgments. In Fig. I are shown copies of some of these drawings.

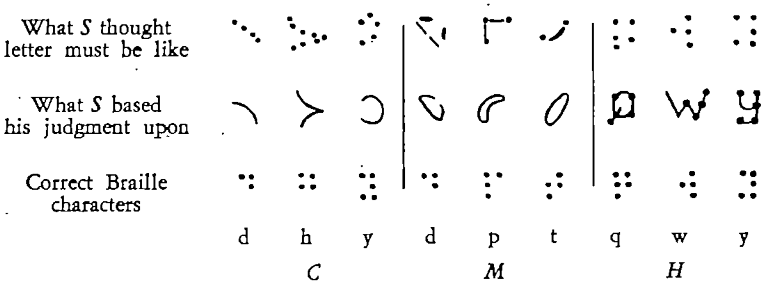
What *S* thought letter must be like
What *S* based his judgment upon
Correct Braille characters
d
h
y
d
p
t
q
w
y
C
M
H

Fig. I. Showing Graphic Recall of Some of the Letters

Method II. Transfer from visual to tactual reading. The extent to which tactual perception might be based upon a previous visual training was tested by the second method. Five new *S*s were visually presented with the Braille letters, each letter printed on a separate card. On the first day of training each *S* was shown the letters one at a time in alphabetical order and allowed 15 sec. to look at each letter. Immediately afterward, at the same sitting, a haphazard order of presentation was given with 30 sec. allowed for visual recognition of each letter. *S* was told if his response was correct, or if incorrect he was given the correct name of the letter and allowed 5 sec. for further visual inspection. This prompting method was repeated once daily until *S* made one errorless performance. Immediately following the errorless performance, at the same sitting, *S* was blindfolded, presented with the embossed Braille cards, and asked to recognize them tactually with the index finger of the right hand. He was allowed 30 sec. in which to name the letter correctly. If he failed he was told the name of the letter and given 5 sec. for further tactual exploration. The tactual presentation was repeated once daily till *S* named all the letters correctly at one sitting.

Results of Method II. Table II shows the number of trials that were necessary for the recognition of the alphabet and the number of letters recognized per trial in the visual training series for each *S*; also, the number of trials and number of letters per trial for tactual recognition in the test series.

From Table II it may be seen that fewer letters were recognized tactually in the first test presentation than had been recognized in the corresponding test presentation in Method I. Nevertheless more letters were recognized in Method II in the first test presentation than had been recognized in the very first trial of the tactual training series in Method I. The average number of letters recognized in the test presentation of Method II was 15.6; the average number recognized in the test presentation of Method I was 24.4; and the average number in the very first trial of the training series of Method I was 8.6.

CONCLUSIONS

There is a ready transfer from one hand to the other in tactually reading the Braille alphabet.[9] The transfer of the tactual reading ability is greater from a tactual training than from a visual training, which indicates that the transfer is not carried wholly in terms of visual imagery.[10]

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[9] K. Bürklen hints at another transfer of Braille in the following simple footnote: "It should be mentioned that, strange as it may seem, the blind can read also with the toes." *Touch Reading of the Blind* (trans. by F. K. Merry), 1932, 10.

[10] R. V. Merry (An experiment in teaching tactual reading to seeing subjects, *Ped. Sem.*, 39, 1931, 413) who taught two seeing adults to read Braille, concluded that they "obviously interpreted their tactual sensations into visual images."

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

LXIV. Retinal Rivalry[*] in Free Vision of a Solid Object

By M. F. Washburn and Priscilla Manning

In the course of experiments on retinal rivalry as experienced in looking through a stereoscope at slides representing outline figures of cones and pyramids, it occurred to the senior author of this paper that rivalry should be observable in ordinary vision of solid objects. She accordingly set about watching for it, and no sooner was it looked for by fixating some point in the surroundings beyond which the views of the two eyes differed, than it almost invariably appeared except in cases where the field of one eye had greater attention value than that of the other. The present paper is a report of observations under experimental conditions.

They were made in a dark room. The objects used were two wooden cubes, one 1.3 in. a side, the other 2.6 in. a side, painted black, with one side smoothly covered with white paper. The *O* sat with her head in a rest before a shelf. One of the cubes was placed on the shelf on a level with her eyes and at a distance of 10 in. from them; it was turned so that one edge was nearest the *O* and the white side was on the right; thus the right eye saw the whole of the white side and the left eye saw only the right half of the white side. A fixation-point was marked half-way up on the right edge of the white side. A small electric torch was used to illuminate the white side of the cube. To time the fluctuations, a metronome was started beating at 120 per sec. *O* looked steadily at the fixation-point and observed the alternations of the broad and narrow white fields; she said 'Out' when the broad field was seen and 'In' when the narrow field took its place. *E* made short pencil marks in time with the metronome ticks, vertical during the 'in' phase and horizontal during the 'out' phase. Each observation lasted for 2 min. Eight observations were made at a sitting, 4 with the large cube and 4 with the small one, the order differing for different *O*s so that practice and fatigue conditions should be equalized for the two cubes. The duration of the phases in half-seconds could be read from *E*'s records.

No *O* experienced any difficulty in observing the rivalry; in fact, we found that anyone could observe it under ordinary daylight. This fact, together with other considerations bearing on the possible function of retinal rivalry in ordinary binocular vision of solid objects, was reported to the National Academy of Sciences on April 25, 1933.[1] The present paper merely supplies, as a matter of scientific decorum, some numerical data from observations under experimental conditions.

There was no reliable difference between the rate of fluctuation in the small cube and that in the large cube. For the 21 *O*s the median length of a phase in the large cube was 3.83 sec., with a range from 2.78 sec. to 10 sec. The median length

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[*] Accepted for publication November 7, 1933.

[1] M. F. Washburn, Retinal rivalry as a neglected factor in stereosopic vision, *Proc. Nat. Acad. Sci.*, 19, 1933, 773-776.

of a phase in the small cube was 3.825 sec., with a range from 2.75 sec. to 12.995 sec. The rank difference *r* between the large cube rates and the small cube rates was + 0.858 P.E. 0.04. Sixteen of the 21 *O*s found the duration of the 'out' phase, that is, the field of the right eye, longer than that of the 'in' phase, that is, the field of the left eye, the ratio of average 'out' duration to average 'in' duration for a single *O* ranging from 2.81 to 1.14: the other 5 showed longer dominance of the 'in' or left phase, the ratios ranging from 3.41 to 1.03. This is consistent with the greater frequency of right eye dominance in individuals.

Conclusive evidence that the fluctuations were those of rivalry was furnished by the fact that when the eyes were closed at the end of a period of observation the negative after-images of the broad white field and the narrow white field appeared as black bands and alternated: no possible extraneous influence such as shift of fixation could operate here.

The senior author of this report has been unable to find any reference in the literature to observations on rivalry in the ordinary vision of solid objects, but it seems highly improbable that it has been wholly overlooked.

LXV. A Comparison Between the Miles A-B-C Method and Retinal Rivalry as Tests of Ocular Dominance

By M. F. Washburn, C. Faison, and R. Scott

Ocular dominance, the fact that a given individual is usually either right- or left-eyed, the right or left visual field tending to remain in consciousness longer than the other one, may be easily observed in experiments on retinal rivalry, where by means of a stereoscope, for instance, each eye is presented with a different field and the observer sees the two alternately. If the time during which each field appears is measured, it will often be found regularly and markedly longer for one eye, the right or the left, than for the other, the individual thus being revealed as either right- or left-eyed.

Professor Miles has recently introduced a simple test for ocular dominance, which he calls the A-B-C Test.[1] It is based on suggestions from the work of Parson[2] and of Dolman.[3] The person to be examined must not know the purpose of the test. He is told to keep both eyes open, and that the object is to test his "sharpness and quickness in seeing."[4] The examiner stands 10 feet away from the subject, who is given "a paper funnel or V-Scope that is made of folded white cardboard formed in such a manner that it *must be pressed open between the two hands* if the subject is to see through the aperture,"[5] which is large enough only for one eye to look through it. He is told that he must press the V-Scope open with both hands and, on getting a signal, look through it at a card which the examiner will expose, and report which of two spots printed on the card is larger and which is darker. The card is exposed for about 2 seconds. A set of 10 cards is used, each of which in the test for adults has printed on it two round spots differing in size and in shade of filling. *E*'s true purpose, however, is to observe and record with which eye *S* looks through the funnel.

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[1] W. Miles, Ocular dominance demonstrated by unconscious sighting, *J. Exper. Psychol.*, 12, 1929, 113-126.

[2] B. S. Parson, *Lefthandedness: A New Interpretation*, 1924.

[3] P. Dolman, Tests for determining the sighting eye, *Amer. J. Ophth.*, 2, 1919, 867.

[4] Miles, *op. cit.*, 119.

[5] Miles, *op. cit.*, 117.

In preparing to make an experimental comparison between the Miles Test and retinal rivalry as measures of ocular dominance, it occurred to the senior author of this study that perhaps some disturbing influence might be exerted by the fact that a fixation-point on a stereoscopic slide is much nearer the observer than is the case in the Miles Test, where the object to be looked at is 10 ft. distant. It also appeared possible that if the Miles Test were used with a near fixation, the cards being held just at the further end of a shortened cone, its results might be different from those obtained at the 10-ft. distance. Might not the eye dominant for far fixation be the less dominant one with near fixation? With this possibility in mind, we divided the Miles Test into halves, one with far, the other with near fixation. The results however show no greater agreement between rivalry and the Miles Test than when near fixation was used.

The entire proceeding was as follows. In Part I, the Miles Test was given to *O*, following the directions prescribed by its author exactly except that only 5 of the cards were used. Part II was the rivalry test. In a dark room *O* sat with her chin in a comfortably adjusted headrest under a 50-w. electric bulb, and held a stereoscope carrying a card one of whose halves was covered with Bradley saturated red paper and the other with Bradley saturated blue paper. *O* kept her eyes closed until *E* told her to open them. *E* sat at a side table with pencil and large sheets of paper, and started a metronome beating at 120 per sec. She then gave *O* the signal 'Now,' at which *O* opened her eyes and reported whether the slide looked predominantly blue, red, or purple. *E*, as soon as *O* reported, began to make short pencil marks in time to the metronome beats, vertical when *O* said 'Blue' and continuing thus until *O* said 'Red,' when the marks were changed to horizontal, or 'Purple,' when dots were substituted for dashes. This proceeding lasted 3 min. *O* then rested her eyes as long as she felt it necessary, and a second observation was made in the same way, except that the red and blue card was turned over so that the red and blue changed sides, the factor of *color* dominance being thus eliminated.

In Part III, the Miles Test was given with the remaining 5 cards of the set, and with the pasteboard V-Scope shortened to half its original length and the cards held at a distance of 18 in. from *O*'s eyes.

O's ocular dominance in rivalry was expressed in the following way. The total number of half-seconds during which each eye's field dominated was counted, and to each of these numbers there was added half the number of half-seconds during which purple, indicating binocular fusion, was seen. Each of these sums was expressed in terms of a percentage of the total time of observation. The amount by which one of these percentages exceeded the other represented the amount by which one eye was dominant over the other.

These time measurements have of course inaccuracies due to the fact that the shifting from one retinal field to the other is not instantaneously complete, but more or less gradual, and that the binocular purple does not remain constant. For this reason we did not consider monocular dominance marked unless the difference between the two percentages was at least 20; if it was between 10 and 20 it was recorded as slight dominance, and if less than 10 it was called no dominance.

There were 57 *Os*, all young women college students. Of these 19, or 33.3%, showed complete consistency between the results of the Miles Test and those of the rivalry test, 3 of these *Os* displaying dominance of the left eye, complete in the Miles Test and over 20% in rivalry; the other 16 similarly marked dominance of the right eye in both tests.

As the results from those *Os*, comprising two-thirds of the group, with whom there was disagreement between two tests were carefully examined, the possibility appeared that handedness had something to do with the inconsistency. Forty-eight of our *Os* were right-handed. Of these 21, or 43.75%, showed more dominance of the right eye in the Miles Test than in the rivalry test. That is, 5 *Os* showed complete R eye dominance in the Miles Test and partial R eye dominance (between 10% and 20%) in rivalry. Nine *Os* showed complete R eye dominance in the Miles Test and no dominance (less than 10%) in rivalry. One *O* showed complete R eye dominance in the Miles Test and partial L eye dominance in rivalry. Three *Os* showed complete R eye dominance in the Miles Test and complete L eye dominance in rivalry. One *O* showed partial R eye dominance in the Miles Test (at 10 ft. distance, 4 R, 1 L; at 18 in., 3 R, 2 L), and complete L eye dominance in rivalry; one *O* showed partial R dominance in the Miles Test (at 10 ft., 1 L, 4 R; at 18 in., all R), and partial L eye dominance in rivalry; one *O* showed partial R eye dominance in the Miles Test (at 10 ft., 3 R, 2 L; at 18 in., 3 R., 2 L), and no dominance in rivalry.

Among the right-handed *Os*, there were 8 who showed less R eye dominance in the Miles Test than in rivalry.

Of these, 4 showed complete L eye dominance in the Miles Test and no dominance in rivalry; 1 showed complete L eye dominance in the Miles Test and complete R eye dominance in rivalry; 1 showed partial L eye dominance (at 10 ft., 3 L, 2 R; at 18 in., all L) in the Miles Test and no dominance in rivalry; 2 showed partial R eye dominance in the Miles Test (in one case, at 10 ft., 1 L, 4 R; at 18 in., all R; in the other case, at 10 ft., all R; at 18 in., 4 R, 1 L) and complete R eye dominance in rivalry.

There were 6 completely left-handed *Os* and 4 who were left-handed for certain functions. In 6 cases, or 60% of this group, the two tests agreed perfectly: 3 of the left-handed *Os* (2 completely and one partially left-handed) showed R eye dominance, and 3 (one completely and 2 partially left-handed), showed L eye dominance in both tests.

Of the remaining 4 left-handed *Os*, 2 showed more L eye dominance in the Miles Test than in rivalry. In both cases they showed complete L eye dominance in the Miles Test and no dominance in rivalry. Both these *Os* were completely left-handed. Of the other two *Os*, one completely and the other partially left-handed, both showed less L eye dominance in the Miles Test than in rivalry; the first having complete R eye dominance in the Miles Test and partial R eye dominance in rivalry, the other complete R eye dominance in the Miles Test and no dominance in rivalry.

Thus of the right-handed persons, the number showing more right eye dominance in the Miles Test than in rivalry was 21; the number showing more right eye dominance in rivalry than in the Miles Test was 8. Of the completely left-handed persons, the number showing more left eye dominance in the Miles Test than in rivalry was 2; the number showing more left eye dominance in rivalry than in the Miles Test was 1. In the partially left-handed group the figures for 3 *O*s were the same for both tests. The fourth partially left-handed *O* showed slightly less dominance in the Miles test than in rivalry.

Professor Miles writes of his test that its results show "little interference from handedness,"[6] and that "by using a folded cone that must be pressed open between the hands the results are freed in the main from prejudice by the handedness of those tested."[7] The present investigation indicates, so far as the right-handed group is concerned, that the Miles Test does contain the factor of handedness, since right-handed persons are much more likely to appear right-eyed in the Miles Test than when tested by retinal rivalry. The Miles Test is simpler to administer, but the rivalry test is quite possibly freer from the handedness factor and certainly gives a finer scale of measurement, since the results can be expressed in the difference between percentages of time during which each of the fields is dominant. It takes only a little over 10 min. and a dark room is unnecessary.

LXVI. The Comparative Controllability of the Fluctuations of Simple and Complex Ambiguous Perspective Figures

By M. F. Washburn, C. Reagan, and E. Thurston

In a recent study from this laboratory a comparison was made between the efficacy of attempts to control voluntarily the duration of phases in retinal rivalry and in the ambiguous perspective cube.[1] The results indicated that the control was exercised by motor processes, full and incipient. The present report investigates the relative efficiency of attempts to control the duration of phases in three ambiguous perspective figures differing in complexity. These were the cube, the staircase, and the book, with its middle vertical line placed higher than the two side lines. The lines were drawn in black ink on squares of white cardboard, 20 cm. a side. The vertical and horizontal lines of the cube, the vertical lines of the book, and the outside vertical lines of the staircase figure were 5 cm. long, the upper horizontal line of the staircase figure was 8 cm. long and there were 4 steps each 1 cm. high. The oblique lines of the book were 2.5 cm. long.

Method. There were 60 Os, each of whom performed the observations twice, the sittings being on different days, and at the same hour to keep general fatigue reasonably constant. They were held in a dark room. *O* sat with her head in a rest; the card with the cube was placed against the wall 60 cm. distant; a 50-w. light at a constant height above *O*'s head strongly illuminated the figure. *E* sat at a side table with pencil and paper, a stop-watch, and a metronome set at 120 per sec. *O* was allowed to observe the fluctuations of phase in the figure for a few minutes, to make sure that they occurred. *E* then gave her the following instructions:

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[6] Miles, Ocular dominance in human adults, *J. Gen. Psychol.*, 3, 1930, 428.

[7] Miles, *op. cit.*, *J. Exper. Psychol.*, 12, 1929, 125.

[1] M. F. Washburn and A. Gillette, Motor factors in voluntary control of cube perspective fluctuations and retinal rivalry fluctuations, this Journal, 45, 1933, 315-319.

> When I say 'Now,' you are to make every effort to keep the cube lying on the ground. When the up phase appears try as hard as you can to get rid of it. When the down phase appears say 'down;' when the up phase appears say 'up,' but try to hold the down phase.

In all cases, before beginning to make the record, *E* waited until *O* had observed both phases. The metronome was released as *E* gave the signal 'Now,' and she began making short pencil marks on paper in time with the beats; horizontal when *O* said 'down' and vertical when she said 'up.' The observation lasted 3 min., at the end of which *O* was allowed to rest as long as she felt it necessary. In the next observation the instruction was to hold the 'up' phase, and in the succeeding ones the order was: 'in' phase of book (this meant perceiving the middle vertical line as nearest to the *O*); 'out' phase of book; 'stair' phase; 'wall' phase of stair figure. During the second observation hour for a given *O* the order was: cube up, cube down, book out, book in, wall, stair.

The results were treated as follows. By counting the horizontal or vertical pencil marks the total duration, during the 3-min. interval, of the phase supposed to be suppressed was found, and was taken as representing the degree of failure of control for that phase, that figure, that *O*, and that experiment. The final figures were the means of those obtained in the two experiments.

Results. The easiest control was holding the book in the 'in' position, that is, with the middle vertical line nearest the *O*: the average number of half-seconds of failure within 3 min. was 106, S.D. 10.4. The stair was next easiest to hold, with an average failure of 114 half-seconds, S.D. 9.1. The difference between these averages is not reliable. Next harder to hold was the down phase of the cube: average half-seconds failure 144, S.D. 8.8. The difference between this average and that for 'book in,' divided by its S.D. gives 99.74 % of chances for a true difference; that between 'cube down' and 'stair' divided by its S.D. gives 99%. Fourth on the list is 'book out:' average half-seconds of failure 154, S.D. 13.28. The difference between this average and that for 'book in' gives 99.74% chances for a true difference; that between 'book out' and 'stair' gives 99.4%; that between 'book out' and 'cube down' is not reliable. 'Cube up' is fifth on the list: average half-seconds of failure 173, S.D. 11.9. The difference between this average and that for 'book in' is completely reliable, as is that between this average and that for 'stair;' that between this average and 'book out' gives only 86% of a true difference. Lowest in control is 'wall,' with an average failure of 194, S.D. 13.58 half-seconds. The differences between this average and that for 'book in,' for 'stair,' and for 'cube down' are all perfectly reliable: that between the 'wall' average and the 'book out' average gives 98% of a true difference, and that between the 'wall' average and the 'cube up' average gives 87% of a true difference. It will be noted that there is no correlation between the order of controllability and the time order of presenting the figures; if practice and fatigue were influential they must have neutralized each other.

A careful consideration of these results strongly suggests that the ease or difficulty of so-called voluntary control of the perceptual interpretations of these figures rests on two principles: (1) the simplicity or complexity of the response suggested; and (2) its frequency in ordinary life.

The book figure is most readily controllable, and it is the simplest figure. It is easier to hold that perception of the book which has been termed 'in,' that is, the one which perceives the center line of the back of the book as nearer and the two outer edges of the book as further away, than to hold the reverse perception. Obviously the former involves the simpler reaction: it is easier to imagine the book grasped by its back, requiring only one movement, whereas to grasp the outside edges suggest two movements, as does fixating the outside edges.

If the principle of simplicity ruled alone, we should expect that the stair-wall figure would be the hardest to control. But its two interpretations differ greatly in the matter of familiarity. The stair is next to the most controllable perception. The reactions it suggests are a matter of everyday life, whereas an overhanging wall is a very rare phenomenon, whose perception involves some discomfort. Another instance of the principle of familiarity is furnished by the cube, whose two interpretations are about equal in complexity, but decidedly unequal in familiarity. The down phase stands third from the top, the up phase fifth from the top in controllability. We are much more used to sitting down on cubes than to looking at them suspended above us!

LXVII. THE EFFECT OF AREA ON THE PLEASANTNESS AND UNPLEASANTNESS OF COLORS

By M. F. WASHBURN, K. G. MCLEAN, and A. DODGE

Professor J. G. Beebe-Center,[1] in his recent admirable book on pleasantness and unpleasantness, refers to a study on the subject named in the title above which was made in the Vassar Laboratory in 1911,[2] and suggests that in view of the lack of data on the statistical reliability of its results it might well be repeated. The fact is that twenty-two years ago many American psychologists were in a state of statistical innocence, and considered an average sufficiently chaperoned by an average deviation, and the difference between two averages thus accompanied a solid addition to knowledge. In accordance with Professor Beebe-Center's suggestion, the present paper describes a repetition of the 1911 study under better experimental conditions and with a proper treatment of results.

Method. The material was identical with that in the earlier investigation; namely, a set of Bradley colored paper squares 5 cm. a side and another set 25 cm. a side. Eighteen colors were used in each set: saturated red, orange, yellow, green, blue, and violet, and the lighter tint and darker shade of each of these colors. In the original study these colors were viewed by ordinary daylight, and at the beginning of the present study we tried to reproduce this condition, but finally transferred the work to a dark room, using a daylight 50-w bulb for illumination: Thus the background was dark, while in the earlier study it was a gray wall. There were 82 *O*s, each of whom performed the experiment twice, at an interval of more than a day. The colored squares were hung one at a time by an inconspicuous thread against the wall of the dark room, at a distance of 85 cm. from the *O*, who looked at the color for 10 sec. and reported her judgment of its pleasant or unpleasant quality by using one of the numbers from 1 to 7, 1 meaning very U, 2 moderately U, 3 slightly U, 4 indifferent, 5 slightly P, 6 moderately P, 7 very P. This was the method of report used in the 1911 study.

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[1] J. G. Beebe-Center, *The Psychology of Pleasantness and Unpleasantness*, 1932, 156-157.

[2] D. Clark, M. S. Goodell, and M. F. Washburn, The effect of area on the pleasantness of colors, this JOURNAL, 22, 1911, 578-579.

The order of presenting the colors was constant and was as follows, S and L in front of the color initial meaning 'small' and 'large' respectively: LGS, LY, SBT, LR, SV, SYS, LO, SVS, LR, LGT, SOS, LG, SBS, SYT, LB, LVT, SOT, SGS, LRT, SY, LBT, SR, LV, LYS, SO, LVS, SRS, SGT, LOS, SG, LBS, LYT, SB, SVT, LOT. This order was built on two principles; to avoid having the large and small areas of a color presented in immediate succession, since the method called for absolute rather than comparative judgments; and to have the large and small areas of a given color both preceded by the same color to keep successive contrast constant. For each *O* the average affective value, that is, the mean of those reported in the two observations for each of the two areas of each of the 18 colors was found, and the average of these averages, representing the group's judgment of the affective value for each color in each of the two areas, was recorded. The difference between the group affective value for the small area and that for the large area of each color was obtained, together with the standard errors of such differences as were not negligible in amount. This method was supplemented by counting for each color the number of *O*s who gave a higher affective value to the smaller area than to the larger area, and the number who gave the higher value to the larger area: this method ignored the *amount* of preference.

Only two reliable differences in the pleasantness of the two areas were found. They were for the Y shade, whose average affective value in the small area was 2.37 and in the large area 1.58, the differences of 0.79 being 5.5 times its standard error; and for the O shade, whose average affective value in the small area was 3.82 and in the large area 3.08, the difference of 0.74 being 3.4 times its standard error. Other amounts by which the average affective value of the small area exceeded that of the large area were as follows: sat. Y, 0.56, 2.53 times its standard error; R tint, 0.26, 1.86 times its σ; sat. G, 0.375, 1.71 times its σ; sat. O, 0.415, 1.70 times its σ; O tint, 0.34, 1.65 times its σ; sat. R, 0.49, 0.96 times its σ. The following colors showed differences in favor of the small area, all wholly unreliable: V shade, 0.19; B tint, 0.15; sat. B, 0.13; Y tint, 0.10; R shade, 0.02; V tint, 0.01. The following colors showed differences in favor of the larger area, all statistically negligible: G tint, 0.07; sat. V, 0.185; B shade, 0.19; G shade, 0.28.

Approximately the same order appears when the colors are arranged in order of the ratios of number of cases where small area was preferred to number of cases where large area was preferred, the rank difference *r* between the two arrays being +0.868 ± .04.

The most immediately conspicuous result is the decided preference for the small area of Y shade. Now this color was the most unpleasant to our *Os* of the whole series. The sum total of all the affective values for Y shade, small area and large area, together was 3245, the median of these sums for all the colors being 7620. It would seem natural for the most disagreeable color to be the one whose small area would be most preferred over the large area, on the principle that we can easily get too much of a bad thing. If this is the explanation, there should be a reliable negative correlation between the total sum of a color's affective values and the amount by which the sum of the affective values of its small area exceeds the sum of the affective values of its large area. But this *r* turns out to be only — 0.011 ± .166.

The *Es* ascribed the general dislike of these colors, Y shade and O shade, to their very disagreeable associations with mud and with fecal matter. The interesting point suggests itself: can the mere increase of area of a color, within the moderate limits used in this experiment, greatly increase its unpleasantness when the color is considered merely in itself? Must not any great decrease in its affective value with such a change result from association with something that is disagreeable to other senses or because of some taboo? In other words, is it possible for too much of a color to arouse as intense an unpleasantness as can be caused by too much of smells, contact sensations, and objects associated with tabooed situations?

A second statement is suggested, though not conclusively proved, by our results. The colors for which superiority of small over large areas is not wholly destitute of statistical support are: YS and OS (entirely reliable), Y (99.4% reliable), RT (between 96 and 97% reliable), G and O (96% reliable), OT (between 94 and 95% reliable), R (83% reliable). Spectral brightness seems to play a part here. The *saturated colors* on this list are Y, O and G, with R lowest: this is the daylight order of brightnesses. B and V show no superiority of small over large. The *tints* showing superiority are RT and OT: to an ordinary observer these tints in the Bradley colors seem to have more saturation than the other Bradley color tints, but of course there is no accurate information on this point. With the exception of YS and OS, whose marked superiority in the small area we have supposed due to unpleasant associations, there is no shade whose smaller area is pleasanter than its larger area.

In the 1911 paper there was reported a general tendency to prefer larger areas in shades and tints: the present study finds such an indication only in 4 out of 6 shades and 4 out of 6 tints. In the 1911 study all saturated colors except R were preferred in the smaller area; the present study finds indications of this tendency only in Y, O, G, and R; not in B and V. Since the number of *Os* in the present investigation was almost four times that in the earlier one, and since the results have this time been so much more adequately treated, the earlier work may be returned to oblivion.

In summary, the paper just presented finds a marked tendency to prefer small areas (5 sq. cm.) to larger (25 sq. cm.) areas of the darker yellow shade and orange shade of the Bradley colored papers; this preference is ascribed to unpleasant associations. It finds some indication, using the Bradley papers, that colors from the brighter spectral regions and of greater saturation are preferred in smaller areas.

APPARATUS

A THYRATRON CHRONAXIMETER: ITS OPERATION AND CALIBRATION

By GEORGE KREEZER, The Training School at Vineland, and JOHN D. COAKLEY and KARL M. DALLENBACH, Cornell University

Although an electric current of rectilinear form is the standard with reference to which the chronaxies of tissues are specified, much of the work in the field has not involved the use of this form of pulse. Many workers, while affirming the theoretical superiority of pulses of this form as stimuli in the measurement of tissue excitability, have failed to make use of them on account of certain unsatisfactory features of the instruments available for their production. The earlier instruments devised for producing rectilinear pulses were all mechanical in nature. The Helmholtz pendulum, the Keith-Lucas pendulum, the Weiss ballistic rheotome, and the Lapicque chronaximeter are among the better known of this type of instrument. The defects inherent in mechanical methods for controlling pulses of current have been pointed out by a number of workers,[1] and several attempts have been made in recent years to make use of electronic tubes for producing more satisfactory types of instrument for tissue stimulation. Circuits involving the use of vacuum tubes have been described by Fabre[2] and by Koch.[3] Only that of Fabre is adapted for producing single pulses.

Schmitt and Schmitt have described a circuit designated as a *universal precision stimulator* which makes use of the recently developed Thyratron mercury vapor tube.[4] The authors report that it is possible, by means of their apparatus, to vary independently the rate of stimulation, the intensity of the shock, its shape and its duration, and to produce pulses less than 0.1σ in duration. They make no specific reference to rectilinear pulses. Examination of their circuit indicates, however, that it would not be possible to produce rectilinear pulses by means of it since a transformer constitutes an essential part of their apparatus, being used to transmit the pulses from the plate circuit of the Thyratron tube to the output circuit of the stimulator.

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[1] Th. Weickert, Der Stromverlauf einiger physiologischer Reizapparate, aufgenommen mit dem Oszillographen, *Beitr. z. Physiol.*, 1, 1920, 227-254.

F. Scheminsky, Einige Bemerkungen über die Unterbrecher zur Herstellung Leducscher Mäanderströme, *Klin. Woch.*, 5, 1926, 839-841.

H. Lullies, Methoden der elektrischen Reizung von Muskeln und Nerven, *Handb. d. biol. Arbeitsmethoden,* Abt. V, Teil 5A, Heft 6, 1931, 1076-1113.

H. Koch, Eine Röhrenanordnung zur Erzeugung pulsierender Gleichströme variabler Frequenz, Intensität und variablen Unterbrechungsverhältnisse, *Arch. f. d. ges. Physiol.,* 1932, 169-174.

[2] Ph. Fabre, Production d'une onde rectangulaire pour la recherche de la chronaxie, *C. R. Acad. Science,* 187, 1928, 849-852.

[3] H. Koch, *loc. cit.*

[4] O. H. A. Schmitt and F. A. Schmitt, A universal precision stimulator, *Science,* 76, 1932, 328-330.

Thorne has recently described a circuit, involving the use of two Thyratron tubes designed to produce pulses of current of rectilinear form.[5] A circuit of this type has been set up in the Cornell laboratory, with certain modifications, and determinations made of its operating characteristics.

A diagram of the circuit is given in Fig. 1. The apparatus operates as follows.

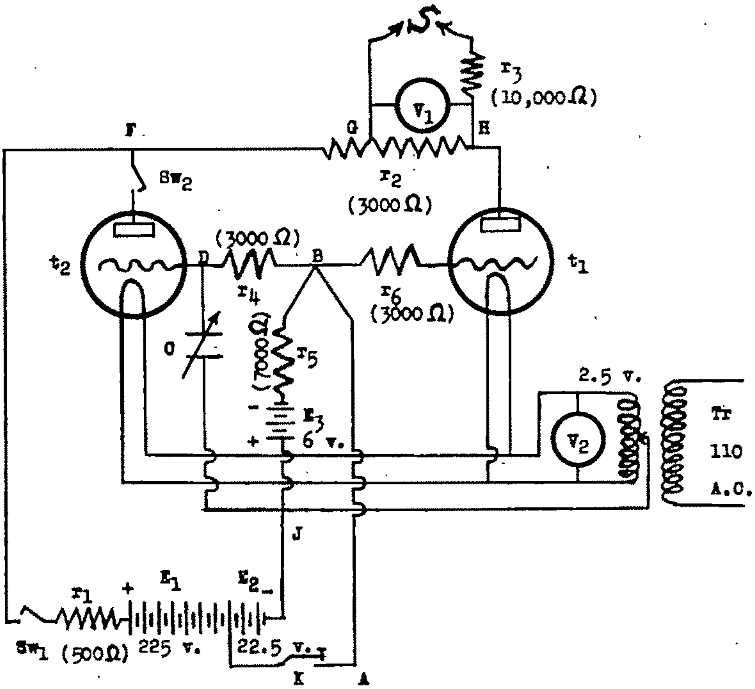

FIG. 1. CIRCUIT-DIAGRAM OF THE CORNELL CHRONAXIMETER

C = decade condenser (0.001 — 1.110 μF.); E_1, E_2, E_3 = power supplies; K = key; r_1, r_2, r_4, r_5, and r_6 = fixed resistances; r_3 = variable or decade resistance used as a potentiometer; t_1 and t_2 = Thyratron FG—17 tubes; Tr = transformer capable of providing 5.4 amperes for each tube; Sw_1 and Sw_2 = switches; V_1 = D.C. voltmeter; V_2 = A.C. filament voltmeter.

S is stimulated only during the time in which current flows through the potentiometer in series with t_1. At the start of any measurement, switches Sw_1 and Sw_2 are closed and the key K is open. No current flows through either tube on account of the negative grid bias maintained by the battery E_3. When the key K is closed, a current immediately starts to flow around circuit E_2ABJE_2, and is maintained until the key is released. At point B the current divides into two parts, one part returning to the battery E_2 by way of r_5, and the other serving to charge the decade condenser C. In order for a current to be initiated in the plate circuit of either tube, the grid must become more positive than a certain critical value (this critical value is about –3 volts for the FG-17 Thyratron when 200 volts are used on the plate). As indicated in the diagram, the grid of t_1 will take on the potential of point B and the grid of t_2 the potential of point D. After the key K is closed, point B will be more positive than point D, on account of the potential difference (p.d.) drop across r_4, until the condenser is completely charged. Point B will therefore exceed the critical grid value earlier than point D; t_1 will accordingly be set into operation before t_2. The time required for point D, connected to one plate of the condenser, to acquire a sufficiently positive potential relative to the filament to initiate a current in t_2 will depend on the p.d. effective in charging the condenser and on the product of the capacity of the condenser and the resistance of r_4.

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[5] F. C. Thorne, A thyratron-tube chronaximeter, this JOURNAL, 45, 1933, 515-517.

As soon as the plate current starts in t_2, it will lead the current in t_1 to stop, since there will no longer be sufficient potential difference between the plate and filament of t_1 to maintain the current. This is because the p.d. available between plate and filament of t_1 depends both on the p.d. existing between points F and J, and on the resistance of r_2. As soon as the current starts in t_2 (which is in parallel with the combination t_1 and r_2) the p.d. between F and J will be determined almost wholly by the resistance of the t_2 branch, since this resistance is very small compared to that of the t_1 branch. The p.d. drop will therefore be about 10 volts, the p.d. drop characteristic of the tube, and which is necessary to maintain the current through the tube after it has started. But in the t_1 branch of the circuit, this total p.d. of 10 volts will not be available between the plate and filament of the tube alone, since part of the p.d. drop must occur in the resistance r_2 as long as current flows. The remaining p.d., which would be available between the plate and filament of the tube, is not great enough to maintain the flow of current. The current through the t_1 branch therefore stops suddenly, as t_2 starts to conduct. There will thus be produced a pulse of current through the potentiometer which starts and stops abruptly, the suddenness of these changes being determined by the ionization times of the tubes t_1 and t_2, respectively. This ionization time is about 0.01 σ for the Thyratron FG-17. In order, now, to deionize t_2 and return the circuit to its original non-conducting state, either Sw_1 or Sw_2 must be opened.

For measuring the p.d. tapped off from the potentiometer and used for stimulating *S*, the procedure described above is repeated, but with Sw_2 left open and the stimulating electrode not in contact with *S*. The p.d. corresponding to any setting of the potentiometer may now be read by means of voltmeter v_1. The readings thus made with *S* excluded from the circuit will be a correct indication of the p.d. between points G and H during stimulation only if the resistance of the *S*-branch ($S + r_2$) is large relative to the resistance between points G and H of the potentiometer. If, however, the resistance of the *S*-branch is not considerably larger than the resistance between G and H, the p.d. drop between G and H will be smaller when *S* is included than when he is not, and the voltmeter readings will be in error. In such a case, the voltmeter-potentiometer combination must be calibrated so that the p.d. with *S* in the circuit which correspond to readings made when *S* is not in the circuit will be known.

It will be recalled that the essential agent effective in stimulating a tissue is the density of the current through it. Measurement of the p.d. at the terminals of the branch of the circuit including *S* is, however, conventionally used as an index of the strength of the current through the tissue since, as expressed in Ohm's law, the current at different times will be proportional to the p.d. if the resistance of the part of the circuit involved is constant. It is necessary, therefore, that safeguards be used to keep the resistance of the *S*-branch substantially constant. For this reason a resistance of 10,000 ohms is placed in series with *S* so that small fluctuations of resistance of *S* will make relatively little change in the strength of the current corresponding to a given p.d. between G and H.

It should be noted that in a circuit of the present type fluctuations of resistance of the S-branch have absolutely no influence on the *duration* of the electric pulses. In the present circuit, duration is dependent wholly on the events occurring in the grid circuits of the Thyratron tubes. In the condenser-discharge type of stimulator, on the contrary, the duration of the pulses is dependent upon the resistance of *S*, and although a special network of resistances is conventionally used to minimize the effect of changes of *S*'s resistance on the duration of the pulses, it is obviously an advantage for the duration of the pulses to be completely independent of variations in *S*, as is true in the case of the present circuit.

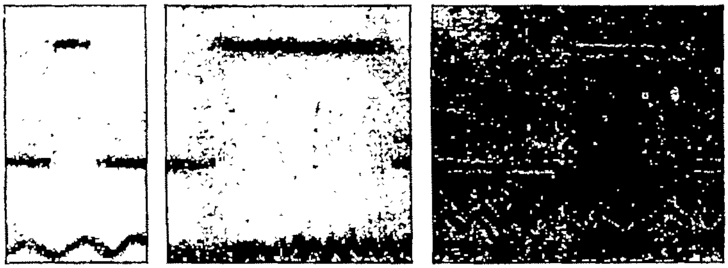
The electrical dimensions of the various parts of the circuit in use in the Cornell Laboratory are indicated in the diagram. The purpose of the various resistances used is as follows: r_1 is used to prevent the plate current of t_2 from becoming greater than 0.5 amperes which is the maximum average current safe for the tube to conduct; r_2 is used as a potentiometer to control the p.d. used for stimulating *S*. The greater the resistance of r_2 relative to r_1, the greater is the proportion of the voltage of battery E_1 which will be available at the terminals of the potentiometer. If, however, the resistance of the potentiometer is made so high that it approximates the resistance of the *S*-branch (about 20,000 ohms), then the voltmeter readings will be in error and, as pointed out above, a special calibration of the potentiometer-voltmeter combination will be needed; r_3 is used in series with *S* in order that the total resistance of the *S*-branch will be so large that possible fluctuations of resistance in *S* may be considered negligible, as pointed out above; r_4 and r_6 are used to limit the average grid current to less than 0.05 amperes, the maximum tolerated by the tube—this need might also be taken care of by a single resistance located in the A-B limb of the circuit; r_4 has the additional function of helping to determine the time interval between the starting of tubes t_1 and t_2; r_5 is used so that practically the entire p.d. available from E_2 will exist between the points B and J. All the resistances, especially those in the plate circuit, should be non-inductive, since the presence of self-induction would reduce the rectilinearity of the current pulses.

Concerning the selection of an appropriate source of power for the plate circuit, a few words may be added. In the diagram, the power supply for the plate circuit is represented as provided by batteries. With 500 ohms used for r_1, and 225 volts for E_1, close to 0.5 amperes of current must be supplied. For current drains as large as this, dry batteries are not appropriate since such discharge rates would tend to quickly discharge a dry battery and render its operation unstable. Therefore, with 500 ohms used for r_1, either storage cells, a motor generator of D.C., or a rectified and filtered A.C. supply should be used. In the present calibration, a motor generator of D.C. was used as the source of power.

Dry cells might be used if the current drain were reduced by the use of a greater resistance for r_1, *e.g.* about 5000 ohms. Then only about 50 milliamperes would be drawn during the time t_2 was conducting. It would be necessary, too, to use a proportionately higher resistance for r_3, so that most of the p.d. provided by the power source would be available at the terminals of the potentiometer for stimulating *S*. In this case, as mentioned above, it would be necessary for the potentiometer-voltmeter combination to be calibrated so that the voltmeter readings would indicate the actual potential difference effective between the potentiometer leads when current flowed through the *S*-branch.

The apparatus described above has been calibrated by means of the cathode ray oscillograph to show the form of the pulses produced,[6] and by means of a ballistic galvanometer to determine the duration of the pulse corresponding to various settings of the condenser C for a given value of r_4. The results of this calibration are given below.

(1) Form of pulse. The photographs reproduced in Fig. 2 indicate the form of single pulses produced by the present apparatus. The duration of the pulse, progressing from right to left, is indicated by the sine wave near the bottom of the photograph. The rising and falling phases of the pulse, because of their abruptness, do not reproduce in these photographs, although they appear on the oscillographic screen as fine, straight lines without any detectable irregularities. The top of the wave, regardless of its duration, is likewise a straight line without irregularities. If r_1 and r_2 are inductive resistances, two irregularities appear in the wave: the first is a small, narrow spike at the end of the rise; the second is a wide, inverted spike appearing just as the curve reaches its initial level. In the curves shown in Fig. 2, no such spikes are apparent, indicating the lack of self-inductance in the circuit.



1000~ 1000~ 500~

FIG. 2. CATHODE RAY OSCILLOGRAMS SHOWING THE FORM OF THE PULSES PRODUCED

(2) Range of duration of pulses. The duration of the current pulses produced by the apparatus is controlled by the setting of the condenser C for a given resis-

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[6] We wish to express our thanks to Mr. T. T. Goldsmith of the Department of Physics for placing his cathode ray oscillograph at our disposal and assisting in recording the form of the pulses produced by the present apparatus.

tance (*e.g.* 3000 ohms) of r_4. The duration in seconds corresponding to particular condenser settings was determined by means of measurements made with a ballistic galvanometer substituted in place of the subject. For a current pulse of rectilinear form, the duration of the pulse may be determined by the use of the formula:

$$T = KdR/E \quad \text{[1]}$$

in which T is the time of discharge in sec., d the deflection in centimeters on the galvanometer-scale produced with a given setting of the condenser, R the resistance of the circuit branch including the galvanometer, E the p.d. applied at the terminals of this branch of the circuit, and K is a constant for the galvanometer equal to the quantity of electricity in coulombs necessary to produce a deflection of 1 cm.

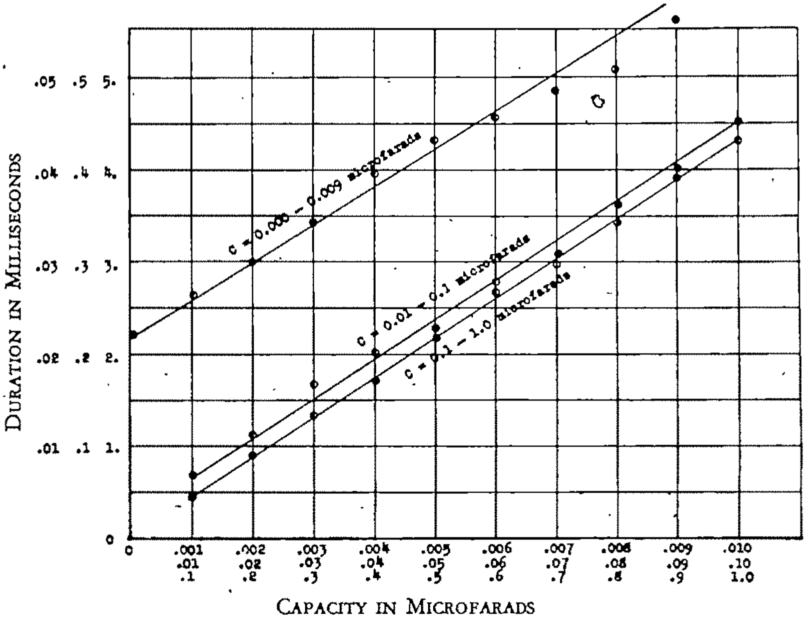

FIG. 3. SHOWING THE DURATION OF THE CURRENT PULSES CORRESPONDING TO VARIOUS SETTINGS OF THE CONDENSER

The duration of the pulses corresponding to various settings of the condenser is shown in the accompanying table and in Fig. 3. The relationship is linear and may be represented by the equation:

$$t = 4.2722\,C + 0.0218 \quad \text{[2]}$$

in which t is the time in milliseconds; C is the capacity of the condenser in microfarads that was used; 4.2722 the slope of the line; and 0.0218 the duration of the pulse produced with a zero setting of the condenser, which is due to the residual capacity present between grid and filament of t_1 or their connections.

The table gives, in addition to the observed durations at various capacities of the condenser C as computed from the ballistic galvanometer readings by Formula [1], the durations as calculated by the generalized Formula [2]. It will be seen from a comparison of the observed (O.T.) and calculated (C.T.) times that the values are very close; they seldom differ by more than one point in the second decimal place and frequently differ by amounts less than 0.00001 sec. The accuracy of the apparatus and of its calibration is thus indicated.

TABLE I

DURATION IN MILLISECONDS OF THE CURRENT PULSES AT VARIOUS CAPACITIES OF THE CONDENSER

C=capacity in μF; O.T.=observed duration as measured by a ballistic galvanometer; C.T.= duration computed by formula [2].

C	O.T.	C.T.	C	O.T.	C.T.	C	O.T.	C.T.
1.000	4.3038	4.2940	.090	.4001	.4063	.009	.0558	.0602
.900	3.8897	3.8668	.080	.3609	.3636	.008	.0507	.0560
.800	3.4190	3.4395	.070	.2984	.3209	.007	.0482	.0517
.700	2.9623	3.0123	.060	.2780	.2781	.006	.0426	.0474
.600	2.6019	2.5851	.050	.2266	.2354	.005	.0421	.0432
.500	2.1572	2.1579	.040	.2036	.1927	.004	.0396	.0389
.400	1.6977	1.7307	.030	.1663	.1505	.003	—	.0346
.300	1.3051	1.3035	.020	.1222	.1072	.002	—	.0303
.200	.8940	.8762	.010	.0695	.0645	.001	—	.0261
.100	.4534	.4490				.000	—	.0218

With the constants specified above for the circuit it is possible to obtain a range of temporal values extending approximately from 0.0218 to 4.3000σ. This range can readily be extended by substituting greater resistances for r_4, since the interval between the operation of tubes t_1 and t_2 is proportional to the product of the resistance of r_4 and the capacity C. The present apparatus thus makes available electrical pulses of rectilinear form and a range of durations whose extent can be readily controlled by simple adjustments of the circuit.

A ROCHELLE-SALT-CRYSTAL MOTOR USED AS AN OSCILLOGRAPH

By HARRISON MUSGRAVE and MILTON METFESSEL, University of Southern California

Several types of phoneloscopes and oscillographs are available, the former energized directly by sound waves impinging on a diaphragm, and the latter usually actuated by the output of an amplifier. Both instruments are expensive and fragile. The motor here described reduces both of these difficulties, and it gives an adequate response from 0 to 3500 d.v. a sec.

Rochelle-salt-crystal loud-speakers may be purchased complete and the speaker-cone removed or the driving motor may be procured by itself.[1] Such a motor is shown in Fig. 1 with the phoneloscope attachment in place. A small block of wood or metal is mounted securely to the face of the motor about 1/4 in. from the end of the driving lever. One leg of an inverted U (Fig. 1, A) is fastened to the block, and the other end to the upright finger at the extremity of the driving arm (B). We have used pieces of hair-spring from an alarm clock for this purpose. The end fastened to the block may be forced through the middle of a flat toothpick, bent back around it, and the toothpick then glued to the block. The other end is fastened to the lever with Duco Household cement. (This is a handy substance to have in the laboratory. It sets quickly, is heat- and moisture-proof, will stick to almost anything save rubber, and can be removed if desired with lacquer thinner.) A rectangular mirror is mounted to the U with gummy shellac. This will hold it securely in place, and yet allow it to be moved around to the point of best adjustment. This point may be determined by trial, pressing the lever lightly with the finger to tilt the mirror. The smaller the U, the greater will be the arc through which the mirror-chip oscillates. The mirror is cut from a silvered microscope cover-glass. There is so much power available from these motors that the size of the mirror may be relatively large.

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[1] The Brush Development Co., 3715 Euclid Ave., Cleveland Ohio; or C. H. Stoelting Co., 424 N. Homan Ave., Chicago.

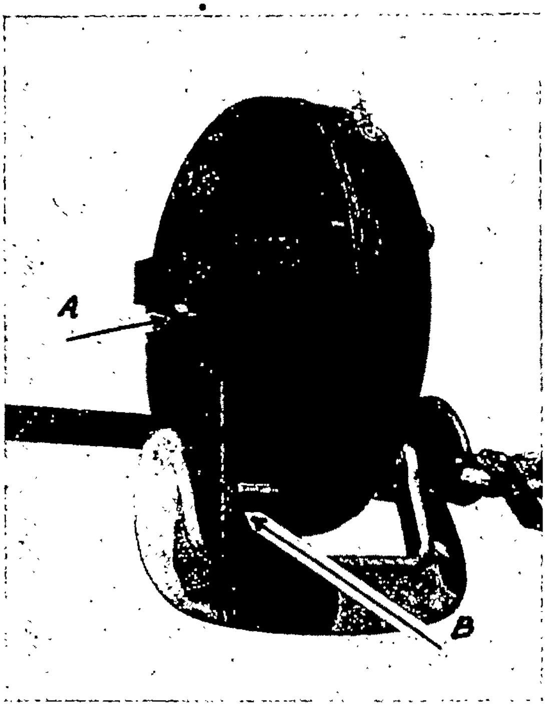

FIG. 1. ROCHELLE-SALT-CRYSTAL MOTOR

If a light beam is direted on the mirror chip while a sound is impressed on the microphone, the wave may be photographed on a moving film, or may be observed directly on an apparatus such as the phonoprojectoscope.[2] As the amplifying unit has been described,[3] further reference will not be made to it.

To test the crystal or the mounting for accurate response, the crystal may be connected directly to the commercial A.C. circuit. A pure sine wave should result.

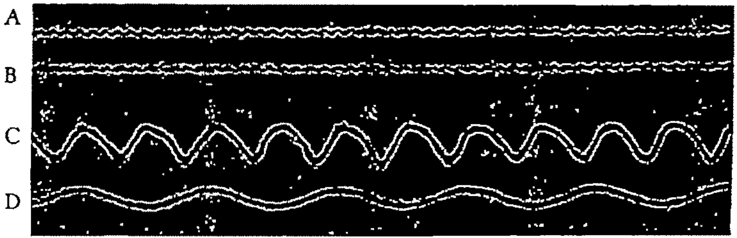

FIG. 2. WAVE-FORMS RECORDED
A = 2800 d.v.; B = 2700 d.v.; C = difference tone of 100 d.v.; D = 50 d.v.

Fig. 2 shows wave forms recorded with this oscillograph. The top line (A) has a frequency of 2800 d.v., and the second line (B) 2700 d.v. These sounds were made from two of Young's difference-tone bars.[4] For the third line (C) the two tones were sounded together. The difference tone, with a frequency of 100 d.v. is very pronounced. The last line (D) is a time line of 50 d.v. made by stepping the 110 volt A.C. lighting current down to 18 volts and applying it directly to the oscillograph. This figure illustrates the performance of the device, and indicates one purpose for which it might be used.

This oscillograph may also be used to get an accurate measure of duration, or the extent of excursion of a movement. As noted before, the crystal bends proportionately to the amount of voltage impressed across its two faces. It will maintain the same displacement as long as the voltage is constant. Since it acts as a condenser, it draws no power from a source of direct current, and the amount of current flowing in any other part of the circuit will not effect its displacement unless the voltage is varied thereby. Thus a simple circuit will enable one to control the amount of the crystal displacement. All that is needed is a source of from 45 to 250 volts D.C. (which may be easily procured from B batteries), a potentiometer, and the oscillograph. Fig. 3 indicates all the necessary wiring. The potentiometer may be one of about 20,000 ohms similar to those used in radios. At 200 volts, the current drain from the B batteries with a potentiometer of this ohmage will be only 10 milliamperes, which means almost shelf life for the batteries. A potentiometer of higher resistance may be used equally well if desired.

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[2] M. Metfessel and J. Tiffin, A new phonoprojectoscope, this JOURNAL, 41, 1929, 122.

[3] Cf. H. Musgrave and M. Metfessel, Mirror tonoscopes, this JOURNAL, 46, 1934, 478-480.

[4] P. T. Young, Series of difference tones obtained from tunable bars, this JOURNAL, 33, 1922, 385-393.

Varying the set of the potentiometer will vary the displacement of the crystal, which in turn will deflect the light beam to a new position on the recording film. The light line on the film will stay at the same level until the potentiometer is again varied. The potentiometer may be varied mechanically by such means as a lever attached to its shaft, a gear and ratchet, or a bow and arrow arrangement with the string of the bow wrapped once around the shaft.

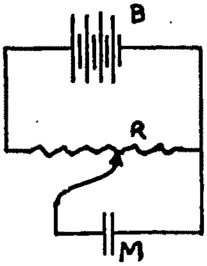

FIG. 3. WIRING DIAGRAM
B = Direct current source, 50–200 volts as required;
R = 20,000 ohm potentiometer; M = Crystal motor oscillograph

The film, of course, must be moving across the light beam and perpendicular to its direction of deflection. One advantage of this recording system is that, if the experiments are to be conducted in the light, the recording apparatus may be at any convenient distance away in a dark room.

If a time-line is desired, a second oscillograph may be operated at the same time, its power coming from the house-lighting circuit. It will probably be found advisable in this case to step down the voltage with a transformer so the time line will not have too great an amplitude.

NOTES AND DISCUSSIONS

SOCIOLOGY AND GESTALT-PSYCHOLOGY

Like Psychology, Sociology as a science wants definition. This is clearly revealed in a recent book which, since it aims to be a critical study of the subject, is employed as a basis for the present discussion.[1]

As regards the contribution of psychology to sociology, this book is more pronounced in its opposition to behaviorism, than in its constructive employment of any other psychological theory. The refusal of behaviorism is the usual one; that a reduction of human conduct to reflexive acts, conditioned by more and more elaborate complexes of stimulation, fails to explain the distinctively human phenomena of culture.

"Since," writes Professor Jensen in his Introduction, "the sciences of culture differ from the natural sciences in their subject matter and problems, they require a different methodology for their development. First, cultural phenomena cannot be analyzed and described without reference to meaning, which implies conscious states; second, they cannot be explained without reference to purpose, which involves a consideration of ends; third, they cannot be predicted, except in terms of crude probabilities, without reference to values; and fourth, they cannot be controlled without reference to motives, both of which [control and motive?] require insight into impulses, wishes, attitudes and desires" (p. xvii).

This somewhat dogmatic statement is elaborated by Professor Ellwood in his critical study, but nowhere do we find a clear definition of the cultural method he advocates. He admits that the processes with which the social sciences deal are "essentially psychological," but insists that they "are only to a small extent amenable to the physical science methods of exact observation, experiment, and measurement" (p. 51). "*The way out which is proposed is that of a broader method,* supplemented by critical scholarship" (p. 5). This "way out" seems to suggest that "the contributions of philosophy, of objective observation, of imagination, of historical interpretation, of psychological analysis, of statistical measurements, and of social experiments, to sound social theorizing must all be evaluated, together with the bearing of sociology upon social practice and of social practice upon sociology" (p. 10). Like philosophy, the social sciences are "necessarily reasoned sciences" (p. 19), and "if there be non-physical, teleological, or indeterminate elements involved in social behavior, scientific method should be broad enough to recognize the fact" (p. 52).

"Culture," it is asserted, "even in the strictest anthropological sense, is at bottom an appreciation of values, and it has many non-material aspects which are more fundamental than its material aspects" (p. 54). Thus, "if science is to come back . . . to the basis of common sense, we must give introspection, or self-examination, a place" (p. 59). "I shall endeavor to show," writes Ellwood, "that the scientific methods found useful in the natural sciences cannot be adapted with any high degree of success to the social sciences, and that the social sciences must therefore develop distinct methods of their own" (p. 67).

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[1] C. A. Ellwood, *Methods in Sociology: A Critical Study.* 1933, 1-214.

These methods appear to be those of "imagination," "understanding," "scientifically written history," "ethics"—for which, however, sociology "must furnish the immediate positive foundation" (p. 135)—and education. "We have every right to conclude . . . that the continuity of human culture from the start was maintained largely by educative processes more or less deliberately undertaken as a means of preserving knowledge and habits which human groups found to be of value" (p. 199).

The "commonplaces of educational and social science show that the general theory of Ward that education is the proximate means of social progress is sound, and that the view of those eighteenth-century social philosophers who held that education is the method of social advance is much more nearly correct than some views which have had vogue during the last few decades" (p. 214).

A penetrating insight into this view has recently been given in a small book by Carl Becker, entitled "The Heavenly City of the Eighteenth Century Philosophers." I may be permitted to quote one paragraph:

> If we would discover the little backstairs door that for any age serves as the secret entranceway to knowledge, we will do well to look for certain unobtrusive words with uncertain meanings that are permitted to slip off the tongue or the pen without fear and without research; words which, having from constant repetition lost their metaphorical significance, are unconsciously mistaken for objective realities. In the thirteenth century the key words would no doubt be God, sin, grace, salvation, heaven, and the like; in the nineteenth century, matter, fact, matter-of-fact, evolution, progress; in the twentieth century, relativity, process, adjustment, function, complex. In the eighteenth century the words without which no enlightened person could reach a restful conclusion were nature, natural law, first cause, reason, sentiment, humanity, perfectibility (these last three being necessary only for the more tender-minded, perhaps) (p. 47).

Professor Ellwood appears in his book as a "more tender-minded" philosopher of the eighteenth century. His heavenly city, like that of the period to which he refers, is in the future, and is to be attained by a combination of "sentiment" and "reason."

Despite the somewhat cynical scepticism of Professor Becker's critique, something can doubtless be said for the cause of "humanity" and "perfectibility" as revealed by "sentiment" and "reason;" but no science of sociology will ever be possible until these, and equally vague words such as "culture," "imagination," and "understanding," have been defined in terms of their functional significance.

As a social scientist Professor Ellwood reserves "the right to judge the quality of the theory of human nature delivered to him by the psychologist, as to whether or not it is adequate in interpreting the facts with which he deals" (p. 78). Since he will have nothing of behaviorism, and appears to make nothing of introspective existentialism, we have as a third possible means of deliverance, *Gestalt*-psychology, to which he makes two commendatory references.

The first is that "the gestaltists in psychology have taught us to regard personalities as functioning wholes. The soundest tradition in sociology has long stood for the same view in the functioning of groups" (p. 21). Unfortunately, this statement is too vague to be serviceable. One might suppose that the "whole man" is a self-sufficient unit in every group, and that group-ways are analyzable into the individual ways of their constituents. But *Gestalt*-psychology finds its wholes both in the performances of personalities and in those group-wholes wherein the personality of the individual members gives way to communal action. There is no suggestion in Ellwood's book of the reality of communal action, or of an appropriate sociological method for dealing with it.

Let us see what can be done with "sentiment" and "reason" as criteria of "humanity" and "perfectibility." Sentiment can be employed as a criterion of all participative, empathic behavior, whether it be an aesthetic experience of beauty in a work of art, based upon imitative gesture, or the involvement of group-behavior in which the individual shares by contributing, not a discrete action, but a partial and itself incomplete portion of the total pattern. Thus understood, the behavior of a group has a pattern which "must . . . exercise constraint upon its individual members," *not* "because in complex societies there is greater opportunity for individual variation between the habits of different individuals" (p. 155); but because, irrespective of complexity, behavior is always in some degree participative. Whether a person individualizes himself, and stands aloof from his fellows, or sacrifices his individuality by participating in the common rhythm of a group to which he "belongs," is a matter of emphasis. The two fundamental modes of behavior are approach and retreat, and approach involves the incorporation of the desired object or goal, whether it be thing, person, or group, into the creature's own pattern of action. One does not respond *to* stimulation but *with* it. In retreat, one escapes from an undesirable thing, person, or group. The moment of escape is non-participative and unsocial, and until a new and attractive goal is manifest, is disordered and dismaying.

Social behavior is always participative, but may emphasize the participating individuals, with a corresponding dominance of leaders over led in a hierarchy of competitors, or may emphasize the group-rhythm in which, properly speaking, there is no leadership, but only a communal act. Thus the statement that "nothing great is accomplished in human society without leadership" (p. 211) is not in accordance with the teachings of *Gestalt*-psychology. On the contrary, *Gestalt*-theory indicates that the "superior society of the future", which the eighteenth-century philosophers held up to view, will only be attained if and when reason gives way to sentiment, and the aesthetic criterion of social participation is employed in the cultivation of an art of living which displaces our now prevalent, over-rationalized, competitive society.

Sociologists seem always baffled by the central problem of their subject, which is that of the social group. Since they can find no help in solving this problem either from behaviorism or from introspective psychology, I commend to them the study of the *Gestalt*-theory. Something more than lip service, however, is required of them; for without understanding the radical departure of this theory from the premises which we have all been wont to employ in our scientific investigations, they cannot devise the definite methods of procedure which are badly needed.

Professor Ellwood's second mention of *Gestalt* is in support of his belief that culture is a product of learning. "The gestaltists in psychology have shown," he writes, "that *insight is the essential element involved in learning*" (p. 62).

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[2] R. M. Ogden, Insight, this JOURNAL, 44, 1932, 350 f.

Presumably learning does have a central place in sociological organization, and Professor Ellwood concludes his volume with chapters on "The Sociological Basis of the Science of Education," and "The Educational Theory of Social Progress." But without qualification the use of the term "insight" as the "essential element of learning" is just another of those "unobtrusive words with uncertain meanings that are permitted to slip off the tongue or the pen without fear and without research."

Gestalt-psychology employs the term "insight" in a very special way to describe the self-regulation of an organism in a field of attractive and repellent forces. If learning involves insight, that is because no special form of behavior can be learned unless the organism is at the time regulating its own procedure. The statement does not imply anything like a rationalized plan of procedure in advance of action, though it does indicate a certain emphasis upon the means of attaining an end in view.[3] It is only because the means are thus emphasized that they become acquired as ways of action, to be subsequently employed in different situations from the one in which they were originally called forth. Unless these self-regulated means to an end are accentuated, they vanish with the original action. Insight involves a certain detachment of the subject from his action, and a certain differentiation of his means from his ends. Thus and thus only can one see into what one is about. Thus and thus only can one begin the process of abstraction which accounts for all the so-called higher mental processes of reasoning. If we would found a psychology of behavior on "sentiment" and "reason," these terms can receive a precise definition as the two fundamental ways in which living organisms become aware of their actions. The first is the way behavior is felt, and the second is the way behavior is known. The first is an awareness of the behavior-pattern without discernment of its parts; the second, which is genetically subsequent to and always accompanied by the first, is the discernment of articulate behavior. Articulation makes possible the selection or choice of partial patterns within a whole, and the abstraction of their quantitative and qualitative features furnishes the terms of reasoning. The participation of an individual in group-behavior may be either sentimental, or reasoned, or both. If it is sentimental, the method is that of feeling ones way into the rhythm of the group-whole which is accomplished only by a surrender of self-interest; if it be reasoned, one retains self-interest and operates with discernment.

Let us review our quotation of Professor Jensen's introductory remarks. The implication of "conscious states" in cultural phenomena can now be defined as feeling and knowing what one is doing. The "meanings" of these phenomena reside in the functional performance of the individual in his society. The "purpose" which appears in the attainment of ends is revealed as a true entelechy, or inward striving for adjustment, which characterizes any organized effort such as behavior reveals itself to be. There are two purposes in organic striving: approach and incorporation of a desirable goal or end, and retreat or escape from an undesirable goal or end. But there is no necessity of recourse to an external agency which considers "ends" before they are employed. The "consideration" is part and parcel of the end-seeking behavior itself, and its revelation in abstract thinking is only a surrogate performance, a postural behavior which does not immediately issue in locomotion.

Reference to "value" is likewise implicit in the behavior itself. If we can assume that the organism strives for adjustment, and is organized for that purpose, it will possess favored, valuable, methods of action before which unfavored and less valuable methods must give way. If the principles of ethical, and perhaps of aesthetic, values rest on sociology, sociology rests upon group-behaviors which establish norms by virtue of an adaptability inherent in the situation-response capacities of the organism and its group-ways of action. As for "motive," this too is inherent in the organism, and likewise in the organized group. Motivation is *need*: its control is environmental. Insight comes whenever articulation is sufficient to define the partial patterns of self, means, and end. But in the group there is no group-self to be differentiated from its means to ends. Otherwise the motives of a group are as actual, and the articulate means of attaining ends are as real, as those of an individual.

What then has *Gestalt*-theory to say of human culture and the perfectibility of social action? Only that organic behavior strives to satisfy its wants by means which are conducive to its well-being as an organized whole. The disruptive, disorganized, flight is at the mercy of chance, but the straightforward pursuit of ends is in accordance with organic behavior, and is subject to valuable norms of procedure which are "natural" to individual and group-ways for the simple reason that they are the ways in which an organism or group of organisms must behave. It is not apparent that we stand in need of any special agencies to lift man above the brute. The distinctive features of humanity may be traced genetically to the lowliest of organisms, and an extension of the same methods which enable us to study the developmental behavior of the human embryo can be usefully applied to its post-natal individual and social behavior.

Although these few hints of the service the *Gestalt*-theory can render in establishing scientific methods of sociology leave much to be desired which I am incompetent to give, I cannot conclude without references to the topological method now being developed in the researches of Kurt Lewin. By following his principles, it seems likely that we are on the track of a scientific method of social research that will even be "amenable to the physical science methods of exact observation, experiment and measurement" which Professor Ellwood thinks "cannot be adapted with any high degree of success to the social sciences." It may be added that these methods are not the property of the physical sciences, but belong to any one who can use them without forgetting the particular use to which he puts them.

Cornell University R. M. OGDEN

A CASE OF HEMERALOPIA

Of much interest, especially in establishing the duplicity theory of vision, are cases of so-called hemeralopia or night blindness due to lack of rods, or lack of function of the rods. At least three forms of the disease may be noted. In all types the condition is inherited. One is static and since it seems to be connected with no other type of abnormality is known as the idiopathic form. The rods simply fail to develop. In two other forms, the lack of rods is associated with injury to other structures and the disease may be stationary or progressive. One, *choroideremia*, is connected with congenital absence of the choroid, and is usually progressive. The other, *retinitis pigmentosa*, accompanies a diseased condition of the pigment cells and usually shows steady, if slow, deterioration in vision. All have approximately the same disturbances. The main difference is in the distribution of the different forms of vision upon the retina.

The case presented is of the type of retinitis pigmentosa. A male student came up at the end of a lecture in which the narrowing of the visual field in hysteria had been mentioned and said that his field of vision was similarly contracted—that he could see very little except in the center of the field. A few questions showed that he not only had a narrow field but had very defective sight with low illumination. He said that as a boy in Texas as far back as he could remember he could see very few stars. Everything pointed to hemeralopia. He agreed to come to the laboratory for tests.

The apparatus available made only comparatively rough tests possible. They were sufficient to establish four of the points that would be expected to distinguish pure cone vision from the normal compound of rod and cone vision. The first would be the total lack of brightness vision outside of the region where hues can be distinguished, the second would be lack of brightness vision at intensities below the limen for hue. Thirdly, adaptation to fainter lights with the passage of time would be lacking or greatly reduced. Fourthly, there should be no Purkinje phenomenon. Each of these was tested with the results that were to be expected.

The ordinary student perimeter showed that color vision was limited to about 5° from the fovea. No consistent difference existed in the extent of vision for different colors, nor for the different diameters. On the temporal axis blue could be seen on the average to 5½°, red to 6¼°. On the nasal axis red and blue both extended to 4¾°. Down, blue was seen to 5½° and red to 4⅞°. Up, blue extended to 5½°, red to 5⅛°. Yellow and green both gave measurements that departed very little from 5°. The results were the averages of in and out measurements, and with the exception of a few readings were strikingly consistent. The results given above were for the right eye. Fewer measurements were made on the left eye, but they also showed very little deviation from 5°. Individual results varied from 4° to 6°, for both red and blue.

Occasionally the patient would report that he saw something at 10°. In practically every case these would be mistakes. At times a color would be said to be gray or white before it was seen as color. Much more frequently it would be reported to be a wrong color. Red was reported to be green once, purple reported as blue. The degree of certainty would markedly increase as the 5° range was reached. Some of these mistakes may have been due to uncontrolled eye movements, others to mere guesses. The change in attitude when 5° was reached made it altogether probable that there was no brightness vision outside the color range. The range was very little if at all extended when the area of the colors used for testing was increased from 16 to 6500 sq. mm. There seemed to be almost none of the summation effect from parts of the larger areas that is present in the normal eye as the limit of a color zone is reached. The results indicate that not merely the rods, but the cones over much of the area they normally cover, had been destroyed by the disease that originated in the pigment cells.

The measurements of the limen for hues or the photochromatic interval was made on an instrument adapted by Dr. Shepard for the use of the elementary laboratory students. Essentially it was a dark box at one end of which was a chamber for an electric lamp. In the chamber a slit of variable width was inserted. Light from the slit was projected upon a white cardboard screen inclined at 45°, 170 cm. distance. From this the light was diffused upon colored filters in the side of the dark box. The intensity of the light could be varied by changing the width of the slit. The width of slit gave a rough measure of the intensity of the light reflected on the colored filters. The values are relative only and have significance for us only when compared with readings of normal individuals on the same instrument.

We confined our readings to green. Although red was shown with it, green was brighter and alone was noted. On the average with the dark adapted eye, a faint light was seen at 6 mm., a definite blue appeared at 8 mm. of slit width. Six normal students in the elementary laboratory course gave values of 5 mm. for color and 0.5 mm. for brightness. The difference for the hemeralope between the faintest light that can be seen and definite color is quite as likely a range of uncertainty of judgment as it is of a true photochromatic interval. He remarked several times that in this range the color seemed to fluctuate. It would be seen at times and then would disappear. At the most the photochromatic interval is very much less than for the normal individual.

A very crude test was made of the presence of adaptation in the hemeralopic eye. In the normal eye most of the adaptation is assigned to the rods. It seemed interesting to know if there was any adaptation in pure cone vision. The test consisted in asking the patient to look directly at a daylight color bulb for 1 min. and then making tests for visibility of minimum color intensities. After 1 min. the limen for brightness was 18 mm., color was seen clearly and constantly at 20 mm. After prolonged looking at the bulb of the light the limen rose to 32 mm. for brightness and 34 mm. for green. When the patient looked for 1 min. at the field illuminated by the lamp and not at the bulb itself, brightness was seen at 15 mm., color at 20 mm. The experiments are so rough that they showed only that there is adaptation in the cones, that it varies with the brightness of the light to which the eye is exposed and with the duration of the exposure.

Later these results were compared with Dieter's measurements of three hemeralopes of the innate, idiopathic type, on the Piper adaptometer.[1] Dieter states that his cases showed approximately the same course of adaptation as the upper part of the curve of the normal eye, that is for the first 5 min. At that point there is a sharp downward turn in the curve for the normal eye, that makes a marked break in the curve, while the hemeralope shows only a very gradual increase in adaptation for the rest of the hour the tests lasted. From an initial 100,000 of his micro lux units, the sensitivity of the normal eye increases to appreciate 25,000 ml. at 5 min., while the hemeralope stands at about 20,000 ml. At the end of the hour the three hemeralopes range from 4500 to 6900, while the normal eye has increased to 9 micro lux. From this measurement by Dieter it appears that in the normal eye the adaptation of the first 5 min. is due to the cones, most of the later increase is to be ascribed to the rods. Adaptation of the same form has been shown by Kohlrausch in the rod free part of the fovea.

Incidentally, it may be of value to record that after looking at the bright light a flight of colored after-images was seen that lasted for a number of minutes. The first of these after-images was a bright green, followed by red and then by purple. The red was surrounded by a green haze. The images alternated rapidly. Occasionally black spots would appear in the colors. Possibility of the flight of colors in an eye that has only cones may at some time prove of theoretical importance.

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[1] Walter Dieter, Untersuchungen zur Duplicitätstheorie. III. Die angeborene, familiär-erbliche, stationäre (idiopathische) Hemeralopie, *Arch. f. d. ges. Physiol.* 222, 1929, 381-394.

A test was also made for the Purkinje effect. The one that proved successful was to ask the patient to compare the brightness of red with blue, first in full daylight and then in reduced daylight. There was no change in the relative brightness for the patient, while for the experimenter and another with normal vision the red was markedly darker in the reduced illumination, and noticeably brighter in full daylight.

All four of the characteristics of vision that are ascribed to the rods by the duplicity theory were lacking in this individual suffering from hemeralopia. The peripheral brightness vision is lacking, adaptation to reduced illumination shows only the stage that is run through by the normal eye during the first 5 or 6 min. There is no photochromatic interval. When a color is too faint to be seen as color it is not seen as brightness. In consequence there is no vision at all in the light normally present outdoors at night. The Purkinje phenomenon is altogether lacking. Taken together these facts constitute an interesting complement to the phenomenon of total color blindness in support of the duplicity theory.

It should be added that tests with the anomaloscope showed entirely normal color vision. Oguchi asserts that this is not always true for the hemeralope.[3]

Unfortunately we could obtain no definite evidence of heredity. A maternal uncle was said to have gone blind at thirty-eight years. The blindness was ascribed to cataracts, but the Christian Science belief of the family probably prevented a competent diagnosis. The patient had no report of other cases of blindness in the family. He did not mention brothers or other uncles on the maternal side.

The diagnosis of *retinitis pigmentosa* was made by Dr. Fralick of the Department of Ophthalmology who kindly agreed to see the patient after the writer had made his tests. Ophthalmoscopic examination left no doubt of the nature of the disease. Dr. Fralick also found markedly reduced visual acuity. The right eye with glasses showed 6/21–1 and the left eye 6/30+2.

Hemeralopia is relatively rare. Dieter found only eight of his idiopathic form in 200,000 individuals examined. The different forms seem to occur rather more frequently in Japan as most of the twenty or more articles in the last twenty volumes of Von Graefe's Archiv are from Japan. This may be due to the interest that Oguchi has aroused among his colleagues there or may indicate an actually greater frequency of occurrence. The rarity of the occurrence of the disease seems to make it worth while to report this case even if time were not available to prepare or secure instruments for the most complete examination.

University of Michigan W. B. PILLSBURY

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[3] C. Oguchi, Weitere Mitteillungen über die Oguchische Krankheit, *Arch. f. Ophthalmol.*, 97, 1926, 208-218.

SYSTEMATIC DIFFERENCES IN INTROSPECTION

In the course of a recent investigation the writers had occasion to use a group of *Os* of diversified systematic interest. There were 3 psychoanalysts (M.D.'s), 5 eclectics (Ph.D.'s whose training had been primarily behavioristic), 1 structuralist (Ph.D.), 2 Columbia University graduate students in psychology, 1 undergraduate psychology major from Colgate University, and 2 psychologically naïve individuals. These *Os* were asked for introspective reports of "what happened in your consciousness" during a series of stimulus-situations designed to evoke a wide variety of experiences. No attempt was made to demand introspections from the *Os*, and, when they reported "nothing," this statement was accepted without comment. Nor was there any attempt to enforce certain categories of report. The *Os* simply reported conscious content in their own terms, such as "pleasant feeling," "surprised," "effort," etc. These reports have been assembled in the upper part of the accompanying table, and classified by the present writers under the following types of experience: affectivity, check, conation, emotion, expectation, intellectuation, tension, and miscellaneous. The average number of reports per *O* for each of these categories is given, with the *Os* classified according to their systematic interests.

AVERAGE NUMBER OF RESPONSES PER OBSERVER

Classification	No. Os	Affect.	Check	Con.	Emotion	Expect.	Intell.	Ten.	Misc.	Total
Psychoanalysts	3	2.7	5.0	3.7	11.3	0	1.0	0	1.3	24.9
Naive	2	0.5	8.5	7.0	6.0	2.0	4.0	0.5	1.5	30.0
Eclectics	5	2.4	9.6	6.2	10.6	0.8	2.4	0.2	1.8	34.0
Students	3	1.0	11.0	15.0	16.3	1.3	0	9.0	3.7	57.2
Structuralist	1	9.0	12.0	9.0	5.0	1.0	2.0	2.0	10.0	50.0
Dementia praecox	4	1.5	1.0	5.5	2.8	.3	1.3	0	0	12.3
Manic depressive	4	1.8	4.3	4.3	4.3	0	0.5	0	0	15.2

	Individual Responses (First list)	Individual Responses (Second list)	Sexual Responses	Sexual Introspections
Psychoanalysts	13.0	7.0	1.6	3.6
Naïve	8.5	1.5	0.5	0
Eclectics	5.8	1.4	0.4	1.2
Students	8.3	4.3	0	0.3
Structuralist	6.0	5.0	0	1.0
Dementia praecox	6.0	7.5	0.7	1.0
Manic depressive	8.3	6.2	0	0

These *Os* were also given two word-association lists, the first one derived from the Jung list, and the second, briefer one, from other sources. On the first list the *Os* were asked to supplement their responses with introspective reports of what went on in consciousness during the word response. The association word responses were inspected and scored for individual responses (significant or unusual reactions of diagnostic value) according to the Kent-Rosanoff technique. The second part (the extreme right portion) of the accompanying table lists the average number of individual responses on each of the two word-lists, the number of definitely sexual association words given by the *Os*, and the number of introspections (of the conscious states accompanying the word responses) which contained further sexual material. While the number of *Os* is not sufficiently large to make the results significant, the writers feel that they are interesting. A group of 4 dementia praecox cases and a group of 4 manic-depressives were added for contrast.

One notices immediately, upon looking at the first part of the table, that of the professional groups the lone structuralist leads in the number of conscious states reported. He is surpassed, however, by the graduate students. The difference between these two groups and the others is great in amount of material reported, although there seems to be no particular trend of any one group to favor a specific type of conscious content. The only effects that systematic bias would seem to introduce here are in the direction of limiting or restraining the quantity of an *O*'s performance in introspecting. The comparative paucity of the material reported by the psychotic groups shows the relative difficulties of using introspection with such patients.

The only outstandingly different group on the word-association material as seen in the lower part of the table is the group of psychoanalysts. On both the long and short lists the psychoanalysts lead in the number of individual or diagnostic responses, the number of sexual word responses, and the amount of further introspections containing sexual material. Is this due to systematic preoccupation with such matters or to an acquired liberation from normal restraint? It is interesting to note that the abnormal groups do not differ significantly from the normals in number of significant word responses and sexual matter reported.

Connecticut College for Women WILLIAM A. HUNT
Columbia University CARNEY LANDIS

THE NINTH ANNUAL MEETING OF THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

The Ninth Annual Meeting of the Midwestern Psychological Association was held May 10-12, 1934, at Purdue University.

On Thursday evening, May 10, there were three symposia: The first on "The Relation of Psychology to the Social Sciences," was directed by R. H. Wheeler, University of Kansas. Dr. J. F. Brown spoke on "The New Field Theory of Social Behavior," and Dr. Wheeler on "The Future of Psychology in Social Science." Both speakers emphasized the point that if there is to be an adequate social psychology in the future as a basis for social science in general, atomistic assumptions and thinking must be abandoned. Brown suggested ways and means of applying principles of dynamics to social behavior and Wheeler pointed out that science and ethics are convergent.

The second symposium dealt with "The Relation of Psychology to Psychiatry." Dr. Martin L. Reymert presided. The speakers were Dr. Mandel Sherman and Dr. Lowell Selling on Psychiatry, and Dr. C. H. Calhoun and Dr. Luton Ackerson on Psychology. An interesting discussion developed regarding the relative scopes of the two fields and the competence of psychologists to engage in clinical work.

The third symposium was concerned with "Reading Disability." Dr. T. G. Hegge presided. Dean William S. Gray was the introductory speaker.

There were two programs Friday morning, both devoted to physiological psychology. Papers were read by E. E. Anderson, University of Illinois; John D. Layman, University of Chicago; Norman R. F. Maier, University of Michigan; A. H. Maslow, University of Wisconsin; John E. Wenrick, the Ohio State University; C. W. Darrow, Institute for Juvenile Research, Chicago; D. B. Lindsley, Harvard Medical School; G. Finch and E. Culler, University of Illinois; Louis D. Goodfellow, Northwestern University; and H. Werner, University of Michigan.

Psi Chi fraternity held a luncheon Friday noon.

In the afternoon there were two sessions, one dealing with problems of infant and child behavior and the other with systematic problems. Professor G. D. Stoddard, University of Iowa, presided at the first and Professor J. P. Porter, Ohio University, at the second.

Papers were read by F. C. Dockeray, Ohio State University; Melvin S. Hattwick, Child Welfare Research Station, University of Iowa; Fred Brown, Bureau of Juvenile Research, Columbus, Ohio; James H. Taylor, Ohio State University; Richard Ledgerwood, University of Illinois; Christian A. Ruckmick, University of Iowa; L. L. Thurstone, University of Chicago; Hulsey Cason, University of Wisconsin; R. H. Wheeler and F. T. Perkins, University of Kansas; and J. F. Brown, University of Kansas.

A dinner was held Friday evening. The toastmaster was Professor Herbert Woodrow, University of Illinois. An address of welcome was given by President Edward C. Elliott, Purdue University. Dean A. A. Potter, Purdue University, gave an address on "Psychology in a Technological Civilization," followed by an address by the retiring President, J. J. B. Morgan on the "Psychoses of Business." It was emphasized that the fundamental problems of economics and business are psychological in character.

Saturday morning there were two sessions: one on tests in applied psychology in general; the other on memory and learning. Professor J. B. Miner, University of Kentucky, presided at the first and Professor S. L. Pressey, Ohio State University, at the second. Papers were read by Harvey C. Lehman, Ohio University; Clair V. Mann, Missouri School of Mines and Metallurgy; N. C. Meier, G. Mennenga, and H. J. Stoltz, University of Iowa; James P. Porter, Leonard L. Henninger, and Charles E. Fiddler, Ohio University; C. C. Ross, University of Kentucky; Floyd L. Ruch, University of Illinois; Grace O. McGeoch, Stephens College, Columbia, Missouri; Fred McKinney and John A. McGeoch, University of Missouri; Clellan L. Morgan, Purdue University; R. H. Waters, University of Arkansas; and R. W. Leeper, University of Chicago.

Saturday afternoon there were reports from the Laboratories. G. C. Brandenburg, Purdue University, presided.

The Annual Business Meeting followed.

Dr. John A. McGeoch, University of Missouri, was elected President. The University of Kansas was selected as the place for the 1935 meeting.

University of Kansas RAYMOND H. WHEELER

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

The Energies of Men: A Study of the Fundamentals of Dynamic Psychclogy. By WILLIAM McDOUGALL. New York, Charles Scribner's Sons, 1933. Pp. ix, 395.

The author presents in this volume "the most essential parts" of his *Outline of Psychology* and his *Outline of Abnormal Psychology.* The new volume is not, however, a scissors and paste mosaic but has been written as a completely new book. Six of the twenty-four chapters are devoted to abnormal phenomena, as such, and the remaining eighteen are upon the material of general and comparative psychology. The six chapters on the abnormal follow the rubrics and more nearly the content of the corresponding *Outline,* while the eighteen others show many changes from the *Outline* in organization, emphasis and in specific point of view.

The major features of the author's purposivism are well known, as are the criticisms and counter-criticisms which it has provoked. This review will, therefore, concern itself primarily with what McDougall considers the two principal novelties of the volume, viz., the treatment of the instinct-intelligence problem and an outline of a theory of learning.

Instinct, in its most generally accepted sense, is said to be peculiar to the lower animals and its application to man has led to confusion and controversy. The word is, therefore, avoided and the term *propensity* is substituted. A propensity is "any part of the innate constitution whose nature and function it is to generate upon occasion an active tendency" (p. 64). "A tendency is an active energy directed towards a goal" (p. 64). "A propensity 'geared' to certain abilities is, then, the innate basis of a train of instinctive activity directed to some particular goal . . ." (p. 49). the old dichotomy between instinct and intelligence is false. Although the direction of behavior is innately prescribed, the behavior is also adapted in some degree to the specific conditions of the moment by virtue of the organism's appreciation of such relations among the things dealt with as are relevant to the attainment of the goal.

The human species has at least eighteen propensities which are enumerated in a "tentative and approximate" list (p. 97). They vary from food-seeking and sex through appeal and laughter to a comfort and a migratory propensity. Human beings possess all or most of the propensities of the rest of the mammals and perhaps some additional ones, together with richer and more plastic native abilities.

The substitution of "propensity" for "instinct" implies a greater flexibility and generality and nominally avoids the "instinct controversy," but only nominally. A majority of the now well-known arguments are as cogent against one as against the other. The list of propensities is, likewise, still an arm-chair list. No evidence from controlled observation is offered for the inclusion of each propensity; we are offered only some general field observations in support of the existence of propensities in lower animals and the deduction that, since animal life is a continuous series, man must have nearly the same propensities as do the animals and probably more besides.

In this volume for the first time McDougall explicitly asserts learning to be one of the principal problems of a theoretical psychology, although the discussion of learning is left until the next to the last chapter and although little explicit use is made of the concept of learning in the treatment of other problems. For him learning has two phases: (1) adaptive achievement; and (2) memory or retention. Cutting across these two is the problem of fixation and elimination, "the Waterloo of the triumphant mechanists" (p. 350), to which a McDougallian law of effect is offered as a solution. The Thorndikian law is held to be inadequate because the "pleasure" (actually, Thorndike says little of pleasantness and unpleasantness in his later writing) invoked does not appear until the movement which led to it has ceased. Since "causes must precede their effects" (p. 353), this after-effect cannot be a selective agent. McDougall's law, on the contrary, meets this difficulty and may be called a law of the effect of foresight. Its essential point is that *"as you foresee the attainment of the goal and the steps necessary to the attainment of it, you experience something of the pleasure of success;* the pleasure of success *accompanies* the making of those movements; and, according to the fundamental law of feeling, it reënforces, sustains, invigorates, those movements" (p. 354).

It may be questioned how, in a great many learning problems, the subject can foresee the effect of a response unless earlier unforeseen effects, or other earlier conditions attendant upon practice, have prepared him to do so. Foresight frequently requires learning and cannot be offered to explain learning. The meaning of foresight in the context of learning is not, itself, unequivocally treated. Pleasure and pain are not, moreover, the sole effects which accompany action. The theory is not worked out in detail, nor is the treatment of opposing theories or of the facts of fixation and elimination. As it stands it offers little aid to our understanding of the problem of selection.

The book might, except for its use of the data from a very few experiments upon animals, have been written in the preëxperimental era. There is scarcely even an indirect reference to the experimental work on human beings and the atmosphere of the laboratory is uniformly lacking from the generalizations. They are supported by deduction or by argument from non-experimental observations. Many of the chapters deal with problems on which little experimental work has been done and few of the well-worked experimental problems are even touched upon. Quantitative statements are wholly lacking. Much of the book is, instead, a kind of literary psychology in which deduction from postulates, self-analysis, and a shrewd casual observation and *ex cathedra* assertion mingle in varying proportions. Nevertheless the reader is assured that the systematic structure here set forth will presently be recognized generally as the only acceptable one.

If, however, one grants McDougall's postulates, the better part of his system follows coherently. It is, moreover, written smoothly and interestingly at a high conceptual level in the best manner of the tradition of Bain and Stout and Ward. There is no doubt of the scholarly power of the author nor that he writes from an accumulated organization born of mature reflection. Psychology needs more systematizing at a high conceptual level in which our multitudes of disparate facts are ordered in a large perspective, but contemporary psychology permits and requires concepts dripping with experimental particulars and subject to incessant alteration, not a closed system which needs observation only to clarify and illustrate it.

University of Missouri JOHN A. MCGEOCH

An Introduction to Animal Psychology, the Behavior of the Rat. By NORMAN L. MUNN. Boston, Houghton Mifflin Co., 1933. Pp. xxii, 439.

Munn's *Introduction* is a competent and thorough survey of the literature dealing with the behavior of the laboratory rat. It is clearly and simply written and no reader need feel handicapped because of theoretical or systematic background. Munn devotes more space to reviews than to interpretations and he keeps the interpretations distinct from the reviews. His book is primarily a systematic condensation of journal articles and, secondarily, a criticism of them. His interests appear to center more about experimental than systematic problems and although he makes use of Hunter's version of behaviorism, he does so for purposes of classification and for his own interpretations. These interpretations may not always be accepted but they are characteristically tempered, conservative, and critical. Munn does not point out reasons for the study of animal behavior. He does not refer to "behavior" and "mind." He simply takes for granted the obvious fact that animal experimentation is established and proceeds with his survey. He is interested in animal psychology, not comparative psychology. In these respects he differs from his predecessors.

Certain principles that appear in Munn's interpretative comments may be of interest. His usage of the term "reflex" does not imply the independent existence of discrete units. Apparently he means nothing more than specialization when he says, "The data on fetal behavior indicate that the specialized reflexes result from the differentiation within a large general pattern involving the whole body." His discussion of the conditioned response theory of learning is, however, atomistic in one sense. He believes that "learning is probably the development of hierarchies of conditioned responses" and that "this theory assumes that learning is the acquisition of positive and negative conditioned responses." His use of "hierarchy" is not clear and he appears to mean succession rather than generalization, as is illustrated by his applications of the theory to discrimination problems. One criticism may be urged here of his exposition. In his theory of learning he does not take into account the more generalized factors found in maze work such as orientations to direction of food, to landmarks, to shifts of direction in complex mazes, although studies of such factors are listed and summarized on various pages. He believes that responses may be made to relationships between stimuli in discrimination problems but he does not indicate how his theory accounts for it. Finally, he does not account for the fact that problem boxes are solved not by an invariable sequence of the same movements. In brief, the theory as outlined here overlooks that kind of behavior that is referred to by such terms as "set."

Munn prefers stimulus-response descriptions and he objects to "lumping the specific behaviors together under blanket terms." His objections to blanket terms are to a large degree justifiable but he does not consider the possibility that blanket anthropomorphic terms may be useful. Thus, it may be possible to speak of "neurotic rats" because their behavior bears some resemblance to that of human beings in similar situations, if, of course, it is possible to speak of "neurotic human beings."

For similar reasons, Munn objects to the term insight. Sudden mastery is the outcome of implicit learning and to call the behavior insight does not explain how the insight has arisen. Furthermore, he says, there is little evidence for sudden mastery in the rat. He does not consider the criticism that the piecemeal nature of the usual experimental situation prevents the appearance of sudden mastery. Neither does he point out that the learning which does occur in such situations still requires explanation.

The conditioned response theory is applied to a discrimination problem, a maze habit, and a puzzle-box. Munn does not regard this theory as an explanation for he considers it necessary to go to the nervous system for explanation. He points out several differences between Pavlov's situations and those of the animal psychologists which may permit conditioning beyond the third order and overcome the fragility of the Pavlovian phenomena. Otherwise, Munn's presentation is similar to those contained in many text-books, and they may all be criticized on the ground that they make use of only one set of conditioning phenomena; namely, the attachment of a response to a new stimulus. No attempt is made to apply the various relationships introduced by extraneous stimuli, inhibitions, etc. If these phenomena are fundamental and valid, a conditioned response theory worthy of its name ought to include them.

The rat's behavior, according to Munn, is not an invariable sequence of identical movements. For example, he considers the problems of tropistic behavior as unsettled until the variabilities are accounted for, and maze behavior as controlled by multiple and variable stimuli: "the kinaesthetic chain-response explanation of the maze behavior . . . has not been substantiated." (It might be remarked here that Munn identifies kinaesthesis with kinaesthetic chain-reflex or response, as do many others. What the data refute is not kinaesthesis but kinaesthetic chaining.)

In his condensations Munn has, on the whole, avoided important distortions and in only one case has the reviewer noticed a misinterpretation. On p. 227 Munn says that Tryon "concludes that the high correlation between performances on different mazes indicates a rather *general capacity* to learn." What Tryon says is this: "It is concluded, therefore, that *relative to the maze abilities dealt with here,* the high correlation between them indicates the existence of a fairly general capacity to learn" (italics not in the original). Tryon's data were restricted to his two mazes and the capacity is general only in that sense.

The necessities of conservation of space require omission of many details but in certain cases, the importance of the experiment is indeterminate without certain information. For example, in view of the recent studies of kinaesthesis, the Carr and Watson experiments cannot be evaluated without knowing the number of animals, number of trials, etc.

A brief review of the experimental content may be of service. The chapter on unlearned behavior overlooks none of the important work, although to the bibliography should be added Schaeffer's work on spiral movement. The chapter is marked by an able review of the Lamarckian experiments and of tropistic behavior, although the latter might have received less space.

The chapter on activity and drive ("persistence of response in the presence of obstruction," an awkward but descriptive phrase) contains descriptions of standard apparatus and methods. Similar plans are followed with good effect in the chapters on sensory processes and maze behavior. The chapter on the learning process is comprehensive except in the discussion of reliability of methods. Here the distinction between differences between means and differences between individuals is not sufficiently clear, and the importance of the methods of obtaining the coefficients warrants more extensive treatment than they have received.

The chapter on the nature of learning is interesting for its general characteristics of learning but its section on learning theory, engrossed, as are many other such discussions, in terminological problems, appears weak.

Symbolic processes are discussed in the shortest chapter of the book. This should not be taken to mean that there are relatively few experiments on these problems but as illustrations of the difficulties of classification, for the previously discussed work on direction orientation, insight, etc., might have been brought in here.

It may be well to glance over the field of animal behavior as Munn has pictured it. A comparison of the bibliography on the behavior of the rat with that on other animals shows that rat workers have not been greatly concerned with social behavior. Neither bibliography shows much work on abnormal behavior, personality characteristics, etc. Relatively few studies involving long-time control of the development of behavior are reported. Except for very recent beginnings along these lines, the primary interests of rat psychologists seem to be motivation and learning.

In summary, it may be said that Munn's book is of undoubted value. The reviews are generally excellent, the interpretations conservative, and there is little danger of confusing data with these interpretations. The book is more nearly what "rat psychology" actually is than what Munn thinks it is. Finally, the reviewer's negative criticisms are of less importance than the systematic collection of the many independent investigations.

University of California A. G. BAYROFF

The Correction of Defective Speech: A Complete Manual of Psycho-Physiological Technique for the Treatment and Correction of the Defects of Speech. By E. B. TWITMYER and Y. S. NATHANSON. Philadelphia, P. Blakiston's Son & Co., 1932. Pp. xviii, 413.

The purpose of this book is "to provide material that shall offer a technique for the correction of all types of speech mutilations." It has particular "applicability to the unit theory of etiology." It is not presented in defense of a specific theory but in response to the existing need for suitable material. It is not, strictly speaking, a scientific treatise. It is practical and pragmatic. "Our aim," says the writer, "has been to accomplish correction by the employment of natural methods, the resortation of proper coördination through means which are normally employed during the acquisition of speech." The coördination referred to is evidently that of breathing and oral position. The statement, nevertheless, is somewhat misleading for the exercises recommended in Part II could hardly be characterized as natural. Besides, the motivation during the natural acquisition of speech is very different from that during the remedial teaching period. The book is divided into three parts, the psycho-physiological approach, corrective material and technique (the largest part), and the case record.

After citing various causes of defective speech the writer concludes that, "the nearest approach to a common factor is a disturbance in the normal rhythm of breathing." It is doubtful if deafness or brain injury contribute anything to this common factor. "Approximately 85% of all stammerers display such breathing disturbances to a lesser or greater degree." The question remains: are those disturbances continually present, say, in sleep, or are they associated with speech alone? If so, can it be definitely stated that stammering is the result rather than the cause?

Part I includes a treatise on phonetics. Instead of the traditional terms, vowels and consonants, the words 'continuants' and 'stops' are employed, but to no evident advantage. The classifications of consonants are to some extent different from the conventional. There is no separate class for the nasal consonants because they are bilabial and are classified with *p* and *b*. *N* is put with *t* and *d*, *ng* with *k* and *g*. The fricative *th* is classed with *s* and *z*. *L* and *r* are labelled together in spite of the fact of the vibratory quality of *r* which is absent in *l*.

The authors consider it unnecessary to classify *wh* because "in actual speech *wh* is a combination of the *oo* sound with what follows." Are *whale* and *wail* then pronounced alike? *Y* also remains unclassified because "it takes on the character of the subsequent vowel by merging with it from the *ee* position." Does this classify the current pronunciation of *yield?* Some pronunciations of *hideous* and *million* produce something more than *ee*. There are sounds in the English language without symbols and symbols without sounds and no classification is complete without taking that fact into account. A knowledge of phonetics is essential to any exact or even practical classification.

It may not be asking too much from a speech clinician to set up some standard of pronunciation, or at least one acceptable within a prescribed geographical area.

The authors state that "the material is arranged so that each new sound is introduced in *proper* relations to sounds already learned." No attempt is made to find out by scientific methods what the "proper" relations are. The classification (or order) is based "on a combination of facility of expression and pedagogical methods superimposed on anatomical correlatives." The scientific value of such a basis is not beyond question. It does not prove, for example, that *n* is easier than *m*.

In Part II, "corrective material and technique," each "stop or consonant is connected with the five vowel sounds. The long vowels are used. Such combinations as *t-e*, pronounced as *tea*, *t-a* as *tah*, *t-o-o* as *two*, *t-o* as *toe*, and *t-i* as *tie* are repeated ten times each. Later *e-t* (eat), *a-t* (ate), *oo-t*, *o-t* (oat), *i-t* (ight) are repeated. Later, words and sentences containing the sounds are practiced. By using the long vowel sounds without the correct spelling for these from the beginning much confusion might arise for the student; *e.g.* he is drilled in responding to *i-t* as *ight*, later to find that the proper pronunciation is *it*. *O-n* is at first pronounced *own;* later it must be called *on*. This method may correct imperfection of pronunciation but at the expense of confusion in learning to read and spell.

In all there are one hundred sections devoted to word lists and sentences for drill. These are without doubt a valuable contribution to the material for remedial speech training, and are the most valuable part of the book.

Institute for Juvenile Research ANDREW W. BROWN

Studies in Expressive Movement. By G. W. ALLPORT and P. E. VERNON. New York, Macmillan Co., 1933. Pp. xiii, 269.

This book presents an experimental study of consistency in bodily movements. It seeks to determine whether the typical movements of humans are marked by identifying characteristics, and whether there is correspondence in style between the various activities, indicative of some common permeating factor or factors. It is the problem of the reliability of the data on which all personality studies are based. The problem of what is expressed must have as its antecedent the problem of the dependability of the means of expression.

Some 300 measurements were obtained of different aspects of various normal activities of twenty-five men. Typical measures were of normal speed in writing, drawing, talking and rythmic activities; in area covered in writing and drawing; of the length of stride; of grip strength; of pressure in writing and tapping; of the estimation of distance between the hands and to various points outside the body; of weights and of the sizes of angles. There were also several ratings of action traits. More than 30 tests were given.

The average of the (uncorrected) repeat reliabilities for the separate tests is 0.68. On the basis of high intercorrelation of the constituent items, 17 composite measures were constructed with an average of about 6 single measures in each. In all, 34 variables were discovered with an average corrected reliability of 0.81. On the basis (essentially) of intercorrelations three more comprehensive measures were also built up. The first, of a composite nature, was called the *areal factor*. It deals with motor expansiveness, as indicated, *e.g.* by covering a large area in writing. It was composed of nine measures and gave a corrected internal consistency of 0.82. The second measure was of a *centrifugal group factor*, or tendency toward radial movements to and from the body. It was composed of seven measures with a consistency of 0.75. The third was the *emphasis factor*, composed of 13 items with a consistency of 0.82. It deals with forcefulness, as expressed, *e.g.* by pressure in writing. These three factors seemed irreducible and no single, general factor was discovered.

It is a matter of significance that this important study is truly inductive. It starts with no logical premise or hypothesis to be tested as does the traditional study of personality. Its categories are derived mechanically from the data. They are named and described, after discovery, through statistical analysis. To the reviewer this seems a very worthy achievement. The danger of the procedure seems to lie in the fact that the group measures are built up on the basis of some rather small differences in correlations. For example, tapping pressure goes into the *emphasis factor* apparently on the basis of a 0.42 coefficient, rather than in the *centrifugal factor* with a 0.34 coefficient. The probable error of the difference between some of these coefficients is obviously greater than the actual difference. This does not deny, of course, that these coefficients are measures of actual correspondence in the data on hand, nor that they could be combined to produce group factors of the kinds described. The question is whether, with a set of 25 different *Ss*, considerably different intercorrelations and a very different set of composites might not appear. Naturally the intensiveness of such a study forbids its being very extensive. Such limitations are probably unavoidable in a preliminary piece of work.

A second question might arise in regard to the psychological "congruence" observed between the constituent items of the groups after they were constructed. There seems to be danger that one might "read in" or rationalize a meaningful relationship where it does not really exist. An example might be the reference to speed in talking, an item in the centrifugal group, as "verbal centrifugality." As a trail-blazing study, it is a worthy contribution.

The latter part of the book is a report by Edwin Powers of an experiment in matching descriptive sketches of the personality of 10 men with samples of their handwriting. Matchings were correct to an extent only slightly greater than that based on chance. It is recommended that studies of handwriting be in terms of patterns rather than of elements.

University of Colorado THOS. H. HOWELLS

The Psychology of Intelligence and Will. By HORACE G. WYATT. New York, Harcourt, Brace & Co., 1931. Pp. viii, 273.

Here is a thorough and penetrating discussion of the specifically psychological nature of intelligence and will. The relations of these activities to one another and to other processes is also considered. It is refreshing to read a book like this, so different from the usual standardized texts.

Intelligence is viewed as the capacity to apprehend true or useful relations, or discover subjectively, between objects, relations which hold objectively (p. 43). It is argued that the explanation of intelligence on the basis either of conditioned reflex theories or those of associationalism is inadequate, because of the failure of these doctrines to account for the organization of alternative patterns of behavior, or series of interrelated acts, which is characteristic of intelligence. The principle of the Gestalt school escapes this criticism, but it is argued that relations are first learned, and then, by means of them, actions are put together into patterns; as when one of Köhler's apes siezed a short stick and reached with it for a long stick with which it then drew in a banana. It would seem as if it might be more convincingly argued that animals and men discern first the forms and patterns of things and of their own behavior in regard to them. In the discernment of these patterns relations are implicit or marginal which may afterwards become focal. If ideas of relationships are thus acquired they may still later be used to construct new patterns of things, thoughts, or actions. This, however, is intelligent behavior at one of its highest levels. It would seem sufficient for one of Köhler's apes, after pausing "to think things over," suddenly to "see" a behavior pattern in which short stick, reaching, grabbing long stick, and pulling in banana were constituent parts, but in which ideas of particular relationships were very vague. In other words it would seem more probable that in general ideas of relationship are acquired from patterns, rather than patterns being first built up by the use of ideas of relationship. An example at a still higher level would be early Greek geometry, the first of the pure sciences, which developed from the analysis of forms familiar to everyone, the line, the triangle, the square, the circle, etc., but whose precise internal relationships were in general unknown until the science demonstrated them.

In regard to volition an excellent summary appears on p. 198. Volition is merely a term for the ordinary fact of self-activity, and is thus in a degree present in all life. It adds no extraneous factor. Consequently the recognition of volition and intelligence as factors in human behavior does not involve the fallacy of hypostatization. Quite the contrary. For this fallacy in psychology lies in endowing abstractions like ideas or neurones with intelligence and volition in order to help out the explanation of behavior.

Volition is present at all levels from stentor to man. In man at its highest level it functions in the choice between impulse and a principle discerned by intelligence, which principle observes uniformities, in opposition to behavior as a chaos of conflicting impulses. From this point the passage to the treatment of ethical problems on a broad psychological basis lies open before one; and for Wyatt psychology opens out into ethics, as it does with Aristotle. "Once the significance of the distinction between the life of impulse and the life of reason is clearly grasped, we understand at once why the martyr is a more typical instance of volitional behavior than the big financier is. For the martyr represents behavior according to a principle based not on impulse, but on principle as such. He has exercised his intelligence freely, not in the service of impulse."

Critical reviews and discussions of current theories of intelligence and will occupy a considerable portion of the book. Perhaps the majority of these other theories have as their background the philosophy characteristic of Democritus and La Mettrie. That a twentieth century writer should prefer one more like that of Aristotle and Thomas Aquinas is not so surprising as some might think, in view of the changes which have occurred in our views of the nature of the physical world. Wyatt's treatment of the subject is thoroughly modern, and his application of more fundamental principles than those underlying several of the modern views is what gives interest and significance to the book.

Hobart College FOSTER P. BOSWELL

Adolescent Psychology. By ADA HART ARLITT. New York, American Book Co., 1933. Pp. viii, 238.

This textbook is "intended primarily for students in colleges, universities, and teachers colleges and for educators of adolescents" (p. vii); but for anyone who has had a thorough course in general psychology it is rather elementary.

A comparison is made between the preparation of primitive youth for adolescence and that given by modern education and the influence of social institutions to the youth of today. The treatment is based upon the more recent research studies of primitive tribal customs. The discussion of physical changes now known to take place at adolescence is accurate and complete. The author's discussion of instinctive tendencies adds nothing to clarify this controversial problem. A "middle ground" view is taken. At adolescence it is difficult to differentiate between patterns that are native and those that are acquired, but, "certain of these tendencies tend to operate more strongly at adolescence than at any other period while still others operate strongly before six years of age and again at adolescence" (p. 34). It is not easy to accept "gregariousness," "play," the "tendency to do as others do," which is interpreted as imitation, or the "tendency to become uncomfortable at the sight of suffering" as native responses. The development of homosexual interests in some individuals is attributed to social conditions which block normal sex activity. There is no reference to recent experiments in biology which show the relation between such interests and the functioning of the endocrine glands.

The treatment of emotions and problems of emotional adjustment of the adolescent covers four chapters. A brief explanation of the physiological changes that take place in an emotional response is followed by a discussion of social situations that cause insecurity and conflict in the attitudes of the adolescent. The mechanisms by which some children try to escape from a feeling of insecurity are discussed in a practical way, but the terms 'sadism,' 'masochism' and 'exhibitionism' are defined inaccurately. For example the definition of sadism is "the desire to hurt. Satisfaction is gained from inflicting pain on persons and animals" (p. 238). This is explained as a normal tendency of the child which may become exaggerated in the adolescent as a desire to hurt the individuals he loves. This term is generally used in abnormal psychology to apply to gratification of the sexual impulses by inflicting pain on the object of sex interest.

Two of the four chapters on the emotions present the problems of emotional maturing, and here the author departs from her earlier accurate biological and social explanations of the emotions and the environmental causes of acquired emotional attitudes and presents Flügel's interpretation of the Freudian view of emotional development. The inconsistencies of this view and the lack of experimental evidence to support it are not made more logical by such inadequate statements as: "Stages [which evidently refers to the 'narcissistic' period, the 'mother fixation' period, etc.] may be skipped entirely or may be telescoped. No girl should be discouraged from passing through any of the stages. With her as with the boy, it is probably necessary that all of the stages be passed through if a normally developed love life is to result" (p. 102).

The treatment of memory and reasoning is well done and should prove of practical value both to educators interested in adolescents and to adolescent students themselves. The problem of intelligence and mental growth is less satisfactory. Representative definitions of intelligence are given with little interpretation. The "three-faculty theory" of mental organization is briefly discussed. Spearman's two-factor theory is presented as he first stated it without any reference to or critical evaluation of his modification of this earlier view. In a footnote (p. 145) Thorndike's conception of the three types of intelligence—abstract, mechanical and social intelligence—is given without any reference to its originator. The Army Alpha results obtained from the American soldiers are accepted uncritically as the best indication of the mental ability necessary for success in the different vocations, and yet it has often been pointed out that these results must be interpreted in light of the environmental influences which have affected the different groups tested.

One chapter is devoted to personality and one to disturbances in adolescent personality. Watson's grouping of personality traits is followed by a list of specific traits from some of the earlier studies of personality. There is no reference to recent work. The Downey Will-Profile test of personality is fully described and, though it is stated that its value has not been proved, there is no reference to those experimental studies which have questioned its reliability and validity. The maladjustments treated are defense mechanisms, over-timidity, a sense of inferiority, day dreaming, over-compensation, the various forms of dementia praecox, manic depressive psychoses, hysteria, neurasthenia, psychasthenia, and the anxiety neuroses. General descriptions of these maladjustments and concrete illustrations of their development in adolescence are given.

The development of standards of moral conduct, the fundamental elements essential to moral behavior and the values and problems of religious development of the adolescent are presented effectively. The discussion of the hygiene of adolescence is brief and practical. There is a glossary of forty-three terms.

Georgia State College for Women EURI BELLE BOLTON

Diagnosing Personality and Conduct. By PERCIVAL M. SYMONDS. New York, Century Co., 1931. Pp. xvi, 602, 99 tables.

This volume summarizes in a comprehensive and rather thorough manner the researches to 1931 which deal with the methods and techniques employed in the study of personality and conduct in the non-intellectual aspects. The treatment is characterized especially by its practical applications and results, and its evaluations are constantly made in terms of the effectiveness of technique toward meeting the practical demands in character development, education, industry, and successful adjustment of individuals to their environment. "The book is primarily addressed to educators whose breadth of view causes them to see conduct as a primary concern of education" (p. vi). The author stresses the value of making use of techniques based on relationships established in experimental research, as opposed to the perils of resorting to mere personal judgment which are particularly apparent in case-study procedures. The intellectual factors have been omitted simply because they are so well treated in other works which are now generally available. Ninety-nine useful tables to facilitate calculation, to supply data on correlations, reliabilities of tests and rating devices, norms, pulse rates and blood pressures for different ages, are included.

The book considers especially factors and conditions making for best results in observational procedures; rating and ranking methods; the use of various questionary methods in the obtaining of data, attitudes, interests, and adjustments; tests of conduct, knowledge, and judgment; performance tests; the free association method; physiological measures of emotions; interviewing; psychoanalysis techniques; external signs of conduct; and measures of environmental factors. There is a final chapter on "The Case Study: A Comprehensive Study of the Individual," which emphasizes the need of bringing together all the relevant information bearing on the problem of the sort of behavior or conduct in question in any case. The "case study is not a research method. Primarily its function is to study the individual with a view toward helping him." In itself it is no guarantee of completeness or validity; it is "just as good as and no better than the methods employed in gathering the data" (p. 555 f.). Symonds suggests three cautions that are important in considering any abridgment of a case study: (1) "an investigator must draw conclusions on the basis of *proved* relationship" (2) the "study should not be abridged when one suspicious factor has been found, particularly when the relationship between this factor and the disturbing symptom is loose and uncertain;" and (3) the placing of value on "small correlations." He holds that practically all the high relationships have been found in studies of personality and conduct and that it is "well known that *extreme values* of factors which have low regression weights in the regression equation are potent for determining an issue" (p. 558 f.). He cites an example involving low relationship between cheating and low economic status of the child's family, in which the latter factor may carry with it several other related factors of significance. In such a case the constancy of relationship with these other factors is, however, important. Will this not require a further working out of correlations some of which are not small and studies by multiple-factor methods, not considered in the present volume?

After a summary study of the work on character by Hartshorne, May, and Shuttleworth, the author, on the basis of the summary data which he has presented in the volume under consideration, concludes that future research on the development and refinement of the diagnosis of conduct will emphasize standardization to make available suitable norms; will put particular stress on the interview, free association, and psychoanalysis as the most fertile fields for yielding to social scientists suggestions and ideas for elaboration and experimentation; will put stress on studying and improving reliabilities; and will investigate thoroughly the relationships among different variables and different sorts of data as the Character Education Inquiry has already attempted to do. He looks to various types of questionaries and rating methods as of most immediate promise.

The book is thus not only critically evaluative of the studies reviewed and of the tests and techniques employed, but it presents a hopeful view as to the future progress of thoroughly scientific investigations. One may find room for differences of opinion here and there, but will find the volume thoroughly worth while, and his indebtedness to the author for this valuable critical summary of a large number of researches great.

J. P.

Le Suicide. By CHARLES BLONDEL. Strasbourg, Librairie Universitaire d'Alsace, 1933. Pp. 133.

The practice of suicide is perhaps as ancient as the human race, but humanity has never become reconciled to this practice. An extensive literature written from the varied viewpoints of law, theology, ethics, psychology, medicine, and recently of sociology has developed in the attempt to explain the phenomenon.

The object of the present study is to define, evaluate, and establish the etiology of suicide in the light of a critical comparison of sociological and medical, or psychiatric, methods and conclusions. This is particularly important at the present time, since what Blondel calls the "imperialistic pretentions of sociology" sometimes lead its exponents to the strange folly of correcting specialists, in other fields. Moreover, Durkheim's rules of sociological method in demanding a social cause, usually determined statistically, for every social fact establish a misleading emphasis on those aspects of suicide which can be treated statistically and objectively. The varying physiological and psychological details of the clinical picture, however, are neglected or even dismissed as incidental or as merely introductory. These are most important to physician and coroner.

Blondel, a psychiatrist, prefaces his own theory with a brief, critical summary of the views of suicide entertained by sociologists and moralists of the 19th and 20th centuries, but perhaps relies too completely on French and German sources. Attention is principally directed to Durkheim's and Halbwachs' voluminous analyses of suicide from the sociological point of view, and the author has little difficulty in convicting these writers of inconsistencies as well as obscurity. Durkheim's definition of suicide, viz., "every case of death which results directly or indirectly from a positive or negative act of the victim himself, which he knows must produce this result," ignores the psychological element of intention, and the rôle of the collective representations which Durkheim generally stresses. Moreover, it confuses suicide with sacrifice, a matter of entirely different psychological motivation. Halbwachs, on the other hand, expressly reinstates intention as an element of suicide, and rejects its assimilation to sacrifice without relinquishing the demand for a purely social causation.

On the question of the moral appreciation of suicide, the author notes that Durkheim accepts the popular condemnation of suicide on personal grounds which his own sociological method would reduce to a preconception or prejudice. Bayet's extensive historical researches, leading to the interpretation of suicide as a function of conflict between the simple morality of the masses, which condemns, and the casuistry of the aristocratic or intellectual classes, which approves suicide, is vitiated by Bayet's confusion of suicide with sacrifice; and it appears that popular common sense, which judges both as moralist and alienist, is correct in its unfavorable attitude toward suicide.

The evolution of the psychiatric conception of suicide reveals a consistent belief in the pathological nature of the act throughout the history of medical thought on the subject, and this view is now more dominant than ever. On account of his intimate, detailed and professional experience with cases of suicide, the psychiatrist is regarded by Blondel as best qualified to judge of its causes. The psychiatrist's statement has become increasingly more precise from the semi-psychological theory of Esquirol, who stressed abnormal emotionality, to the modern detailed analyses of physiological and organic antecedents of morbid character. Esquirol's emphasis on psychogenic causes of suicide seems capable of assimilation in a social psychology of the stimulus-response type, so it is adopted by the French sociologists; and there is a similar emphasis on the psychogenic factor in the psychoanalytic schools, which might be invoked to support the views of the sociologists. The general tendency in psychiatry, however, is to eliminate psychogenic factors, and find the cause of abnormal behavior in organic, toxic, or constitutional factors. Having stated his thesis with which the majority of psychiatrists and alienists agree, Blondel returns to the sociological conception of suicide and devotes four chapters to the theories of Durkheim and Halbwachs as to the relative importance of different aspects of suicide, the limitations of statistical method, and the deplorable results arising from a one-sided approach to the problem. In this section Blondel embodies a number of critical conclusions of great weight. He feels that psychiatrists and sociologists could be of great assistance to one another if they could once agree upon their respective tasks.

An English translation of this work is desirable.

University of Cincinnati CHARLES M. DISERENS

Children of Preschool Age: Studies in Socio-Economic Status, Social Adjustment and Mental Ability, with Illustrative Cases. By ETHEL KAWIN. Chicago, University of Chicago Press, 1934. Pp. xv, 340.

The title of the volume "Children of Preschool Age" gives the one note of unity in this work which is really a collection of widely different studies. Part I, entitled "The Preschool Department of the Illinois Institute of Juvenile Research," and Part II, entitled "Research Studies," while both well done and of considerable interest, cover such entirely different fields and points of view that their inclusion in the same volume tends to detract from the general impression of the whole book.

Part I presents a detailed picture of the activities of the Institute of Juvenile Research and gives some fifty pages to case studies representative of the various functions of the Institute. This section will be most interesting to persons who are connected with clinic and consultation services for young children and for parents. The cases are presented in a clear and interesting manner with considerable detail and are definite contributions to the rather limited group of case histories accessible to the ordinary reader.

Part II presents three research studies on children of preschool age. The first on "Young Children of Low and High Socio-Economic Status: A Comparative Study of Their Performance on the Merrill-Palmer Scale" was carried out on the children in two nursery schools which served widely different social groups. The findings of this study "indicate that children of high socio-economic status excel in those functions which are measured by verbal tests and suggest the possibility that children of low socio-economic status tend to excel in some motor tests, while the former group are superior in others." However, "even though there is a mass tendency for high IQs to be associated with superior social and economic status, and vice versa, no prediction with regard to these two variables can be made with respect to an individual case. Children from both groups are found in the highest and also in the lowest percentile ranks. This implies the possibility that individual children may rise to intellectual heights quite out of line with the expected as based upon their social and economic backgrounds." The author reminds us that "we have never sought to discover through our tests what are the special inadequacies of the underprivileged group, with the view of trying to overcome those inadequacies by education and enriched environment" and she suggests that we might well attempt to determine "the qualitative differences in the abilities of children from different socio-economic back-grounds." If such differences can be discovered at the preschool level —"whether they be innate or acquired—it may be found that those functions in which any group is handicapped by its inferiority may . . . improve with especially directed educational and environmental opportunities."

The second piece of research reported is a study of "Social Adjustment in Children of Preschool Age." This section is of especial interest since it is among the pioneers in research work based upon case studies. The material used is admittedly not laboratory data but it is as detailed and as accurate as any material of the kind which has been published and the statistical treatment is as cautious and as accurate as one could desire. The author concludes that the social adjustment of a young child to other children outside of his own family does not appear to be so conspicuously related to any other single factor in the child's own make-up or environment that the one can be said to be the "cause" of the other. What this study does indicate is that there is a *group of factors* which appears to be related to the child's social adjustment. It appears that whether a young child is socially well adjusted or presents problems of social adjustments will depend upon a 'constellation' consisting of various factors in his own make-up and his life-situation. "Outstanding among these are the intelligence of the child, the occupation of his father, the relationship of the father to the child, and the agreement of the parents in regard to the child's training."

The third research section of this volume is concerned with the analysis of Stanford-Binet and Merrill-Palmer Test results on children of preschool age. The author concludes that the "Merrill-Palmer Scale appears to be better standardized, in regard to the adequacy of the age norms, than the lower ranges of the present Stanford-Binet. It appears, however, . . . to be subject to greater inconstancy on retests than does the Stanford-Binet Scale."

In summary we may say that Miss Kawin's "Children of Preschool Age" presents a number of research studies which are excellent examples of statistical method applied to material obtained by a service organization. The unity of the book is marred by the inclusion of a section describing in detail the organization and work of the Illinois Institute of Juvenile Research. The significance of the findings on the performance of young children of low and high socio-economic status on the Merrill-Palmer Scale as presented in the first of the research studies, is lowered by the results of the third research study which shows that this scale is subject to greater inconstancy than is the Stanford-Binet. Considered individually, instead of as a single unit, the studies are a distinct addition to experimental child-psychology.

Institute of Child Welfare, Chicago, Ill. JOSEPHINE C. FOSTER

Heredity and Environment: Studies in the Genesis of Psychological Characteristics. By G. C. SCHWESINGER. Ed. by F. Osborn. New York, Macmillan Co., 1933. Pp. viii, 484.

In this volume are gathered, interpreted, and critically evaluated the numerous studies of intelligence and personality which offer evidence concerning the influence of heredity and environment in the determination of individual differences. It is quite apparent that the author has the eugenist and general reader as well as the professional psychologist in mind. Approximately one half of the book contains material which to the professional psychologist is elementary. The eugenist and general reader, however, will find in the discussions concerning the history of testing, the nature of intelligence, the chief tests of intelligence and personality, the classification of personality types, and representative viewpoints on personality a well-informed, interestingly written yet critical introduction to the methods and concepts of the clinical psychologist.

The most important section of the book, covering some two hundred pages, is entitled "Studies on Genetic Factors and Stated Environmental Differences as They Affect the Development of Intelligence." This section is a distinct contribution to the subject. Not only does it offer a critical review of the important individual investigations, but it colligates their results in the form of suitable tables and graphs. The general trend of the findings, where such exists, is thus made clear. Enumeration of the individual problems concerning which the author presents data is impossible in a short review. To the reviewer's knowledge, however, no important problem is neglected. All of the twin, parent-child, sibling, foster parent, and other relevant data are presented. In addition there is a mass of material on the effect of physical deficiencies upon intelligence. A section in animal studies reviews Tryon's investigations.

The author is not a propagandist, hence carefully weighs the evidence and bases her conclusions directly upon it. The conclusions are to the effect that heredity and environment are both influential in the determination of individual differences in test intelligence. "Of the two sets of influences which have been examined for effect on test intelligence, namely, social-cultural and physical factors, the evidence weighs more in the direction of the former and less in the direction of the latter than uninformed preconceptions would have led us to believe. Of the beneficent culture influences . . . evidence has accrued to indicate the positive importance of good cultural environment of the home, urban residence, and some inconclusive evidence in favor of nursery school attendance. Retarding influences were found associated with bilingualism in the home and rural residence. But whether in the direction of heightening or of lowering the I.Q. certain conditions must be met before the above influences can become effective. These conditions are: (1) That the difference in cultural backgrounds of the home must be extreme, as compared with the 'average' American home; (2) that cultural change, if any, be introduced at a very early age; and (3) that change to the new environment . . . be maintained over a long time in the developmental period." Of physical anomalies, only deafness and impairment of the central nervous system appear to bring about any marked permanent retardation of test intelligence. The author suggests that the measurement of *general intelligence* rather than of many specific forms of behavior may mask important influences of cultural and physical factors. She says in this connection that "environmental handicap or advantage is reflected in specific test items much more distinctly and definitely than in the general composite of test intelligence, represented by I.Q.'s, M.A.'s, or raw scores. Preferably these specific tests should be of specific mental processes, unique in the psychological sense, as set down by Kelley, Spearman, and others."

It is clear from the author's presentation and interpretation of the experimental findings on nature and nurture that an extreme position in either direction is, to say the least, premature. "The evidence that there are important differences among individuals in hereditary capacity for intelligence is entirely conclusive; the variabilities and averages of large numbers of individuals under influences of varying environments are in process of being quite accurately determined." The influence of environmental factors, although quite evident, is clearly limited by the hereditary factors.

The book is thoroughly documented, and extensive bibliographies and suggested sources for further study add to its value. In an appendix are suggested many problems for future research. There are thorough author and subject indices.

One must conclude that the author has made a timely and significant contribution. The reviewer's only negative criticism of the book is that it appears to be overpadded with elementary material. To the reader not trained in psychology, however, this may well be an advantage.

University of Pittsburgh NORMAN L. MUNN

Invention and the Unconscious. By JOSEPH-MARIE MONTMASSON. Trans. with a Preface, by H. Stafford Hatfield. New York, Harcourt, Brace & Co.; London, Kegan Paul, Trench, Trubner & Co., Ltd., 1932. Pp. xxiv, 338.

The translator's opening statement in his preface is: "In this book, M. Montmasson is concerned to demonstrate a fact of the first importance, easily overlooked, . . . that human inventions in the widest sense of the word, are products of the unconscious." This 'fact' is to be demonstrated by the author in opposition to the view that invention is at the level of the highest point of self-consciousness. Since the author's treatment "is entirely from the point of view of introspective psychology," the translator is careful to give the orientation of the author with respect to vitalism and to mechanistic psychology and biology. One of the problems that the author, from his point of view, had to meet in this book is whether or not invention in man is the psychical correlative of mutation in organic development. Hatfield is convinced that it is, and he holds that the "investigations of the psychology of highly originative human types shows them to be generally abnormal and not far removed from psychopaths" (p. 15 f.). Montmasson calls the inventor "the pioneer of universal order," "constructor," "critic," and never once hints at pathology, but so far as the reviewer can see, he fails constantly to make it clear how invention comes about specifically. One is impressed by the vague and even mystical conceptions involved, and finds it difficult to acknowledge that the book is at all scientific.

The author discusses preparation for the invention, sudden intuition, and verification. With this as a background he develops his theory of "integral knowledge" which gives the proper relationship of all the factors necessary to explain invention. He calls it "psychical invention," and holds that it deals with "idea," "feeling," and "action." There is no scientific experiment to support his claims of logic. He vaguely pictures the "automatic unconscious" as a "reservoir which preserves syntheses previously constructed. But it is also a kind of regulator which causes stored-up elements to appear exactly in the order in which they were recorded." There is not a hint as to how these things are done—only names are given. No hypotheses susceptible of scientific test are offered. (Since the automatic unconscious thus fails to explain invention the author uses such additional futile and mystic terms as "dynamic unconscious," and "aesthetic consciousness." No scientific experiments are given.

It is regrettable that the attempted verifications of the author's views stated in this book have been so completely based on 'introspection' and upon opinions of various schools of philosophy. There are several excellent accounts of invention, and had the author stayed more closely to these data much of the material would have been more valuable to the psychologist, and to scientists generally. There is a bibliography of seven pages, consisting mainly of speculative literature.

Peabody College J. J. RAY

The Poor White Problem in South Africa: Report of the Carnegie Commission. Part II. The Poor White. By R. W. WILCOCKS. Stellenbosch, Pro Ecclesia-Drukkery, 1932. Pp. xxxiii, 206.

The entire report of the study of the poor white problem in South Africa made by the Carnegie Commission is published in six volumes. The results of the psychological investigation are given in this volume. The purpose of this division of the investigation was to determine the part psychological factors have played in the development of rural impoverishment.

There are certain weaknesses in the methods used in the study. Some of the data were obtained from personal interviews and in some instances the information secured was written down from memory after the interview. Some of the data concerning the economic status of the parents of the poor white children were secured from questionaries filled out by the children themselves. Since children are inaccurate in making such reports this source of information is questionable.

The investigation shows that environmental circumstances have resulted in the development in the rural population of "certain mental traits which have impeded, and, often, prevented successful adaptation to present-day conditions of economic life in South Africa" (p. 170). There is a tendency for the rural population studied to stay on the farms because of a lack of motivation by stimulating examples of those who have gone away and succeeded. The effect of tradition makes the farmers conservative and prevents the adoption of better methods of farming. They develop a passive, fatalistic attitude towards life because the uncertainty of weather conditions makes success difficult. Isolation causes a lack of knowledge of business methods and makes coöperative activities difficult. The effects of the credit system in all of the groups of poor whites studied prevent the development of habits of thrift. Prejudice against Kaffir work tends to hinder the development of habits of industry. When the lower classes of poor whites sink to an economic status on a level with the natives the color line tends to disappear and the social problem of racial intermixture develops. The people as a group develop an attitude of inferiority as a result of being unable to change the conditions of living. The effect of having the state give the people charitable assistance without requiring any recompense in return has resulted in developing in them an attitude of dependence and a feeling that such aid is theirs by right. This analysis of the attitudes of the poor whites is of interest to psychologists generally because it shows the need of developing more objective techniques which may be applied in studying the effects of cultural patterns on the development of mental traits.

The most important phase of the problem studied is the comparison of the intelligence test scores made by the group of 3281 poor white children with those made by a sampling of 16,574 children of the general European population. The South African Group Test of Intelligence, which the author considers a valid measure of Spearman's *g*, was given to all of the subjects, except 208 of the poor white group to whom the American Army Beta test was given. Sixty-one percentage of the poor white children fall below the average of the European children. The average intelligence quotients for the different age groups were compared by fitting a straight line to them by the method of least squares. This comparison showed a tendency for the intelligence quotient to fall with an increase in the age of the children and the result is interpreted to mean that unfavorable environment tends to lower intelligence test scores. Since the difference in the lower and upper ages tested is only thirty-five months it is doubtful if this statistical treatment of the scores from one testing has greater reliability than the more empirical method of comparing the intelligence test scores of subjects before and after they have been subjected to certain environmental influences. The statistical procedures used in the analysis of the data are, however, accurate and dependable.

Georgia State College for Women EURI BELLE BOLTON

Constitution-Types in Delinquency. By W. A. WILLEMSE. New York, Harcourt, Brace & Co., 1932. Pp. viii, 266; with 32 plates and 19 diagrams.

The doctrine of constitutional-psychological types as originated by Kretschmer and extended by Delbrück and others, was applied by Willemse, after preliminary research with normal youths, to the study of nearly 200 male juvenile offenders in or near Pretoria. The body types, described in detail, illustrated by many reproduced photographs, are, with some overlapping and admixtures, found to fall into three main classes. These are the leptosomes (thin, weak), the athletics (strong, robust), and the pyknics (short, thick, high domed). Groups differ also in type of skin, hairage, and body chemistry. These physical characteristics find correspondence in mental traits, less of the cognitive than of the "emotional-volitional." The leptosomes exhibit the asthenic, autistic, self-insufficient, submissive, childlike, or feministic attitude. When very weak they remain infantile; at best, unless there are mixed athletic traits, they reach only the early pubertial development of personality. The athletics are markedly sthenic, unemotional, bold, aggressive, logical, scheming. Their "stage of development" is *termed* that of "the normal adult male." These two types, psychiatrically considered, vary from the schizothyme (normal) to the schizoids and schizophrenes. Pyknics (fat, inclined to baldness) are of the unreflective, impulsive, socially frank, happy-go-lucky type. They afford the cyclothymes, cycloids, and manic-depressives. This delinquent material is deemed especially convincing, though the theory applies as well to the average or superior groups—scientists, artists, rulers. The individual delinquents are not described on the basis of cognitive intelligence levels, though the group average proved lower in verbal thinking and higher in mechanical ingenuity than that of trade school boys of similar age. "Intelligence," says Willemse, "is not the main cause of delinquency." Environmental influences likewise are discounted.

The battery of tests and methods of procedure used by Willemse include the following: Interpretative observations ("intuitive descriptions"); study of case records; study of letters written by subjects; experiments directly involving temperament (ergogram from each boy, with Mosso ergograph, the blot picture experiment of Rohrschach, the Bourbon concentration test, a spontaneous drawing, and friendly and confidential interviews checked by records); and photographs and anthropological investigations.

The text is a valuable addition to recent studies of endocrine-vegetative-somatic correlations with psychic tendencies. Perhaps too little weight is given to intelligence levels and the compensatory behaviors of those persons lying just beyond the bare normal. The reader's attention also fixates the frequent environment of the broken home and other highly provocative conditions, as well as the apparent absence of old-fashioned parental preachments or new-fashioned "conditionings" that might have salted into youthful nervous systems the simple homily that "it pays to do right." One wonders about the "blends" of types. Would a pyknic with scholarly, retiring tendencies, or an athletic enjoying the tender emotion, be able to place himself on the *Triangle of Temperament?*

Woman's College of Alabama SARAH M. RITTER

Mental Hygiene of the School Child. By PERCIVAL M. SYMONDS. New York, Macmillan Co., 1934. Pp. xi, 321.

The author states that this book is "designed primarily for teachers" with its main emphasis on the "positive, preventive aspects of mental hygiene." The behavior with which Dr. Symonds is concerned is not the extreme behavior of the psychopath, but rather the mild idiosyncrasies and peculiarities which may forecast later maladjustments and unhappiness for the individual.

The book falls into sections (not indicated in the table of contents), each discussing the subject from a different point of view. After two introductory chapters, come three chapters of heavy, uninteresting presentation of learning, drives, behavior mechanisms and even a paragraph on *Gestalt* psychology. This section would seem to be unnecessary for the psychologist reading the volume, and of little value to the teacher who may be discouraged by these pages and fail to reach the later sections which are written in the simple language of every day with practically no reference to the psychological terms elaborated here.

Chapters 6 through 12 form the practical section of greatest interest to teachers.

Later chapters discuss organized work in mental hygiene in the schools, the training of specialists in that field, and the mental hygiene of the teacher herself. The final chapter presents a group of case-studies on pupils in junior and senior high schools.

The pages which are of primary interest to teachers present a sympathetic point of view toward the difficulties and problems of children with a constant endeavor to trace the reasons behind the undesirable behavior. The child's actions are to be thought of as symptomatic of underlying ideas and attitudes, and treatment should be applied to the basic difficulties, not to the behavior itself. Emphasis throughout the book is on prevention rather than cure of extreme cases. The author presents a number of bits of behavior commonly met with in the class-room, such as "preferring to sit on the side-lines," "being constantly restless in study and easily distracted," "being exceptionally neat," "exhibiting anxiety over mistakes," and so on, with many concrete suggestions as to what may be the fundamental cause of the behavior. One of the best sections in the chapter on positive habits of mental hygiene discusses learning to admit "mistakes, errors and faults." In this chapter also the author strikes a happy, common-sense middle ground in expecting a child "to accept some things on authority without demanding the reasons" while he is also able "to reject habitual standards in favor of more intelligent standards."

Throughout the book, the author makes a definite effort to present the material from a constructive, preventive point of view. While many sections demonstrate that this is a more difficult undertaking than is the presentation of autocratic rules of procedure and that it is easier to present examples of bad behavior than it is to describe cases of completely satisfactory adjustment, nevertheless the reader will be left with a great many useful suggestions, considerable information about the set-up of remedial work in progressive schools and an understanding of the mental-hygiene point of view which should make him better able to cope with the difficulties which the average child presents.

University of Minnesota JOSEPHINE C. FOSTER

Social Development in Young Children. By SUSAN ISAACS. New York, Harcourt, Brace & Co., 1933. Pp. 480.

The author's method of studying the development of social relationships in young children is largely observational. She keeps records of the various manifestations of social behavior in the children in her school, and these are supplemented by letters from mothers and nurses, and quotations from other sources. One-half of the book is devoted to a description of the factual material which she has at hand. She divides her problem into two parts: elementary social relationships, including primary egocentric attitudes; and hostility and aggression. There is also a subsection devoted to friendliness and coöperation. The second part of her study she calls the deeper sources of love and hate. In this section, her chief interests are sexuality, guilt, and shame. Through these she attempts to develop the "deeper sources." Though Dr. Isaacs denies that she is entirely in agreement with the Freudian psychoanalytical school, the second part of this volume is largely psychoanalytical in nature. Much of the factual material in this book deals with erotism, which is to be expected inasmuch as children have not yet developed attitudes which would prevent their discussing these matters; but one somehow gets the impression that Dr. Isaacs selects her material to bring out this point. The manner in which she presents her records make them offer rather convincing evidence of the significance of some of the Freudian ideas, particularly the attitude of one child toward another which is developed through love, and the significance of the transference as far as the teacher is concerned.

Much of this book deals with the author's analysis of the application of Freudian principles to pedagogy, and from this viewpoint it is extremely interesting. When one considers the vast amount of observation that this study must have entailed, and the author's obvious leanings, the end product is much less subject to criticism than might be expected. Dr. Isaacs offers better evidence for Freudian genetic psychology than has been offered in the past. Her presentation and interpretation of her study are very much like those in other psychoanalytical literature, but she brings out a new viewpoint which is deserving of serious study.

Eloise Hospital
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LOWELL S. SELLING

Das Beschreiben: Experimentelle Untersuchung des Beschreibens von Gegenständen. By FELIX VIERGUTZ. *Neue Psychologische Studien,* Vol. 10, No. 11. Edited by Felix Krueger and K. von Dürckheim-Montmartin. Munich, C. H. Beck, 1933. Pp. 92.

The main body of material interpreted in the present study was derived as follows: Subject *A* describes to Subject *B* an object hidden from *B*—a tumbler, a porcelain cup, a napkin ring, and a shapeless cast of plastic material; both subjects now write an account of their experiences, based in part on detailed questions; finally *B* is shown the object and he again gives his experiences, particularly with reference to the difference between the original impression based on *A*'s description and his present perception. On the basis of the three different statements, an analysis of the process of describing is undertaken. The aim is to "deal with the process of describing, with description as an activity, in order to establish the most common characteristics of the activity and to comprehend it in the light of its conditions."

In the preliminary descriptions of the responses many concepts of rhetoric find a place: the statements are unified and they cohere; one is written from an esthetic point of view and another personifies; one is fluent, rich in choice of words, and another lacks these qualities. In the reports of the hearer factors more psychological in nature, such as the attitude of attending and certain factors growing out of earlier habit systems are discovered.

The central problem of the study, which deals with description as a psychological process, is discussed in the terminology of the psychological concepts of Krueger and Dürckheim. The subject is confronted by a problem; attitudes of expectancy appear; the sense of a goal which is to be reached by prescribed requirements and which gives direction to subsequent activity becomes apparent; in the experiences of describing, words and structural elements of language arise to mediate between the object and the hearer in the light of the task; in the end the subject looks back to the experience of having done a piece of work and also of having finished a task. The enquiry has turned from a study of descriptions to the analysis of the process of describing.

Any evaluation of the study must be in the light of the system of psychology on which it is based or of the system itself as a scientific account of human life.

Florida State College for Women P. F. FINNER

Applied Psychology. By RICHARD W. HUSBAND. New York, Harper & Bros., 1934. Pp. 644.

The material in this textbook differs from that in others of like title in several ways. The author states that the choice was determined by the interests of students, by "what the experts have emphasized most, as evidenced by the mass of literature dealing with the various topics" and by his desire to "make the book as scientific as possible."

The principal topics dealt with are: (1) vocational guidance, including the use of intelligence and personality tests and the nature and measurement of interests; (2) personnel management, including employment devices and training systems, and some material of administrative rather than psychological nature; (3) industrial fatigue, scientific management, and labor relations in industry; (4) advertising (four chapters) and salesmanship and store management (two chapters); (5) the following miscellaneous problems: psychology and medicine; psychology in law; psychology and athletics; personal physical and mental efficiency.

The text is characterized by breadth of interest and a seemingly broad synthesis of research on these topics. The reviewer agrees with the general plan; it leads however to two difficulties, which the author does not entirely overcome. One is the inclusion of considerable material which has not been subjected to research, and the other is a needless amount of repetition. The treatment of individual differences is less thorough than is justified by the "emphasis of the experts;" there is practically nothing said about sex, age and race differences. The student unacquainted with intelligence tests would be little helped by the three pages on the measurement of intelligence. Correlations are given (pp. 28, 33) before there is any discussion of their meaning (p. 234). The index does not list either "ability," "correlation," or "critical score," although all are discussed in the text. In general, methods have been subordinated to results, and the elementary treatment of statistical methods under the chapter heading "Personnel Research" does not satisfy requirements. The discussion of employment tests does not seem sufficiently critical, and the reviewer wonders why the Minnesota research on mechanical abilities is not cited.

Texts in applied psychology have not had the interest for the student that they should have, and it is to be said for this one that it gauges the student interest fairly accurately. It can be recommended for the course in business psychology for which it is probably designed. References, tables, and illustrations are adequate.

University of Texas W. H. BRENTLINGER

Psychology: Its Methods and Principles. By F. A. C. PERRIN. New York, Henry Holt & Co., 1932. Pp. xii, 336.

This is a revision of the book of the same title which appeared in 1926, by the present author and D. B. Klein. The present edition seeks to present psychology from a more eclectic point of view than did the first; hence there is a new chapter on "Sensory and Perceptual Processes" which incorporates a considerable number of data from the older experimental psychology and some from more recent experiments, but the author has little, if anything, to add in the way of interpretation in this field. Visual perceptions are still projections of "our visual experiences out into space" (p. 116). There is little attempt, in line with several recent experiments, to relate perception to learning and to general integrative processes so well considered in other parts of the book. It is probably not denying the value of an eclectic point of view to suggest that data derived from different sources be somewhat integrated when possibilities for such integration are so plentiful.

The discussion of learning has been considerably improved, particularly in the partial omission of long discussions of different theories, studied mainly *as theories,* largely independent of the demands for hypotheses to account for processes not directly observable and of the evaluation of these hypotheses in relation to the consistency of the entire picture they make of verifiable data. Still further reductions might have been made with profit, if the readers were given hints as to possible explanations at points where direct knowledge is inadequate and allowed more opportunity to organize their own interpretations tentatively as they proceed. One might wish that the book were more inclined to stress experimental approaches at the expense of excessive theorizing as a procedure for the student beginning in psychology.

The other data presented in the book are mainly unchanged, even though chapter headings are somewhat differently worded and some new groupings are made; and references are brought up to date. The printers and binders have done their part in making this a very attractive book.

J. P.

Human Sterilization: A History of the Sexual Sterilization Movement. By J. H. LANDMAN. New York, Macmillan Co., 1932. Pp. xviii, 341.

As the author states, this volume is not designed as propaganda either for or against sterilization. It is rather a scientific and unprejudiced treatment of the biological, legal, and social aspects of the problem. The general topic of eugenics and social legislation is discussed in the several chapters of Part I. The author takes a sane, conservative position in evaluating the eugenics movement in its historical perspective. The need for eugenics is shown by the citation of statistics on the increase of the mentally incompetent in the United States during the past few decades. This is followed by a comparison of the legislation of the several states relative to human sterilization. Part II deals with the various judicial decisions which have been rendered in the attempt to apply such legislation as exists. Part III is devoted to a consideration of the biologic and eugenic bases for compulsory sterilization. This section includes an analysis of dysgenic types in our population—the potential parents of socially inadequate children. An attempt is made, also, to evaluate the evidence for hereditary taint in such strains. The present status of the problem of the inheritance of mental traits is briefly indicated. Part IV is concerned with the surgical aspects of human sterilization. In the final section, the author seeks to formulate a practical social policy for dealing with social inadequacy in which sterilization is assigned a limited rôle. The volume is well written and should serve as a valuable handbook to those interested in this field of social endeavor.

Columbia University C. J. WARDEN

Le devoir. By RENÉ LE SENNE. Paris, Félix Alcan, 1930. Pp. 604.

A systematic, closely reasoned discussion of the foundations of ethics, in relation to other disciplines. Primarily philosophical, but of interest to the theoretical psychologist because, although there is little explicit reference to psychological literature, many live problems in our field are treated. The arguments and subject matter by chapters are: (1) that maladaptation is the leading characteristic of mental life, that interference with the satisfaction of needs is the basis of every mental function; (2) that the methods of resolving logical difficulties in physical science are by hypotheses and research to the elimination of conflict between emotions or more explicit motives by mental hygiene or moral ideals, the purposive standpoint in psychology being stressed; (3) is an elaborate discussion of volition, especially of moral decision, including, incidentally, a critique of James; (4) includes comment on the opposition between science and epistemology and on the function of ethical systems in the moral life, and offers a critique of hedonism; (5) considers moral obligation and the problem of freedom; (6) the interrelations and conflicts among metaphysics, science, religion, and art; and (7) the problem of evil. This chapter completes the main argument, which suggests a world-view roughly similar to that of Royce (without his mathematical analogies). The final chapter, *L'éducation morale,* is very suggestive. One special point may be mentioned, the writer's belief that aims and methods in moral education must take account of individual differences.

McGill University C. E. KELLOGG

The Art of Feeling. By HORACE G. WYATT. Boston & New York, Houghton Mifflin Co., 1932. Pp. x, 293.

Mr. Wyatt has chosen an apt title for this treatise in practical philosophy. He proposes to "search the relation of feelings and emotion to other happenings in the human mind, for it is in governing their interplay that our art consists." His thesis is that "emotion" or "feeling," conceived very broadly, is or reflects the fundamental basis of human motivation. The advertisement on the wrapper and the subtitle designate the subject as psychology, but one must not take this designation too seriously, even though some of the chapter headings might lead him to expect a recital of facts. Mr. Wyatt is conversant with the material in the field but, avowedly, facts for him are secondary to the uses to which they may be put and their consequent practical values. He makes this clear in his introduction. "It follows that for the ordinary human the findings of professional and experimental psychology are helpful and acceptable so far as they concern and elucidate his ordinary humanity, and where they disregard or obscure it are suspect or useless." In accordance with this he takes a definite stand. He rejects what he calls "the cult of the abnormal and the animal in human nature" and he accepts or rejects factual material on the basis of its utility in expounding his thesis.

It would be absurd to try to evaluate the book by way of a minute analysis and it is impossible to dismiss it with a word. The reviewer found the book in the main interesting reading but singularly uninforming. "Emotion" or "feelings" covers almost everything that can be taken as in any way motivating behavior. This gives the author the chance to offer a solution for every problem from sex to war, and to outline experience from imagination and thought to character and hypnosis.

Such a book has its merits and no doubt its uses. It contains frequent practical observation of human nature, and many a challenging statement that might well serve as the text of a sermon.

Hobart College FORREST LEE DIMMICK

A Psychological Approach to Literary Criticism. By NORMAN R. F. MAIER and H. WILLARD RENINGER. New York and London, D. Appleton & Co., 1933. Pp. xii, 154.

This book attempts an objective approach to literary criticism. It begins by giving quotations which show that many book reviews give little or nothing except an expression of personal judgment as determined by emotional attitudes.

Criticism should concern itself with general laws of written communication. These should be based on a psychology which recognizes the existence of mental patterns such as are most emphasized by Gestaltists. Criticism should answer three questions: (1) What is the artist's configuration? (2) How valuable is it? (3) How effectively has the artist communicated it?

An exposition of the nature of productive thinking follows, illustrated by experiments in problem solving. The same truths, it is asserted, are involved in literary expression. The subjective and objective problems of literary criticism in various forms of literature, humor, tragedy, etc., are discussed and illustrated with emphasis upon the importance of the directions of thought-patterns in securing maximum results.

As a psychological and common sense *approach* to critical reviews the book has considerable value, but the suggestions for an objective approach to literary criticism are not objective in the scientific meaning of the word, but only in the sense that accepted general truths of psychology are to be used as standards of judgment instead of the individual ideas and opinions of reviewers.

Leominster, Mass. E. A. KIRKPATRICK

Monographien über die seelische Entwickelung des Kindes. II Erinnerung, Aussage und Lüge in der frühen Kindheit. By CLARA and WILLIAM STERN. Fourth ed. Leipzig, Johann Ambrosius Barth, 1931. Pp. x, 256.

Since its first appearance in 1908, this monograph has twice been reprinted without alteration. In the present edition much of the original material has been revised in the light of more recent findings by other investigators and one entirely new section, *Personalistik der Erinnerung,* has been added. In this section Stern draws a sharp distinction between memory (*Erinnerung*) and knowledge (*Kenntnis*). The former is specific with reference to the individual's own past; the latter, although a function of experience, is non-specific and not consciously related to the personal history of the individual. (I "know" that a chair is to sit on but I do not "remember" how I gained this knowledge.) Memory, on the other hand, has both a temporal and a spatial reference which is more or less exact. Through his increasing series of specific memories the child gradually acquires the idea of continuity of time and space. Within this ever-changing, though continuous series of events he remains the center about which all else revolves. Thus, in contrast to the awareness of a continually changing world there gradually emerges the idea of a persisting self. The development of memory is therefore inextricably bound up with the development of the whole personality.

Section II which is based almost wholly upon diary records of Stern's oldest daughter is practically identical with the earlier editions. The remainder of the book has been largely rewritten. Throughout, the new version shows a distinct shift from the description of developmental phenomena as such toward a greater stress upon the utilization of this descriptive material for the understanding of fundamental problems in psychological theory.

University of Minnesota FLORENCE L. GOODENOUGH

The Keys of Power. By J. ABBOTT. New York, E. P. Dutton & Co., 1932. Pp. vi, 560; index, glossary, appendices, and numerous diagrams.

"The Keys of Power," portrayed with photographic clearness and encyclopedic fullness, from the author's personal observations, are the taboos and magic rites by which the natives of India seek to control the benefit or bane "inherent in" such as the following: the evil eye, gifts, the ground, water, fire, metal, salt, stones, days of the week, months of the year, colors, numbers, trees, weather, animals, man, woman, shadow, spirit. These and other factors of native concern are treated severally in the 26 chapters of the text. Shades of differences between Hindu and Mohammedan custom are pointed out. For the one, for instance, Monday is a bad day to shave, comb the hair, or clean cotton. For the other, it is a good day to be born, to travel, to begin new work, to write a charm. Tuesday is a calamitous day for a Hindu to start on a journey or to hold a consultation. "A dumb man is said to consider every day Tuesday." The ritual of agriculture, though now rapidly changing, is held by the native to be especially important. The threshing floor is a "hallowed spot," for there the grain is magically increased in measure.

The research student interested in customs or in relations of belief to behavior will find the text a mine of ready material. The average reader would like a more extended glossary, but finds in the pages the interest of popularly written science. The author fulfills his announced purpose of depicting, before they yield to "the disrupting force of modernism," this mass of custom and ritual preserved from the "'dim centuries' . . . beneath the animism of Hinduism and the monotheism of Islam." The work is descriptive rather than interpretative.

Woman's College of Alabama SARAH M. RITTER

The Mind in Conflict: A Simple Explanation of How Modern Psychology Sets Out to Solve Nervous Difficulties and Problems. By RICHARD AMARAL HOWDEN. New York, Humphrey Milford, Oxford University Press, 1931. Pp. xi, 83.

This book is a short explanation (written largely for the layman) of the psychoanalytic method of dealing with mental disorders. The author calls his book one in 'medical psychology'—defining medical psychology as that branch of psychology which deals with the human mind in sickness, or more scientifically with the "pathological states of the human mind." There is a section on the mind with an explanation of the conscious, preconscious, and unconscious. There is a chapter on the importance of the unconscious in which an attempt is made to define or explain many of the commonly used psychoanalytic terms, as conflict, adjustment, neurosis, hysteria, repression, rationalization, etc. Causes of mental disorders are discussed in relation to conflicts in the 'unconscious mind' as a causal factor. The treatment of mental disorders by psychoanalytic method is outlined with a discussion of five different procedures: free association, dream analysis, hypnosis, getting patient to talk about himself, and reëducation. A chapter on sex and modern medical psychology attempts to counteract the often-made criticism, that Freudian psychology overemphasizes sex. The author holds that 'sex' is frequently used broadly to mean 'instinctive urges' in general. The importance of instruction and training of the young in sex matters is stressed but there is little practical advice to be gathered from the book. Under the prevention of mental disease, two things are stressed—early and immediate treatment of cases, and psychological development of the child.

The book is probably too elementary to be recommended to the general medical man, and certainly too elementary for those already dealing with the treatment of mental disorders. The chief criticism that we might have of this book is that it gives only the psychogenic story of mental disease. While the author suggests in his introductory chapter that mental disorders should be viewed as diseases in much the same light as are scarlet fever and whooping cough, his subsequent writing does not seem to follow his own suggestion—at least not in the parlance of the man who usually deals with scarlet fever and whooping cough. Howden considers the psychogenic causal factors in mental disease (mainly conflict) as primary; physical factors (glands, poisons, infections, etc.) as secondary or rarely as primary causes. Progress in the study of mental disorders seems to be reversing these primary and secondary categories, and it would seem that a less one-sided presentation of the causes of mental ills is to be desired. The lay public has already had too great a proportion of literature on mental disorder addressed to it stressing psychogenic or psychoanalytic origins of disturbances. What it needs for more practical dealing with the many cases at hand is a sound presentation of the physiological causal factors.

George Washington University THELMA HUNT

A Survey of the Science of Psychology. By J. R. KANTOR. Bloomington, Ind., Principia Press, 1933. Pp. xvii, 564.

This volume is a thoroughly systematized introduction to organismic or interactional psychology. It is not, as some might suppose, a revision of the author's *Principles of Psychology.* The chief aim has been to "lay before the college student or general reader the materials of psychology not as firmly settled facts and principles but as problems to be faced and worked over." According to the author there are three distinct viewpoints in psychology, viz., the mentalistic, the behavioristic, and the organismic. Mentalistic psychology is regarded as having its roots in intellectual tradition rather than in scientific observation. Behaviorism gives undue emphasis to biological processes, appearing to reject one aspect of psychological dualism in favor of the other. Organismic psychology, by investigating the interactions of organisms and their environment, avoids dualism and at the same time escapes the behavioristic predicament of having to reject introspection and its fruits. "For the organismic psychologist the term introspection does not refer to a unique kind of scientific world, nor even a special method of knowledge. Rather it merely means observing one's own interactions with things instead of some other person's." This viewpoint enables the author to be eclectic in selection of subject matter without yielding to systematic inconsistency.

The twenty-seven chapters cover such topics as what the psychologist studies, analysis of psychological interactions, the reactional biography from prenatal to adult societal life, personality, attending, perceiving, implicit interactions, knowledge and other intellectual interactions, feeling, remembering, forgetting, reminiscing, learning, thinking, linguistic interactions, voluntary interactions, imagination interactions, organisms as structure-function mechanisms, the brain and nervous system, the evolution of organisms, man as a biological organism, and man as a cultural organism.

The reviewer believes that there is no other introduction to psychology which covers such a wide range of topics and overlaps so extensively the fields of biology, sociology, and anthropology. It should give the introductory student who will read it a valuable appreciation of the interdependence of psychology and the biological and social sciences. The fifty pages of references at the end of the book are arranged according to topics. More than one hundred figures illustrate the text.

University of Pittsburgh NORMAN L. MUNN

Psychoanalysis and Medicine: A Study of the Wish to Fall Ill. By KARIN STEPHEN. New York, Macmillan Co., 1933. Pp. 238.

This book consists of eight lectures delivered at Cambridge, England, mainly to medical students. It is put out as essentially medical and clinical.

Working from the Freudian hypothesis, the author gives a minute and occasionally brilliant analysis of several cases she has studied to illustrate the importance of the general practitioner, recognizing that some of Freud's cases must be handled by a psychologist. She emphasizes the tortuous meanderings a mind may pursue to hide its anxiety source, and the peculiar and sometimes Janus-like aspects a psychogenic illness may assume.

For her the 'unconscious' is that part of the personality which cannot be recalled by the individual himself, but only through psychoanalysis, chiefly by the free-association method. This mechanism of the unconscious, she holds, takes origin in frustration of desire and activity, resulting in defense by repression and symptom-formation. This whole defense mechanism is essentially what she means by the wish to fall ill. She differs from certain psychoanalysts in holding that "the unconscious has a complete disregard for consistency, believes in the magical power of thoughts and wishes, is taken up entirely with wishing and not concerned about knowing, confuses fantasy with fact, thinking with doing, and makes the same reactions to all situations which resemble one another, emotionally failing to discriminate one from the other" (p. 237 f.). Not only is there no attempt made to use the scientific method, but the author dismisses this method as inapplicable in this field, holding that the 'unconscious' will never prove amenable to the quantitative treatment.

While the material of this book has already been covered in several other works, the last three-fourths of the book, studying the basis of the neurosis as it is formed in very early childhood, follows the latest trend in psychoanalysis and is both lucid and complete.

Peabody College ALDERSON FRY

Functional Affinities of Man, Monkeys, and Apes. By S. ZUCKERMAN. New York, Harcourt, Brace & Co., 1933. Pp. xviii, 203.

This volume is unique in that it represents the first thorough-going attempt to classify animals upon the basis of physiological rather than morphological data. Since it is chiefly classificatory in nature and treats of data outside of the customary sphere of psychology, most psychologists will probably not give this book the attention claimed by the author's *Social Life of Monkeys and Apes*. Nevertheless, the chapters dealing with receptor processes, behavior patterns, intelligence, and the evolution of Primate behavior will be of interest to all comparative psychologists. These chapters indicate not only the significance for classificatory purposes of comparative behavior research, but the paucity of material at present available. Zuckerman points out that available data do not support the belief that apes are more intelligent than monkeys, but he adds that it "cannot necessarily yet be regarded as incorrect. It is possible that the usual methods of experimental psychologists are incapable of demonstrating the significance in overt behavior of differences which are generally believed to exist in the complexity of the sensori-motor equipment of different sub-human primates. Other kinds of investigation might succeed where these methods have failed." In particular, what is clearly needed, he thinks, is an adequately controlled comparative study of the learning capacities of different animals "in a series of carefully selected situations."

It may be that the above needs will in some measure be met by an extension of Gellermann's important researches on double alternation and form discrimination in Primates, researches not available to the author. Similarly Klüver's ingenious "pulling-in" technique might, if applied to representative Primates, yield behavior data of taxonomic significance.

The organization of the large amount of material represented in the book is excellent. There are numerous tables concisely envisaging all that is known concerning such things as brain and body-weight ratios, haematology, reproduction, hybridization, reflexes, and sexual characteristics. Twenty-four plates illustrate the chief Primates. The book is well indexed and it contains a bibliography of almost 300 titles. Apart from any interest that it may hold for the general psychological reader the book will be a valuable tool for those whose research concerns the Primates.

University of Pittsburgh NORMAN L. MUNN

Social Psychology. By ABRAHAM MYERSON. New York, Prentice-Hall, 1934. Pp. xv, 640.

This work is divided into four quite unequal parts: general social psychology, occupying over four hundred pages; visceral social psychology, about one hundred pages; social psychology of the family; and misdoings. These last two parts cover less than a hundred pages. The first part treats of environmental and hereditary factors, gregariousness, the nervous system, instincts and emotions, intelligence, learning processes, group and individual interactions, and language. The second deals with the gastro-intestinal tract, the visceral functions and the sexual functions.

Dr. Myerson very effectively develops two theses: (1) that the visceral organic structure of man is basic to the understanding of psychology, and (2) man is developed in a milieu (physical and social) that fosters, modifies, or destroys his capacities. In the discussions mental as well as physical traits of man, and purposes as well as physiological functionings, are considered. The author continually notes how these originate social customs, and on the other hand, how they are continually modified by such customs.

As might be expected from the author's experience as a psychiatrist the book discusses general individual psychology in considerable detail. There is no discussion of social institutions, and very little on the development of social customs and social organization. There is little that constitutes a new contribution to the special field of social psychology, although the author shows familiarity with its literature as related to general psychology.

Considerable space is used in presenting the older views of specialized functioning of the nervous system, although the author is aware of recent researches throwing doubt upon the truth or significance of such diagrammatic representations of neuro-muscular processes. He regards the fore brain as the chief center of all social influences which modify individual behavior. Such simplification might lead to misconceptions unless certain precautions are taken. Dr. Myerson's general psychological attitude is eclectic, and he is disposed to use words in any convenient way rather than to adhere to exact definitions of meanings. In the present state of the science such a practice may sometimes be justifiable but it is not in the case of the word "certainty" in Chapters XI and XXI where it is made to include the impulse to gain safety and satisfaction.

The language used is generaly non-technical and popular, but statements are usually supported by reference to special scientific books and articles. This tends to make the book acceptable to both the scientists and the general reader, or, possibly in some cases, objectionable to certain individuals in both classes. One example of emphasizing the popular rather than the scientific is in his reference to the ages of the men who negotiated the Treaty of Versailles and his statement that "probably the most important social step that could be taken would be a periodic examination of all persons having social responsibility as to the state of their heart, arteries and blood pressure." The present writer doubts if Dr. Myerson would, if he could, make arteriosclerosis a test of eligibility to hold office.

The authorities and suggestions for additional reading are well selected, and there is a separate author and subject index. The printing is well done but publishers who use such heavy paper should supply a bookrest.

Leominster, Mass. E. A. KIRKPATRICK

Lehrbuch der Sprachheilkunde (Logopädie) für Ärzte und Studierende. By EMIL FRÖSCHELS. 3d. rev. ed. Leipzig and Vienna, Franz Deuticke, 1931. Pp. xxiv, 715; 113 fig., 3 pl.

In the present edition, the third of a text that appeared originally in 1912, Fröschels attempts an encyclopedic treatment of the entire field of speech deficiency. It presents the general physiology of the speech organs and breathing (55 pages), the experimental methods for the study of speech (16 pages), the nature of normal speech and its development (26 pages), and the broad field of speech disorders

(567 pages). The bibliography has about 1000 titles. A register of names but no index is provided.

The view of Fröschels both in the general theory of speech disorders and on the methods of correction agree in the main with those of Fletcher. The book is noteworthy for the detailed study of the disordered individual that it advocates, for the comprehensive examination of rival theories and practices, and particularly for the synthetic view of a large body of literature that it finally offers. The method of dealing with problems which constitutes the outstanding merit of the treatise, also leads to certain weaknesses; too much is attempted on certain topics. The section on the measurement of intelligence requires a separate volume. The discussion of normal speech development borrows largely from Bühler; it remains highly superficial and unrelated to the main topics of the book. The experimental methods in the study of hearing, rhythm, and even topics in vocal responses do not include some of the most effective modern techniques.

Fröschels comments kindly, and correctly, that American writers are neglecting almost entirely the German literature on speech deficiency. The provincialism of scientific work in the field is, however, more truly represented by noting that the extensive bibliography and register of names given in the volume omits probably ninety percent of our most significant studies on the topics treated. No student of speech disorders can in the future neglect the critical thought, the extensive organization of experimental and theoretical literature, and the practical recommendations that this volume offers.

Florida State College for Women P. F. FINNER

Speech Disorders. By SARAH M. STINCHFIELD. New York, Harcourt, Brace & Co., 1933. Pp. xii, 341.

The first part of this book consists mainly of a systematic presentation of speech abnormalities according to a classification of disorders of speech compiled by the Nomenclature Committee of the American Society for the Study of Disorders of Speech of which Dr. Stinchfield is a member. Speech defects are divided into seven large classes: dysarthria, dyslalia, dyslogia, dysphasia, dysphemia, dysphonia, and dysrhythmia. Each class in turn is divided into further descriptive groups and subgroups. Under this classification Dr. Stinchfield discusses briefly the conditions and causes, speech symptoms, and prognosis and treatment for each particular type of speech defect. On the whole this part of the book is admirably done. There are certain statements to which one could readily take exception. One such is that it is unsafe to assume that there is a dominance exercised by one side of the brain over that of the other side. In this connection it is pertinent to point out the recent work of Weisenburg (*Arch. Neurol. & Psychiat.*, 31, 1934, 1-33). He found aphasia or a history of speech difficulty at the time of the attack in 65 of 67 right handed patients with demonstrated lesions of the left hemisphere or neurologic signs of such lesions and no aphasia or history of it in 22 of 23 right handed patients with demonstrated lesions of the right hemisphere or neurologic signs of such lesions. This shows that there is a crucial hemisphere for speech in at least 95% of the cases.

Part two deals with statistical studies of the speech of 3000 college women and of public school children. The studies used a speech data sheet, the Thurstone Personality Schedule, handedness inventories, oral and silent reading tests, speech tests, and case histories. From these studies a wealth of material is made available for the clinical and the research worker in the field of speech pathology.

The book has a supplement listing the better known speech tests and their uses. There is a good index and a few illustrations and graphs scattered throughout the book. Dr. Stinchfield does not attempt to offer any new theories in regard to the nature of any type of speech defect. Rather it appears that she has undertaken a systematic and careful review of the whole field of speech pathology. The book can be recommended highly for use particularly at the graduate college level.

University of Iowa LEE EDWARD TRAVIS

Psychoanalysis of the Neuroses. By HELENE DEUTSCH. Trans. by W. D. Robson-Scott. London, Leonard & Virginia Woolf, 1932. Pp. 237.

Eleven lectures are given here to supply some sort of substitute for clinical demonstration, and the author assumes that the reader has a basic knowledge of the subject—an assumption contradicted by the simplicity of much of the text. The lectures are concerned with the part of the actual conflict in the formation of neurosis; there are, besides the introductory lecture four lectures on hysteria, three on phobia, two on obsessional neurosis, and one on melancholia. Taking clinical cases which she has treated, Dr. Deutsch states the problem, how she arrived at a diagnosis, and the curative steps taken, if any. She recognizes, as others often fail to do, that many cases (involving the patients who are most unwilling to be cured) may prove incurable. "Psychoanalysis," she says, "emphasizes three etiological factors in tracing the origin of neurotic diseases: (1) *fixation* of the libido; (2) *regression;* and (3) the so-called current or *'actual' cause,* which through some frustration may become a decisive factor in the genesis of the illness." (p. 12).

Unfortunately she enters a field already mired in a morass of obscure definitions with new ones, and much of the text is poor reading because of the esoteric terminology; but she is often, when unhampered, lucid and writes in fine style. For example, "The neurotic symptoms are indeed nothing else than unsuccessful attempts to solve the inner conflict" (p. 15).

Peabody College ALDERSON FRY

Ethics. By JOHN DEWEY and JAMES H. TUFTS. 2nd ed. New York, Henry Holt & Co., 1932. Pp. xiii, 517.

The present volume, which constitutes an exposition of ethics as an experimental rather than a normative science, retains the basic plan of the first edition. However, extensive changes as regards both material and method of presentation have been introduced into the later edition.

The work is divided into three parts. Part I, concerned with the beginnings and growth of morality, distinguishes three levels of behavior: (1) the biological level; (2) the level of customary morality; and (3) the stage in which morality becomes reflective and self-conscious. These three stages are amply illustrated from history, the revised edition having a new chapter dealing with the Roman contribution to morals.

Part II deals with the theoretical aspect of the moral problem. According to the authors, ethicists have been more or less "hypnotized" by the traditional notion that there exists some ultimate end, commonly referred to as the *summum bonum.* The acceptance of this ethical dogma is held to be largely responsible for the general lack of constructive thinking in the field of ethics. For this *a priori* type of approach it is proposed to substitute the method of experimental logic. Each moral situation is held to be unique, hence, the authors maintain, it is a mistake to search for some absolute and universally applicable ethical principle. So-called moral laws should be used as instruments for the analysis of specific situations rather than as inflexible rules for determining conduct.

In Part III, which section is almost entirely new, the authors succeed in the difficult task of pointing out the ethical implications of the present social order without becoming partisan. Perhaps the most important contribution found here is the insistence upon the necessity for open-mindedness, and for the willingness to experiment with social issues.

On the whole, the present volume may fairly be said to constitute a valuable addition to the literature in this field. However, it is the opinion of the reviewer that in arguing for a progressive standard the authors fail to distinguish between the form and the content of the 'good,' thus giving the impression that both are entirely relative. If the latter be true, it is difficult to see how progress itself can be determined, since mere change does not constitute progress. Thus we are left with a plurality of changing individualized ends each having its own irreplacable good, but with no criterion by which to determine that they are goods, and, in case of conflict, no means for determining which is the most appropriate good in any given situation.

Vanderbilt University EUGENE G. BUGG

Educational Psychology. By W. A. KELLEY. Bruce Publishing Co., New York, 1933. Pp. xiii, 502.

The general character of this book may be best set forth in the words of the general editor of the series and of the author himself in his preface. The editor: "Christian educators will welcome the appearance of an *Educational Psychology* which meets alike the demands of science and religion—a book including between its covers whatever is best and most progressive in modern texts upon this subject, yet maintains inviolate all the postulates, principles, and high ideals of the Christian Faith. . . . Kelley's originality finds expression in the happy combination effected in his book between the modern scientific elements and the splendidly reasoned system of Scholastic philosophy. . . . How trivial a thing, indeed, is the so-called modern mind compared with the profound wisdom, the keen intelligence, and the bold yet safe, originality of thought which in their day characterized the great Schoolmen!"

The author: "The substantial foundation of this book is the principle that man is a compound of body and soul, endowed with free will responsible for his actions, and destined for an eternal life."

Thus the book is almost homiletic in its outlook and it is this, and this almost alone, which seems to justify calling it a text in educational rather than general psychology.

The effort to combine the neo-Scholastic position with modern experimental work is not to be lightly dismissed. It is too seldom realized how shot through and through the most "hard-boiled" psychology is with metaphysical assumptions. The Scholastic assumptions deserve consideration at this point equally with those of a more recent philosophical past. Indeed, a candid examination of a different set of assumptions may even serve to make us critical of our own uncritically received dogmas. The reviewer regrets that in the present work the virtues of Scholasticism are obscured for most psychologists by the excessively moralistic and hortatory style.

Ohio State University HORACE B. ENGLISH

Mysticism East and West: A Comparative Analysis of the Nature of Mysticism. By RUDOLPH OTTO. Trans. by Bertha L. Bracey and Richenda C. Payne. New York, Macmillan Co., 1932. Pp. xiv, 262.

Rudolph Otto in a penetrating analysis of these two types of mysticism reveals a singular kinship as well as a line of divergence between them. In parallel passages of great beauty from the Latin and Sanscrit, respectively, of Eckhart of medieval Germany (1260-1327) and Sankara of medieval India (early 9th century) he traces a thought pattern that has influenced men, East and West, in their quest not for the 'Absolute' alone but, it seems, for some mental elixir of life, some key to immortality. Each has found this secret to lie in an insight, or 'redeeming knowledge,' that explains the world by explaining it away and banishes death by the discerning of the soul's *oneness* with 'Eternal Essence.' Not empirically and not intuitively is this insight gained, but by the 'ratio pura.' The emotional factor, in each school, is marked by an absence of the erotic; it is the 'rapture of pure thought,' akin to the more familiar losing of oneself in esthetic contemplation. The divergence lies in the objective. The Eastern teaching, according to this Occidental interpreter, points only to quiescence or escape from earthly ills, while the Western is dynamic, aspiring—Gothic—in its lines. Otto is self-styled 'non-mystic,' because of an acceptance of space-time conditions of reality. He writes, however, sympathetically, and as a learned theologian. His own thought patterns are Gothic.

Woman's College of Alabama SARAH M. RITTER

Workbook in Educational Psychology. By A. E. BROWN. New York, Macmillan Co., 1933. Pp. v, 226.

This book is a student's workbook of 45 assignments, of which the ninth, nineteenth, thirty-first, and forty-fifth are reviews. Gates' *Psychology for Students of Education* is the main reference text. In 28 out of the 45 assignments the reference is to this one text alone. In general, the assignment comes first; then come the directions for study, additional notes, and study questions in true correspondence-course style. The study questions are usually (1) yes-no; (2) completion; (3) incomplete outline; and (4) questions for written elaboration. The notes are satisfactory; the questions, thorough-going. There are two criticisms of a rather serious nature. In the first place, it seems unwise to have students study only one textbook when there are such widely divergent views about the simplest psychological phenomena. In only seventeen of the forty-five assignments is any other reference made than to Gates. In the second place, knowledge broken up into forty-five sections with forty items to a section can hardly be really integrated and organized knowledge. This type of workbook emphasizes entirely too much the acquisition of a great number of unrelated items and too little their organization into a few great principles.

University of North Carolina A. M. JORDAN

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